Quantification in British Medicine and Surgery 1750-1830, with special Reference to its Introduction into Therapeutics

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ABSTRACT

ULRICH TRÖHLER, M.D.: QUANTIFICATION IN BRITISH MEDICINE AND SURGERY 1750-1830, WITH SPECIAL REFERENCE TO ITS INTRODUCTION INTO THERAPEUTICS

The aim of this thesis has been to enquire into the development of mathematical methods of assessment in the study of disease and treatment. The traditional view has been t ha t quantitative analysis was "delayed" until around 1830 when it became recognized as an achievement at the Paris hospitals. The principal reasons advanced to account for this delay are t hat large hospital Services hardly existed in the l8th Century, and that the imperfect state of pathology prevented sufficient identification of disease entities. Furthermore, Paris was a centre for mathematical science, and the quest for certainty in medicine acquired a considerable momentum there from the late 1820s.

Upon examination, these factors have been found inadequate to account for this delay äs a European phenomenon: For instance my re-search on British medicine from 1750-1830, concentrating on the major medical problems within the growing towns and the armed forces (fevers, scurvy, syphilis, midwifery and the major surgical operations), has un-earthed a much earlier, deliberate use of quantification in clinical medicine.

This thesis describes a movement comprising doctors who were promoting the analysis of (mass) observations by simple arithmetic äs a new and the only sure way to gain certainty in medicine. They have thus been termed "arithmetic observationists". The movement took distinct shape in London, the provinces, and in the Navy around 1780. Thereafter, the method spread steadily despite Opposition, becoming a Standard technique äs revealed in the publications of authors associated with dispensaries, specialized hospitals and the armed forces in the early 19th Century. This thesis also discusses, in philosophical, social and institutional terms, the origins, significance and limitations of this movement, and its men both in civilian and military life.

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My conclusions are l) that historiographically the French contributions to quantitative nosography and evaluation of therapy, and the many subsequent endeavours right up to the early 20th Century, should be reconsidered in the light of these earlier British achievements, and 2) that arithmetic observationism marked a shift away from reliance upon Authority,to personal responsibility. For, to work credibly, the investi-gator required, as he still does, the observation of stringent moral Standards both in the conduct of research and in the Interpretation of results.

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TABLE OF CONTENTS

CHAPTER THREE I DISPENSARY AND HOSPITAL MEDICINE

CHAPTER FIVE : ARMY MEDICINE

PREFACE

All books and articles in journals referred to have been designated by the name of the author, the year of publication and, if appropriate , the page numbers in the references at the end of each chapter. Full references are given in the Bibliography at the end of this thesis. For some works a shortened title only has been indicated. The volume numbers of books have been preceded by the abbreviation Vol., those of periodicals have been underlined. All numbers are indicated by Arabic numerals.

The abbreviations employed for periodicals have been based mainly on the international code contained in World medical periodicals (published by the World Medical Association, New York, 1957) and according to the World list of scientific periodicals (4th edition, London, Butterworths, 1963). Some periodicals, not being included in either of these works, were abbreviated as follows:

Ann. Med.	Annals of Medicine (Edinburgh, 1796-1804)
Ann. Phil.	Annals of Philosophy (London, 1813-1826)
Archs. gén. Med.	Archives généraux de Médecine (Paris, 1823-1914)
Dublin Hosp. Rep.	Dublin Hospital Reports and Communications in Medicine and Surgery, (Dublin, 1818-l830)
Dublin quart. J. med. Sci.	Dublin quarterly Journal of medical Science (Dublin, 1846-1871)
Edinb. med. surg. J., or Edinburgh Journal	Edinburgh Medical and Surgical Journal (Edinburgh and London 1805-1855)
Ess. Obs. phys. lit.	Essays and Observations; physical and literary, read before a Society in Edinburgh, (Edinburgh, 1754-1771)
Hist. Acad. roy. Sci. (Paris)	Histoire de l'Académie Royale des Sciences Paris (Paris, l699-1790)

Lond. med. J.	London medical Journal (by a Society of Physicians) 1781-1790
Lond. med. Gaz.	London medical Gazette,(London, 1827-1851)
Lond. med. Rev. Mag,	London medical Review and Magazine (London, 1799-l802)
Lond. med. Rev.	London medical Review, (London 1808-1812)
Med.-chir. J.	Medico-chirurgical Journal and Review, (London 1816-1818)
Med.-chir. Trans.	Medico-chirurgical Transactions published by the Medical and Chirurgical Society of London, (London, 1811-1902)
Med. Ess. Obs.	Medical Essays and Observations, (Edinburgh, 1733-17HU)
Med. Obs. Inq.	Medical Observations and Inquiries, (London, 1757-1784)
Med. phil. Comment.	Medical and philosophical Commentaries by a Society of physicians of Edinburgh, (Edinburgh, 1773-1779)
Med. Comment.	Medical Commentaries, (London, Edinburgh, 1780-1795)
Med. Trans. roy. Coll. Phys. (Lond.)	Medical Transactions of the Royal College of Physicians of London, (London, 1768-1820)
Mem. Acad. roy. Chir. (Paris)	M&noires de l'Académie Royale de Chirurgie, (Paris, Delaguette, 1743-1774
Mem. med. Soc. Lond.	Memoirs of the Medical Society of London, (London, 1787-1805)
Mem. Soc. mSd. Obs. (Paris)	Memoires de la Société Médicale de Observation de Paris (Paris, 1837-1856)
Prix Acad. roy. Chir. (Paris)	Recueil des pièces qui ont concourru pour le Prix de 1'Académie Royale de Chirurgie, Paris, (Paris, Le Prieur 1757-1778)
Trans. Soc. Improvement med. chir. Knowledge	Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, (London, 1793-1812)
Trans. Ass.F. Lic.K.Q. Coll. Phys. Irel. or Transactions of the ... College of Physicians in Ireland	Transactions of the Association of Fellows and Licenciates of the King's and Queen's College of Physicians in Ireland, (Dublin, 1817-1828)

Trans, med. Soc. Lond.

Trans. Med. Soc. Lond.	Transactions of the Medical Society of London, (London, 1810-1817)

Other abbreviations sometimes used in this thesis are:

D.N.B.	Dictionary of National Biography (See Bibliography)
D.S.B.	Dictionary of Scientific Biography (See Bibliography)
F.R.C.P.	Fellow of the Royal College of Physicians
F.R.C.S.	Fellow of the Royal College of Surgeons
F.R.S.	Fellow of the Royal Society
F.S.A.	Fellow of the Society of Arts
H.E.I.C.	Honourable East India Company
Hirsch	Biographisches Lexikon (See Bibliography)
Kieler ceitr. Gesch. Med. Pharm.	Kieler Beitrage zur Geschichte der Medizin und Pharmazie
L. & C. or Lloyd and Coulter	Lloyd and Coulter 1961, (See Bibliography)
L.R.C.P.	Licenciate of the Royal College of Physicians
M.B.	Bachelor of Medicine
M.D.	Doctor of Medicine
Munk's Roll	Munk 1878, (See Bibliography)
Garrison and Morton	Morton 1954 (See Bibliography)
Int. Congr. Hist. Med.	Proceedings of the international Congress of the History of Medicine
Int. Congr. Hist. Sci.	Proceedings of the international Congress of the History of Science
Johnston's Roll	Peterkin and Johnston 1968 (See Bibliography)
n (in description of results)	number of patients or observations

With the exception of quotations, where the original spelling has been kept, the use of capitals has been restricted to names of persons, institutions, places and periodicals, and to the first word of titles of references.

Translations of French and German passages are my own unless an original English edition could be quoted.

Biographical references to the D.N.B., to Munk's Roll, Johnston's Roll and Hirsch are usually not specifically referred to.

CHAPTER ONE: INTRODUCTION

A. Clinical Statistics - The traditional View

My interest in the history of quantification in clinical medicine, especially in therapeutics, arose in spring 1976 during the preparation of a lecture on the Swiss surgeon and Nobel Prize winner Theodore Kocher (1841-1917). I was struck by the important place statistics took in his clinical work, and at the same time by their one-sidedness and inadequacy in demonstrating the presumed superiority of a particular technique, or of surgery as compared to the "natural course" of a disease, or to internal therapy.

Looking out for antecedents I found that Kocher directly adopted in the 1870s for thyroidectomy the programme Spencer Wells (1818-1897) of London had used in the 1860s for ovariotomy: the launching of an operation by showing its safety - and/or its superiority - with statistics of all cases operated on. Kocher was also influenced by the great Theodor Billroth (1829-1894), professor of surgery at Zurich and Vienna, who in the 1860s emphasised and demonstrated the objectivity of surgical experience by expressing his hospital practice with true and complete figures. Billroth, in turn, was influenced by Wells,[1] but especially recognised the earlier and contemporary work of German and American military surgeons.[2] Later he also drew attention to work form Paris, where Joseph François Malgaigne (1806-1865) had in fact analysed statistically the results of major operations of nine great hospitals around 1840. He also had used the numerical method for investigations into the aetiology and clinical picture of surgical diseases like fractures, luxations, and herniae.[3]

As Billroth was also a medical historian it is since his times that Malgaigne is considered the first major surgical statistician,[4] Well an European pioneer,[5] and Billroth "the founder of true and utilizable surgical statistics".[6]

Not only for surgery but indeed generally for clinical medicine, medical historians have been looking at numerical research, both in nosography and therapeutics, as one characteristic achievement of medicine at the great Paris hospitals in the beginning of the 19th century.[7] The pioneering role of the internist and psychiatrist Philippe Pinel (1745-1826) and of his full-time psychiatrist pupil Jean-Etienne D. Espuirol (1772-1840) have been emphasised, with reference to their philosophy of "*empirisme raisonné*" having its roots in turn in the quantifying tendencies of the Encyclopaedists and certain 18th century French naturalists like Buffon (1707-1788).[8] Certainly there was a precursor, the naval surgeon James Lind (1716-1794), who is sometimes considered "the real founder" of the extremely important method of controlled clinical experiment.[9] And, as also mentioned by Ackerknecht, some Paris physicians used the numerical approach in nosography and presentation of therapeutic results in the 1810s and 1820s, yet without shaping any conscious programme.[10]

It is the physician Pierre Charles Alexandre Louis (1787-1872) with his works on phthisis (1825) and on the value of bloodletting (1828, 1835), and with his *Société Médicale d'Observation* founded in 1832,[11] who is viewed by all authors as the key figure of the numerical method in clinical nosography and therapeutic research in both personal and institutional terms. His influence and that of his direct pupils (especially remarkable in America and Switzerland) has been studied for some time.[12]

A contest about the applicability of statistics to clinical medicine arose in French medical literature in the 1830s.[13] It was formalised in two discussions within both the *Académies des Sciences* (1835) and *Médecine* (1837).[14] The work of the mathematician Gavarret (1809-1890), who improved the method mathematically, is appreciated as the first real monograph on medical statistics, and finally Queletet's (1796-1874) *Sur l'homme* fostering the notion of *l'homme moyen* was first published in Paris in 1835.

Shryock in his History of quantification in medical science wonders why quantitative methods were so rarely used in therapeutics before about 1830 in Paris.

"With all the enthusiasm of the iatro-physicists for measurement in principle, and with all the controversies about *this* treatment or *that*, one might have expected numerical checks long before 1800. Did not many physicians, as [...] Louis later noted, claim that a given drug was best because "most" of their patients benefited from it? And could not means for reducing such vague quantification to exactitude be found in the statistical approach already so common in public hygiene - or more immediately, in the statistical studies made for decades after 1720 on inoculation, which was close to a clinical procedure?"[15]

In his answer Shryock shows that many physicians until late in the 19th century took an attitude of antipathy to quantification, partly in reaction against the speculation of the earlier iatrophysicists, partly from a dislike of the idea of submitting their insights and accumulated wisdom to the prosaic test of numbers. But more significant, according to him, were three professional circumstances.

Firstly, relatively large hospital services hardly existed until after 1800. Secondly, the state of contemporary pathology did not allow sufficient identification of disease entities potentially subject to measurement. Both these hindering circumstances changed when the hospitals of Paris were reorganized, and clinical pathology flourished after 1800. Clearer pictures of distinct diseases were emerging by the 1820s.

As a favourable circumstance, thirdly, Paris also happened to be a centre of mathematical interests. Laplace (1749-1827) published there in 1814 his classic *Essai philosophique sur les probablilités*, the development of a series of lectures he had delivered in 1795 to the Ecole Normale. This was indeed a popular work addressed to the general educated reader, added as introduction to al later edition of his technical *Théorie analytique des probabilités* (first published in 1812). In it he called attention to ways in which statistics could be employed in medical research.[16] Shryock suggests that this was the influence underlying Pinel's use of statistics and their use by the hygienist Villermé (1782-1863 in the late 1820s.

He concludes that "in these several ways, the medical stage was set by about 1830 for a full presentation of clinical statistics", in which Louis was the chief actor.[17]

This view looks at highlights largely within the framework of Paris hospital medicine. It is in agreement with that of Rosen who saw another favourable circumstance in the fact that "the question of certainty in medicine was a matter of considerable moment" as from the late 1820s:

"Confronted by a variety of conflicting schools (vitalists, empiricists, Brunonians, Hippocratics, humoralists) how was the practitioner to determine the true from the false? Furthermore, related to this question was the problem of establishing how far the course of a disease was due to the power of nature or the art of the physician."[18]

Undoubtedly Paris *was* important at that time with respect to clinical quantification. And this view is sustained by Louis himself, who in his published work, did not see himself as an heir of any tradition, but rather took the tone of a revolutionary innovator for the use of statistical evaluation in medicine[19] (see below). It is also supported by the existence of abundant contemporaneous medical literature. It reflects, however, partly at least the lengthy and somewhat theatrical discussion of the question held in the settings represented by the rich institutional background of Paris.

B. The problem

Returning now to the topic of surgical statistics, I recognized that the work of the French surgeons before Malgaigne has received rather scant attention from this point of view. One cursorily noted exception are the debates on the treatment of bladder stone with lithotomy or lithotrity fought on statistical grounds on the Paris scene from the late 1820s onwards.[20]

On closer examination I found that the lithotritists promoted their new operation with the same means Wells and Kocher would later use for ovariotomy and thyroidectomy respectively. This fact raised the possibility that this method had been employed for launching new techniques for lithotomy and other operations already introduced in the 18th century. I also came to realize that not only Billroth but Wells and Malgaigne himself had been initially motivated, and had developed their flair for statistics, by their experiences in the Army or Navy, rather than by their connection with the great urban hospitals.[21] This was obviously true for Lind's trial, too.

From these observations, and aware also of 18th century innovations in surgery, military and naval medicine, I began to doubt the four principal explanations for the “delay” until the 1830s of the introduction of quantification into therapeutics of nosography.

The argument of scarcity or inexistence of larger hospital facilities for research into diseases, rather than for mere caretaking of patients, applied throughout the 18th century to the conditions in Paris[22] and to the teaching wards established according to Boerhaave’s Leyden model e.g. in Edinburgh and Vienna, which had only twelve beds.[23]

However, this fact did not prevent the eclectics of the so-called first Vienna School from presenting numerical results of therapeutic studies in the second half of the 18th century. Some were carried out over years - which in turn presupposed accurate record-keeping, a prerequisite for the application of statistical analysis. Van Swieten (1700-1772) had 4880 registered cases of syphilis treated with mercury. Haen (1704-1776) compared numerically treated with untreated fever patients. Auenbrugger (1722-1809), the inventor of percussion, collected, over a period of twenty years, ten cases of mania, which he had first observed during some months before treating them with camphor.[24] Stoercks’s (1731-1803) therapeutical works (especially with hemlock on cancer) were tremendously admired during the later part of the 18th century. However from today’s point of view their value was limited, for the number of patients treated was small. In addition Stoerck’s progressive methods for research on new drugs (from the animal experiment over the self-assay to the patient) were submerged by his uncritical judgement: whenever a patient died, he explained this away by the weather, consumption of wine or bad general condition of the patient.[25] Eventually, too, all the patients dying in the *Allgemeines Krankenhaus* of Vienna had to be autopsied, and not only those previously selected for the small teaching-wards of twelve beds.[26]

Similarly, record keeping was a feature of the Edinburgh medical school which was the most influential in the western world after 1740.[27] Thus the use which one of its founders, Alexander Monro *primus* (1697-1767), made of them would appear worth consideration in this context.

Furthermore in Britain for example, where towns were rapidly growing in the second half of the 18th century, there occurred the gradual organisation of medical facilities by the establishment of a great number of new non-university medical institutions, hospitals and dispensaries, designed to provide care for the masses of labouring urban poor and sometimes to serve for the advance of knowledge in physic. As Chaplin suggests, many of them owed their origin both to the insistent demand of energetic physicians, especially from the rising Edinburgh school, who sought a field for clinical work, and to the wider conceptions *en vogue* concerning the Christian duties of the rich towards the poor.[28] Thus the use of these new facilities for numerical research, too, would merit investigation, for they offered early opportunity for controlled mass observation.

Lind worked in conditions furnishing similar possibilities. He was a naval surgeon and, indeed, favourable surroundings were given not only in civil hospitals but also in the armed forces, on shore or afloat. In addition, military medical service was carries out in a general framework of controlling and reporting for tactical and administrative reasons. Naval and military medicine saw great innovations concerning prevention *and* cure of diseases form the 1740s onwards when the great European powers - and especially Britain - were more or less constantly involved in warfare for seventy years. Thus the research for numerical evaluation of therapy within armies and navies would appear to be potentially rewarding.

Shryock's second explanation of the time lag concerns the imperfect state of 18th century pathology. This appears obvious from our present point of view: "Statistics concerning such categories of the time as "convulsions" or "decay" would have been quite meaningless"[29] ... to *us*, but not necessarily to contemporaries, though it would presumably render trials confusing, and with such loose disease-entities medical men would end up proving what they wanted to prove. Louis is signalled out from this point of view because he showed that the two treatments he compared were both ineffective, (as we think they were). Yet, did this finding come to him as a revelation, or was he a therapeutic nihilist also just confirming his pre-existing conviction? This question, the investigation of which would lead beyond the scope of this thesis, shows that what one looks particularly for in quest of objective trials, are authors who are actually surprised by their own results. This is sometimes difficult to evaluate from their *a posteriori* writings, when authors have identified themselves with their own findings. The primitive state of pathology would thus appear to constitute a hindrance for the objective numerical evaluation of therapy, but with certain limits only. Besides, some diseases *were* clinically well distinguished - or became so during the period in question. This holds, e.g., for small-pox, measles, gout, and scurvy, the greatest problem specific to naval medicine,[30] and in some way for the huge category of "fever", which as a whole, was perhaps the greatest concern of the contemporary medical community, civil and military. As I shall outline in a special section on fevers, 18th century physicians recognized that there were consistent clinical differences between fevers. John Huxham, Sir John Pringle, and others had developed a good clinical picture of typhus by the 1750s,[31] as a "specifically contagious disease with a highly specific cutaneous eruption resulting in a direct debility and occurring in a debilitated population".[32] There were also repeated efforts to provide an adequate system of classification of fevers. And, as pointed out by two historians, it was only during what Niebyl calls the "blood-letting revolution" around 1810 in Britain, and later in France, that various fevers were again lumped together indiscriminately for some time.[33]

Yet there was one field of the healing art to which the argument of limited pathology would not appear to be pertinent, and this was surgery: surgical pathology existed well before Morgagni (1682-1771).[34] In operations such as, to name the most frequent, amputation and lithotomy both operators and testimonies could see what was done and what the consequences were, the parameters of evaluation being simply cure, failure or death. Remembering the quite extraordinary "scientific, practical, and social rise of surgery in France as well as in Great Britain during the eighteenth century"[35] and what has been said above on civil and military institutions, one might perceive several motivations for numerical statements on surgical therapy.

As a third important factor for the birth of the numerical doctrine in Paris, Shryock sees the role of that city as a mathematical centre. It might be questioned whether the presence of mathematicians who were to become important for the development of probability calculus, such as LaPlace and Poisson (1781-1840), was really directly stimulating for the beginnings of clinical statistics there, or whether it might rather account for the improvement of the method in the early 1840s (by Poisson's pupil Gavarret).

In fact, the mathematics used by Pinel and Louis were simple comparisons of arithmetical means (although Pinel wrote about the necessity of application of the calculus of *probability*, in 1807 and 1809). Neither he nor Louis referred directly to La Place's suggestion of the possible applicability of the calculus to medicine, and Pinel's programmatic statement appeared actually first in 1807[36] i.e. seven years before La Place's popular booklet (1814).

Lewis analysed the roots of Pinel's attachment to the use of the theory of probability (1967). They were both particular and general. Pinel had been educated in the tradition of the *septem artes liberales*. He took an early interest in mathematics and consorted with d'Alembert, Condorcet and other men of distinction in mathematics. As a young medical man he preferred, with some discrimination, the mechanistic or mathematical approach to physiology and medicine to the vitalist doctrines *en vogue* when he was a student at Montpellier: his master's thesis was "on the certainty which the study of mathematic instils into the judgement of scientific problems", and he published several mathematico-mechanistic papers (on bony dislocation) and an annotated translation of Baglivi's (1668-1707) works.

Generally, Pinel was in his intellectual outlook as well as in his humane impulses a product of the Age of Enlightenment, a younger brother of the Encyclopaedists, and like them he was indebted at some crucial points to English influence and example.[37] This applied not only to his famous reform in the treatment of insane which was an example of French humanism deriving its reaction against Descartes's views from Locke (1632-1704) and other English and Scottish empiricists. From such sources, as d'Alembert (1717-1783) pointed out, the distrust of systems and of facile explanations took its origin.

Pinel had a direct contact with English writings, which he often quoted. He translated Cullen's (1712-1790) *Institutions of medicine* and a selection of papers from the *Philosophical Transactions of the Royal Society*. He considered them as examples of reporting facts and drawing legitimate conclusions from them, serving as a remedy to men of inexact mind, who preferred brilliant hypotheses and the sterile verbiage of metaphysics. Lewis quotes him writing: "I have tried to make these memoirs more widely known, as much for their firm logic and experimental rigour as for novelty and importance of the facts they report." Among the articles Pinel selected were several concerning vital statistics, e.g. midwifery reports. He translated Charles White's and Thomas Percival's observations on tables of mortality (See below p.??) twice (through absent-mindedness!), and included them in both the volumes devoted to anatomy – physiology and to medicine of his *Abrégé* (1790-1791). All these articles did not contain sophisticated mathematical treatment of the data. Pinel

"made no reference to his fellow-countryman, Deparcieux, nor in this context to La Place, d'Alembert, Lagrange (1736-1813), Condorcet or other illustrious Frenchmen who applied the mathematics of probability to social and scientific problems."[38]

He selected simple statistical articles on the mortality of smallpox and the beneficial effect of inoculation neither referring to Daniel Bernoulli's (1700-1782) memoir, nor to the contributions by d'Alembert, and La Place on the same subject. He was clearly less interested, at this late stage, in mathematics *per se* than in its use in demonstrating more firmly the efficacy of a particular treatment for a particular disease.

In this sense passages of his writings (1807, 1809) echo prescient statements by Condorcet (1743-1794) in 1768 and anticipate those by La Place (1814).[39] Pinel set up a research programme designed for the whole of medicine and carried it out in psychiatry.[40]

He passed on his enlightened view to his pupil and friend Esquirol,[41] but it is hard to say whether Pinel's teachings on the use of statistics directly influenced the outlook and practice of other contemporaries. Rosen and Lewis state that to their knowledge, Louis and others who later cultivated this field owed nothing directly to Pinel.[42] In his published work Louis hardly gave direct hints of his philosophical beliefs. Rather he presented himself and his school as the true inventors of the practical application of statistics to medicine. He wrote in 1837 that when he had been a student the collection of facts had become “ a completely unusual thing”, and as chemistry had stepped out of childhood in the last 40 years due to quantification, the time for medicine to do so had now come, too.[43] In his long programmatic defence of the statistical method before the Paris *Académie de Médecine,*[44] or in his article on clinical research in the first volume of *Mémoires* of “his” *Société Médicale d’Observation* of the same year, he would only refer favourably to contemporary French authors.

As good examples of nosography he mentioned the numerical works of his two Geneva pupils d’Espine and Maunoir, whilst criticising Corvisart and Laennec, whose pathological assertions were made merely from memory.[45] Nor did Pinel escape unscathed for he had sent young students to collect the facts: “One would laugh at a chemist who had his analysis done by a beginner in this career – what has one to think of doctors who have collected their facts by young pupils?”[46]

As to the application of statistics to therapeutics, Louis included a justification of “his” method in the enlarged second edition of his *Recherches sur les effets de la saignée* (1835) followed by an analysis of five French works concerning the indication and effect of bloodletting written in 1770, 1805, 1807, 1814 and 1826. (The last three had been prize-winners from medical societies in Paris and Tübingen, and the Royal Society of Marseilles respectively).[47] Louis dismissed them one after the other as founded entirely on *a priori* reasoning and illusory experience, and as lacking any rigorous numerical analysis. “There is no shadow of any concrete evidence…One would even believe that…[the authors] held themselves dishonoured by trying it.”[48] In the same breath Louis also criticised Laennec’s (1781-1837) recent numerical results of the treatment of pneumonias with tartar as compared with those indicated by Rasori (1766-1837): Laennec had neglected to group his patients according to age, sex and seasons, and had provided inadequate diagnostic criteria.[49]

The fourth point raised by Rosen, i.e. the consciousness among medical writers of the questions of certainty in medicine and of iatrogenic influence in the natural course of disease was surely relevant by 1830. But this was also true, for both issues, in the last decades of 18th century France as illustrated by Ackerknecht and more recently by Risse.[50] In Britain, too, one can go back to 1761 or even to 1731, and find a whole group of inquiries "into the means of improving medical knowledge by examining all those methods which have hindered or increased its improvement in all ages". As we shall see, this British quest for "medical certainty" was inspired by Francis Bacon (1561-1626) and Scottish Enlightenment philosophy.[51]

Thus, the institutional, professional and "climatic" arguments adduced by Shryock and Rosen would not a priori explain a late programmatic introduction of quantification into therapeutical research in the 1830s, even in Paris. Rather, their re-examination unravels more reasons for a possible earlier onset.

One further indication for this earlier onset might be seen in yet another aspect of the 18th century medico-philosophical climate. As already hinted at above, in connection with the first Vienna School, there was in the second half of the century, a widespread desire to heal the age-old schism between the dogmatist's and the empiricist's way to truth. This movement was peculiar to no one country,[52] but outstanding in Britain (see below). From a methodological point of view one might wonder whether this new approach, sometimes called "rational empiricism", "intelligent empiricism" or "systematic empiricism",[53] would bring about an amendment of the essential logical error which was inherent in both the rational systematists and empiricists, as long as they proceeded alone, namely their failure to appreciate the full significance of verification:

"The rationalists were misled because they failed to verify their hypotheses. On the other hand, the empiricists rejected potentially valuable hypotheses and deduction in general, simply because they did not realise that proper verification would render the use of such methods reliable."[54]

How did verification, or medical evidence, evolve under circumstances which blurred some of the differences between the two camps, so that by 1800 both saw themselves as the true heirs of a Baconian tradition, emphasising observations and piling up of facts?[55] How did the dogmatist's need for synthesis and deduction influence the empiricist's plea for observation and induction? Would this atmosphere not be a possible breeding ground for statistical analysis of well observed facts?

It follows from the foregoing that a reconsideration of our views on the beginning of quantification in clinical medicine might derive from the study of 18th and early 19th century work on the quantitatively important diseases such as "fevers" and scurvy, on externally recognisable ailments and on surgery, especially by doctors attached to newly organised hospitals and to the armed services.

Indeed, the arguments leading immediately to the formal discussions in the Paris *Académies des Sciences* (1835) and *Médecine* (1837) still were the questions of treating bladder stone by lithotomy or lithotripsy and of curing typhus with or without bleeding and purging respectively. And it is remarkable that on both occasions a few earlier British statistics were quoted for comparison. These British statistics stemmed precisely from such an institutional background.

In 1835 the French surgeons referred to the statistical works on bladder stone by Marcet (1817), Smith (1820), Prout (1821) and Yelloly (1829), which contained detailed indications on mortality of lithotomy. These analyses were drawn from the Norfolk and Norwich Hospital, opened in 1771, and the authors referred to a work on similar lines by Dobson (1779).[56]

In 1837 one physician intervening in the discussion on typhus found that "the statistics of Clarke in London from 1777-1779" were the only available reference for the results of expectant treatment of typhus fevers.[57] In fact this statement referred to the statistics that John Clark, a former naval surgeon, had drawn from his practice at the Dispensary in Newcastle-upon-Tyne, opened in 1777. Similarly, the statistics of Pringle, the famous 18th century British military physician, and of Haen, the contemporaneous Vienna eclectic, were mentioned in discussion in connection with the seasonal variations of disease-patterns.[58]

Thus a number of French physicians, even if not Louis, referred to the British with respect to their statistics. Accordingly, I propose to describe clinical-therapeutic issues chiefly as discussed in Britain 1750-1830. This geographical restriction has been imposed partly by some general hints in the secondary literature suggesting that my approach, if at all, might be particularly successful in the British context: Ackerknecht for example, in his book on therapeutics, states that

"The real practical progress in the field of objective examination of therapeutic experience during the eighteenth century was realised in England. It is probably an accident that this country, changed through the revolutions of the 17th century, remains during the 18th century the land of the philosophy of experience…and progresses also greatly in the field of economics and politics."[59]

Apart from the availability of primary sources in London I was also guided by the consideration of a number of political, professional and institutional factors which would seem *a priori* to be favourable for the use of statistics in British medicine. I shall outline these factors in the next chapter, fully aware that some of them apply to other countries, too. Indeed, research concerning the German lands, Austria, Italy and to some extent France might be rewarding, as indicated by some references which shall be occasionally included for comparison. Yet these circumstances appeared to have been particularly united in Georgian Britain. Before entering into their brief outline I will illustrate my choice with two examples from contemporary primary sources, the first serving more explicitly also as an example of applied rational empiricism.

C. ILLUSTRATIONS

In the chapter regarding therapy of his *Treatise on insanity* (1806) Pinel asked: "Are medical opinions founded upon observations?" Having denounced those "errors of a doctrine depending for its support upon prejudices, hypotheses, pedantry, ignorance, and the authority of celebrated names" he praised as examples: "Ferriar in England, and Laughter in Germany, [who] have made trials of some simple remedies, which sufficiently indicate, that they are on the right path, that of analytical enquiry, to useful definite conclusions".[60]

John Ferriar lived form 1761 to 1815, chiefly, in Manchester, and was attached to the Infirmary opened there in 1752. Pinel quoted from a paper in the first volume of Ferriar's *Medical histories and reflections* (1792) containing also the programme of research which Ferriar had put into action.

According to Ferriar it was the work in public hospitals which afforded

"the most favourable opportunities for ascertaining with precision many facts in the history of diseases and for appreciating the value of established methods of cure…Something may be added to the stock of science, by unwearied attention to a considerable number of patients, indiscriminately taken in a great town."

Indeed, on a single instance of success, however faithfully delivered, no point of practice could rest; and although minuteness in descriptions was recommended by the best systematic writers since Bacon, they could not have intended the great and unnecessary prolixity of modern case-writers. The method "so fashionable at present of publishing single cases, appears not well calculated to enlarge our knowledge, either of the nature or cure of diseases". But, Ferriar maintained, serial observations – resulting from experiments, clinical cases and autopsies – would become reliable only if they were written down in a journal, regularly kept and including both favourable and unfavourable outcome of a treatment. This was "absolutely necessary" if the physician wanted to avoid those false conclusions he arrived at "if he trusts to memory alone". Furthermore, data obtained in this way could and must be compared with those of other physicians.[61]

Ferriar believed that it had been the tendency of medical writers to form systems which hitherto had been the chief obstacle to his plan. These gentlemen, he thought, "would do well to read Mr Locke's chapters on abuse of language. A system ought to be nothing more than the arrangement of [empirical] facts, in convenient order for the memory".[62] And he acknowledged his obligations to Francis Home (1719-1813), a former military surgeon (see below), for the design of this little work.

Further screening of the British literature up to the time of Louis leads up to a monograph on "medical statistics" published in 1829 by Francis Bisset Hawkins (1796-1894), a physician to the Westminster Dispensary.[63] It was the written-up version of the author's Gulstonian lectures delivered at the Royal College of Physicians in 1828, well before the programmatic papers of the Paris School, the Academy discussions there, and Gavarret's book. Hawkins stated dryly:

"Statistics has become the key to several sciences…. And there is reason to believe, that a careful cultivation of it, in reference to the natural history of man in health and disease, would materially assist the completion of a philosophy of medicine…Medical statistics affords the most convincing proofs of the efficacy of medicine."[64]

He subjoined a concrete example from a paper which Sir Gilbert Blane (1749-1834), the well known naval physician and reformer, had given to the London Medical and Chirurgical Society in 1813. Blane presumably got this example for John Millar (1733-1805), a forerunner of Hawkins as first physician to the Westminster Dispensary. Millar had advanced it among his arguments for the use of arithmetic for the evaluation of therapy of fevers in 1779 (see below). If the word "statistical" were replaced by "arithmetic" in Hawkin's following sentence of 1828, Millar might well have been its author fifty years earlier. Hawkins wrote:

"If we form a statistical comparison of fever treated by art, with the results of fever consigned to the care of nature, we shall derive an indisputable conclusion in favour of our profession."[65]

These examples from the work of John Ferriar and Bisset Hawkins belong in the foreground of the picture of my investigation. Before attempting to paint it more fully, I shall, in the next chapter, set more precisely its background and its frame.

D. REFERENCES TO CHAPTER ONE

[1] Billroth 1879, p. 367.
[2] Billroth 1869, preface; Lesky 1967, p.394.
[3] Billroth 1879, pp. vi-vii.
[4] *ibid*; Billroth 1933, o-790; Dennis 1895, Vol. I, p.133; Hirsch 4; pp. 44-45; Wangensteen et al. 1972.
[5] Lesky 1976,p.394.
[6] Lesky n.d., p.328.
[7] Petersen 1877, pp. 153-161: Underwood 1951; Rosen 1955; Milt 1956; Bull 1959; Shryock 1961; Muellner 1966; Mitaritonna 1968; Schipperges 1971; Piquemal 1974; Seydel 1976, pp. 68-69, 73-74.
[8] Delaunay 1953; Muellner 1966; Cadoret 1969.
[9] Ackerknecht 1973, p.91.
[10] Ackerknecht 1967, p. 104; 1973, p. 105-106.
[11] Astruc 1932.
[12] Osler 1908; Greenwood 1936; Steiner 1939; Steiner 1940; Artelt 1958; Muellner 1967; Bloch 1969; Bariéty 1972; Bollet 1973
[13] See e.g. Danvin 1831; Fuster 1832; Fuster 1832; and others quoted by Gay 1939.
[14] Astruc 1932; Delaunay 1953; Ackerknecht 1967.
[15] Shryock 1961, p.230.
[16] The passage is quote in Bariéty 1972, p. 177.
[17] Shryock 1961, p. 231.
[18] Rosen 1955, p.39.
[19] Ackerknecht 1967, p. 10.
[20] Gadient 1963, p.17.
[21] Malgaigne 1842, pp.9-12; Shepherd 1965, pp. 81-81, 92.
[22] Gelfand 1973 B.
[23] Turner 1929, pp. 11-12, 24-26; Probst 1972, pp. 61, 107.
[24] Probst 1972, pp. 101, 138, 143, 163.
[25] Zumstein 1968; Ackernkecht 1973, pp. 81-82, 92.
[26] Probst 1972, p. 198.
[27] Risse 1974.
[28] Chaplin 1919, p.21.
[29] Shryock 1961, p. 231.
[30] Lloyd and Coulter vol. 3, p. 293.
[31] Smith 1976, p. 344.
[32] Niebyl 1977, p. 476.
[33] *Ibid*., p. 476-477; Delaunay 1953, p. 322-323.
[34] Temkin 1951, p. 254.
[35] Ackerknecht 1973, p.90.
[36] Muellner 1966, p. 125.
[37] Lewis 1967, p.9.
[38] *Ibid*., p.12.
[39] *Ibid*., pp. 12-13.
[40] Muellner 1966, pp. 124-125.
[41] Lewis 1967, p. 13; Cadoret 1969, pp. 40-41.
[42] Rosen 1955, p. 42; Lewis 1967, p. 14.
[43] Louis 1837, p. 4.
[44] Bull. Acad. Roy. Méd. (Paris), 1; 730-748, (1836-1837).
[45] *Ibid*., pp. 739-744.
[46] Louis 1837, p. 4.

[47] Louis 1835, pp. 70-117.
[48] *Ibid*., p.89.
[49] *Ibid*., pp. 63-67.
[50] Ackerknecht 1967, pp. 3-8; Risse 1971.
[51] Gay 1939, p. 11; Neuburger 1950.
[52] Shryock 1948, pp.65-66; Milt 1956; King 1971, p. 56; Gelfand 1973 A, p. 380.
[53] Bostock 1833, pp.lxiii, lxix; Shryock 1948, p. 69; Milt 1956.
[54] Shryock 1948, p.37.
[55] Bostock 1833, p. lxiii; Shryock 1948, pp. 36-40.
[56] C.r. hebd. Séanc. Acad. Sci. (Paris) 1; 171, (1835).
[57] Bull. Acad. Roy. Méd (Paris) 1; 498, (1836-37).
[58] *Ibid*., p.537.
[59] Ackerknecht 1973, p. 91.
[60] Pinel 1806, pp. 220, 222.
[61] Ferriar 1792, pp. xvii-xxix.
[62] *Ibid*., pp. xxiv-xxv.
[63] Munk, Vol. 3, pp. 303-305.
[64] Hawkins 1829, pp.2-3.
[65] *Ibid*.

CHAPTER TWO: THE BRITISH MEDICAL SCENE 1750-1830

A. GENERAL BACKGROUND

1. METHODOLOGICAL QUESTIONS

The last pages of the preceding chapter centred on Britain both for the philosophical background of medical rational empiricism and for examples of its application to therapeutic questions over a period of fifty years, form 1779 to 1829. Indeed, it has been claimed in the 1830s already that this "good" method became especially characteristic of this country in the late 18th century,[1] where a mystic something known as "common sense" was said to inhibit speculative enthusiasms. Doubtlessly it was no accident that American medical students – a good indicator of the best in European medicine – were usually found in London or Edinburgh after 1780. "Here they observed not only 'common sense' practice, but also a conscious recognition of the whole trend in methodology."[2]

Not only did the ordinary medical literature often contain discussions of method, but a number of British works, published between 1760 and 1800, were specifically devoted to this subject. The first was perhaps William Hillary's *Inquiry* (1761).[3] John Gregory formulated the modern principles of rational empiricism most clearly in his *Observations on the duties and offices of a physician* (1770).[4] John Aiken included them in a section of his *Thoughts on hospitals* (1777),[5] addressed to John Haygarth and read by Thomas Percival who had himself written two brilliant essays on the dogmatist and the empiricist physician in 1767.[6] James Sims followed with a *Discourse on the best method of prosecuting medical enquiries*, in 1774, John Coakely Lettsom with *The improvement of medicine in London*, in 1775.[7] John Millar described the method in *The duty of physicians* in 1776, Thomas Kirkland in his "Inseparability of the different braches of medicine" in 1783, George Fordyce in "Attempt to improve the evidence in medicine" in 1793 and an anonymous author in the *Edinburgh medical and surgical Journal* under the title "Is there any certainty in medical science?" in 1805.[8]

[This article made no obvious reference to Cabanis's *Du degré de certitude de la médecine* (1798) (which seems not to have been edited in English before 1823, in Philadelphia). It was rather a-warning-description of professional medicine as opposed to quackery, a motive also behind other methodological writings of the time.]

It is surprising then, that a British reviewer of Cabanis's *Du degré de certitude de la médecine,* a book in which the methodical clinical-therapeutic research of the Paris school is said to have some roots,[9] found nothing particularly new or exciting in it? He wrote:

"It is useful, it is good, as far as it goes, but it might have been better, if….[Cabanis] had proved the origin of the evil, by showing how bad reasoning has been misapplied on questions of physiology and physic, and many of his pages would have been better so filled, than with eulogies upon the art itself…."[10]

The sensible character of the best English work received due recognition in Europe as well as in America, as is illustrated by their being freely translated. The need for following English examples was readily admitted at times.[11] Speculation flourished longer in the United States and in several continental countries notably in Germany, Italy, and Spain, where Brunonianism, a most exuberant and almost the last recognised medical system,[12] had more influence than at home. In France, too, "strange to say, rationalism was dominant until almost the end of the century …. The great lay critic Voltaire (1694-1778) held saner views on medicine than did most of his professional compatriots".[13]

Yet there was no lack of critical works in all these countries aimed at Brunonianism and other speculative systems. Further, in France, conditions were shaping up for an era of amazing progress initiated by a medical – as well as political and social – revolution in 1789-1798.[14] And its structural achievements, such as clinical teaching and re-unification of traditionally more dogmatic medicine with an always more empiric surgery in the *écoles de santé*, had been brought to bear to some extent earlier, especially by the surgeons.[15] Clinical teaching had spread also through the influence of Boerhaave (1688-1738) and his men[16] and neither trend was limited simply to France.

In Britain one has only to remember the Edinburgh medical school and later in the century the influence of the free dispensaries on the beginnings of clinical teaching.[17] As for the tendencies of unification one might mention the number of surgeons taking medical degrees – to enhance their prestige and sanction their actual practice, which in the provinces was often on par with that of a physician anyhow.[18] On a social level the Medical Society of London, founded in 1773, was designed to accept as full members up to thirty apothecaries, surgeons and physicians each.[19] The work of William and John Hunter may symbolize both trends.[20]

2. NEW FACILITIES: THE HOSPITAL AND DISPENSARIES

Within thirteen years – from the victory in India during the Seven Years War in 1757, to the peace of Paris in 1763, and then to Captain Cook's (1728-1779) addition of Australia and New Zealand to the Empire in 1768, - Britain became politically the most powerful country in the world and remained so for the latter half of the 18th century. During this period, too, continues commercial development, and the whole series of changes conveniently summed up under the phrase of "early industrial revolution" inaugurated, in Britain, the massive urbanisation of population which has lasted until the present.

Many thousands moved from villages to crowded urban centres, where they found new problems and dangers, not the least of these related to health and disease. The miseries of the poorer classes, once lost in rural isolation, were now much more conspicuous. But the social changes which had led to this emergency also provided to some degree for its solution in the form of a reforming humanitarianism which in turn made possible the progress of clinical medical science. What had been lacking so far in this respect (at least in civilian life) was a supply of "material" for compiling clinical and pathological data related to any particular disease in less than a life-time. This depended upon the development of hospitals with clinical facilities, which had in turn to wait upon the growth of great towns. With the developing urbanisation the more farsighted physicians saw the need of institutions to care for the sick poor. The idea of a close relationship between the voluntary hospital movement and the industrial revolution has recently been challenged, among others on chronological and geographical grounds and because of the admission arrangements of the new infirmaries.[21] Yet old hospitals were reorganised and enlarged and new ones created.[22] In the English-speaking lands especially, they began to limit themselves to the core of the sick, rather than to continue the medieval practice of providing for unfortunates of all kinds and conditions.[23]

The charity hospitals and the out-patient dispensaries are truly one of the most outstanding features in the medical history of the reign of George III (1760-1820). In 1760 London had had seven general hospitals, six special hospitals and two asylums but no dispensary. By 1800 there were fourteen dispensaries, by 1820 34 dispensaries and three new hospitals. The provinces which had counted sixteen hospitals and one asylum before 1760 (mostly founded after 1736 only) had now 45 new hospitals, eight asylums and 36 dispensaries.[24] They were established according to the conventional voluntary hospital model, as initiated in London early in the 18th century (e.g. at Westminster Hospital and St. George's Hospital founded in 1719 and 1733 respectively). The London model spread subsequently to the provinces "providing a general framework for finance, management, and patient selection for most of the hundreds of hospitals established in Britain during the 18th and 19th centuries".[25]

Remarkable in this voluntary hospital movement is also the foundation of a number of hospitals specialising in the treatment of particular diseases: by 1800 there were in Britain ten maternity hospitals [Four of them were in London, not including a department at Middlesex Hospital founded in 1757. The next maternity hospital was opened only in 1826, in Dublin] founded between 1745 and 1798, two hospitals for venereal disease, one for small-pox and inoculation (1746), one for sea-bathing and air-therapy of tuberculosis (1791-92) and at least four fever hospitals.[26] [Among the Fever Hospitals founded in the 18th century were Chester, Dublin, Manchester and Liverpool] A dispensary for children had closed after having been active from 1769 to 1781.[27] An Institution for Investigating the Nature and Cure of Cancer was going to be opened in 1801 with a medical committee of the highest stature, while a special cancer ward was already in existence at the Middlesex Hospital since 1792.[28] A similar institution for research into the cure and prevention of contagious fever (the London Fever Hospital), was going to be opened in 1802.

The growth in number and also in range of specialist hospitals continued in the early 19th century, concentrating upon diseases and complaints frequently excluded from the general hospitals or not satisfactorily treated in them. By 1830 at least three additional lock hospitals had been opened (in Glasgow (1805), Newcastle (1813), and Manchester (1819).[29] The number of fever hospitals or fever wards had steadily increased. A new "Universal Dispensary for Children" had been founded in London in 1816.[30] In England and Wales there were nineteen functioning eye hospitals (five in London, where the first two opened in 1804-1805) and at least two in Scotland, and one in Ireland. Five were combined eye-and-ear institutions, and there was one exclusively ear hospital in London (1816).[31]

Although administratively these specialized institutions fitted the general model outlined above, there were differences between them and the other principal form of hospital, i.e. the all-purpose-hospitals such as the great London hospitals or the provincials infirmaries. As mentioned by Bynum:

"General hospitals tended to be rather completely dominated by lay governors; whereas the smaller, specialized institutions, … were ordinarily established through the exertions of, and subsequently dominated by, the doctor or doctors who staffed them."[32]

This also holds true for a number of dispensaries for out-patients, (see below). This development illustrates not only the above mentioned medicalization of hospitals, but also the scientific advances in British medicine at that time. As pointed out by Shryock, there were still many socio-cultural reasons which may have prevented doctors from using the new structures for research,[33] but English medical practice witnessed the emergence of vigorous schools of surgery, (Hunter), Pathology, (Baillie), dermatology (Willan and Bateman), and obstetrics, (Smellie and Charles White), deriving benefit from scientific inquiries instituted at the leading charities of the kingdom: indeed the new hospitals and dispensaries were staffed by some of the most enlightened physicians in the country.[34]

3. Medical Societies and Periodicals

Further evidence for the "scientification" of medicine and a characteristic feature of the time was the association of medical men with medical societies. Societies began to become common in Britain about the middle of the 18th century. They were founded both for providing better education than was given by the traditional privileged licensing bodies and for encouraging the communication and comparison of data, be they clinical cases or contribution to biological sciences. Edinburgh with its group of practitioners lead by Alexander Monro *primus* (1697-1767) and with its (students') Medical Society had set the example already in the early 1730s for both types of societies. This example was adopted in different parts of the Kingdom, helped not least by migrating Edinburgh students.[35] In London alone at least twelve voluntary associations of medical practitioners were formed from 1746 onwards in the 18th century – some ephemeral, some long lasting.[36] Their names characterise them somewhat: "Society of (Hospital) Physicians" (1752), "of Licenciate Physicians" d(1764), "Guy's Hospital Physical Society" (1771), "Medical Society of London"

(1773) or "Society for the Improvement of Medical Knowledge" (1782), - or "for the Improvement of Medical and Chirurgical Knowledge" (1783).

In the provinces Chaplin mentions societies at Colchester (1774), Plymouth (1794) and Leicester (1800),[37] but his list is incomplete.[From more recent literature I have discovered at least sixteen provincial societies active in England before 1830, a further four societies in Scotland and one in Northern Ireland.[38] I found in addition traces of formal activity of medical societies in Leeds[39] and in Dublin (see below, p.208).]

Again led by Edinburgh – where Alexander Monro's group started publishing proceedings – many of these societies made an effort to publish regular *Memoirs* or *Transactions*. This new type of publication was found convenient and in Britain soon superseded the customary methods of issuing printed pamphlets or making casual communications to the (unspecialised) Royal Society.[40] Before 1790, twelve periodical publications – some of ephemeral character – appeared in Britain. One was a review of relevant medical literature,[41] namely the *Medical and philosophical commentaries* (by a Society of Physicians of Edinburgh), London 1774-1795 (see below). Between 1791 and 1800 and from then till the end of the reign of George III (1820) seven new journals were started in each period. The *Lancet* appeared first in 1823.

The Edinburgh *Medical Essays and Observations* published in five subsequent volumes between 1733 and 1744, reached a fifth edition in 1771. They were praised and translated throughout Europe. The London Society of (Hospital) Physicians' *Medical Observations and Inquiries* (six volumes 1757-1784) saw itself as "a continuation of that valuable work" and the preface stated that it did not want to be liable to the objection of lack of specificity made against both the *Memoirs* of the Academy of Sciences in Paris and the *Philosophical Transactions* of the Royal Society.[42]

Even the privileged Royal College of Physicians of London, concerned through the 18th century more with political questions than with promoting scientific advance,[43] issued six volumes of *Medical Transactions* from 1768 to 1820. The Medical Society of London published six volumes of *Memoirs* from 1787 till 1805, and *Transactions* of *Proceedings* from 1810 onwards. After and important group of members had split from them in 1805 and formed the Medical and Chirurgical Society, this new body printed *Transactions* as from 1809. The Society for Improvement of Medical Knowledge and the *Lyceum Medicum*

Londinense were associated with the *London medical Journal* (1781-90), which continued till 1800 as *Medical Facts and Observations*. The Fordyce-Hunter Society issued three volumes of *Transactions* from 1793 to 1812.

These London Societies also welcomed communications from corresponding members and from provincial societies without their own outlet for publication. All their periodicals contained papers selected from those read at the meetings. The preface to the first volume usually stated the programme of the Society, stressing its methodological direction away from mere abstract reasoning, insisting instead on observation and the publication of precisely recorded facts only.

Thus from the general methodological, structural and socio-professional points of view, taken together, the pre-conditions were not unfavourable to the application of numbers to clinical problems. But before further examining the question in the following chapters, it is appropriate to describe shortly its specific background.

B. THE RISE OF QUANTIFICATION AND MEDICINE IN THE 17TH AND 18TH CENTURIES

1. STATISTICAL QUANTIFICATION DEMOGRAPHY AND VITAL STATISTICS

Broadly speaking, the application of measurement or numbers to medicine may be seen under three headings:

1) Measurements made in relation to a single individual or place (with possible later grouping of the results for comparison): This category includes results obtained by instruments and by chemical analysis of biological fluids or by similar means. During our period we may refer to it as “statistical quantification”.
2) Measurements or numerical statements relating to small groups of individual. This category would embrace the handling, in a scientific manner, of simple data resulting from simple observation or measurement relating to a number of cases. These are “medical statistics”.
3) Measurements or numerical statements relating to large groups of individuals or even to a population as a whole: This is the wide field of “vital statistics”.[44]

Our present concern clearly falls within the second category. It is however appropriate to sketch the development of the two other categories, which had much earlier beginnings, so that certain cross-links may be more easily appreciated. Both began to take shape in the 17th century when former attempts received theoretical and programmatic treatment. Shryock (1961) has surveyed this general evolution whereas Cassedy (1974), in a recent well-documented account, analysed the particularly important British scene with regard to the complex of health conditions, medical and scientific ideas, and social-political pressures of the seventeenth century. As Cassedy put it:

"The historical rise of statistics in England represents in part the incorporation of an element of the common man's medical knowledge into the formal medical corpus of the day. We must thus look mostly outside the organized medical profession for the early ideas and steps which led to an identifiable statistical approach to disease and health problems."[45]

Quantitative medical experimentation was conducted at Parma around 1600 by Galileo (1564-1642), Sanctorius (1561-1636) and others. Harvey (1578-1657) brought the Paduan techniques of physico-mechanical, or statical examinations of the human body to England. Descartes, as a physiologist, provided a model for mechanical medicine in his concept of the animal machine. From 1600 till about 1750 such iatro-mechanical, iatro-chemical and even iatro-mathematical research, theory and speculation was one important means of applying quantification to medicine, which produced largely descriptive data from laboratory experiments.[46]

Francis Bacon, who knew Harvey well, showed yet another potential for numbers in medicine in evocative but unfulfilled suggestions. Indeed, he was interested in the scientific aspects of demographic as well as strictly medical data. In his *History of life and death*, besides noting the need for inquiries into the mechanics of bodily functions, growth processes, and effects of diet, exercise, and medicine, he proposed quantitative studies such as: analysis of the life span, the causes of death, and of longevity; the size of families, racial origins, sexual composition and so on. The scientists of Solomon's House, that Utopian research institute, should thus conduct their own research as well as collect the experience of others. The House was also to have on its staff compilers and abstractors – shall we call them statisticians? – who were intended to condense all of these experiments in Titles and Tables to give the better light for the drawing of Observations and Axioms out of them…"[47]

This was perhaps the nearest approach even to “medical statistics” at this time – and indeed many subsequent writers were to refer to Bacon’s who aimed his lance at a wide variety of vulgar errors of his day, contributed to Bacon’s theoretical fundaments by glimpsing, but not grasping, some possibilities of medical demography and his concern with numbers esoteric and numbers banal.[48]

It was John Graunt (1620-1674), a tradesman who was sufficiently close to everyday vital events of the masses of men *and* to the practical systematic use of numbers (shop arithmetic!), who first took up Bacon’s proposals on demography. With his *Natural and political* observations made upon the bills of mortality (1662) he bridged the gap between the status-beaten world of the dead-searchers and administrative clerks responsible for the Bills for over a century, and business book-keeping and a “modern” learned world. This first great example of statistical methods used in scientific inquiry, fell into a no-man’s land between medical practice and medical research, between professional medical affairs and governmental affairs. Graunt’s work appeared with no close English precedents and little warning. It still provides valuable original data for historians’ work on that early “world we have lost”.[49]

This earliest scientific health report was heartily welcomed in the circle of the young Royal Society, as it fitted closely with the prevailing outlook of the members. “For these were *virtuosi* who self-consciously followed the Baconian injunction of counting, weighing and measuring;”[50] and who went on to apply elementary forms of statistical analysis to several aspects of medical inquiry during the remaining decades of the 17th century and later.

Sir William Petty (1623-1672) was an enthusiastic sponsor of Graunt. “Political arithmetic,” that is the collection and interpretation by very elementary processes of mass observations in the interest of the state or of a policy, got its name from him.[51] As one historian put it:

“If the scientific aspects of Graunt’s work, notably his formulation of a rough table of life expectancies, appealed to continental investigators of the stature of Huyghens [1629-1695] Petty’s concepts, under the broader banner of political arithmetic, shaped a whole generation of mercantilist statesmen and economists at home.”[52]

Petty also sketched out statistically based, Baconian health-related projects for an up-to-date *New history of life and death*. But besides an early evaluation of comparative hospital mortality on an international scale, Petty did not carry this on very far.[53]

The history of vital statistics from Graunt to the 1830s can be related concisely by Major Greenwood (1948). Both he and Cassedy stress the steady flow of data after 1700, the little methodological advance and the gradual carrying-out of Petty' projects such as differentiation of the causes of death. Whilst a first national census was held in Sweden in 1749, and other countries followed, a Census Bill did not pass the House of Lords in 1753 and the first British census took place in 1801 only.[54] But various medical writers conducted local censuses in the second half of the century (see below).

A number of historians draw attention to the use of vital statistics for medical reform throughout the 18th century[55] and in the rising social and preventive medicine during the 18th century. In both respects the role of statistics steadily, if slowly, expanded during the 18th century. As in the 17th century, this progress resulted "from the productive interplay of many kinds of people – the scientists, statesmen, mathematicians, clergymen, and other laymen, some quite ordinary, as well as the physicians".[56] By 1800

"….elementary statistical methods were practised widely and vital-statistical data, however imperfect, were, in comparison with those of Graunt's age, extensive. All the pioneers of Social Medicine based most of their arguments on statistical reasoning. Hardly any of them before Farr [1807-1883] had much knowledge of the calculus of probabilities, although La Place's treatise was published in 1812 and, long before, enough had been published to enable anybody who wished to do so, to use a good many of the tests for `significance` which are now mere routine.[57] [In their series *Pioneers of demography* Gregg International publishers have recently reprinted a number of classic and less known works of 18th century vital statisticians.]

In the next section I shall thus briefly discuss some quantitative aspects of social and preventive medicine.

2. SOCIAL AND PREVENTIVE MEDICINE

Rosen stated in his *History of public health:* "Problems of epidemic diseases and public health provided an important stimulus to a continuing interest in the numerical method."[58] This is easily illustrated, (apart from the development of general mortality statistics), in the evaluation of the merits of inoculation of small-pox: as several historians have pointed out, it was statistical-mathematical from its beginnings in the early 1720s and provided the subject matter for a heated controversy during the greater part of the 18th century.[59] It was in relation to this practice also that the first attempts were made to determine the value of a prophylactic measure by sophisticated mathematical means.[60] It is noteworthy that critical British doctors interested in the improvement of the bills of mortality such as William Black, Thomas Percival and John Haygarth would work also statistically on smallpox[61] (see below).

This is perhaps the best known, as well as one of the earliest, examples of "medical statistics" as opposed to "vital statistics" or "quantification by measurement", taking its origin in Britain. But some 18th century clinical studies of communal diseases, which demonstrate the work of medical practitioners in regard to the direct prevention of disease, provide still further illustration: Sir George Baker's (1722-1809) *Essay regarding the cause of the endemical colic of Devonshire* (1768) which examined the lead colic caused by cider prepared and stored in casks or vats containing leaden linings or weights, is a "most outstanding example of organised research, a paper remarkable for its modernity of construction, and for its conformity to all the rules of logical presentation".[62] It was chiefly based on a privately kept record of the Devon and Exeter Hospital for the five years September 1762-July 1767, and it demonstrated the mode of investigation of all such poisonings. The success of the method of prevention was shown by means of official hospital records kept in Exeter and in Bath.[63] [The reactions to Baker's work and the history of the decline of endemic lead-colic in the late 18th and early 19th centuries have been quite extensively studied.[64]]

Another example was the prevention of puerperal sepsis by cleanliness and ventilation, inaugurated by Charles White (1728-1813) from Manchester. Some verification of success may indeed be expected after such radical changes of old habits (as many preventive measures are): "In the nurture and management of infants as well as in the (preventive) treatment of lying-in women" wrote Lettsom (1744-1815), the celebrated Quaker physician in 1774, one year after the publication of White's epochal book on obstetrics "the reformation hath equalled that in the smallpox; by these two circumstances alone incredible numbers have been rescued from their grave".[65]

Using statistical data to advance medical reform was one thing – using them for medical research was yet another. Medical reform could be, and was often, urged by laymen as well as by physicians. As a research tool, however, medical statistics had to vie with the other methodologies of the day which were broadly speaking, empiricism, study of the classics, observation and description, and laboratory experiment. Empiricism and speculation theories, laboratory experiments and their iatro-chemical interpretations supplied rationales for internal therapies during the 18th century, e.g. for lithiasis,[66] scurvy and certain types of fevers (see below). By the 1690s iatro-mathematics had become the fashionable method in some medical circles, and to some extent drew attention away from statistics. With it the whole array of 17th century quantitative studies in physiology, chemistry and related areas came together. Through the Leyden School it became especially influential in Scotland during the first half of the 18th century.

3. THE WEATHER-DISEASE RELATIONSHIP

However some of these iatro-mathematicians became interested in statistics, too.[67] A unique integration of statics with observation and description that eventually turned out to be of highly statistical nature, occurred in the revival of the old medical inquiry into the relationship between weather and disease. It entailed the marriage of the methodology of the "New Science" – the passion for investigation through ordering, numbering, observation and quantification- with the strong neo-Hippocratic interest in environment. The re-opening of the question is associated with Boyle (1627-1691) and Sydenham (1624-1689), who both skirted statistics *per se*. Yet they might be called proto-statisticians as their work touched on medical statistics (Boyle's advocacy of quantification resting on his own experimentation) or contributed to the common objective of discipline and order in medicine. Indeed, "Boyle's hydrostatic testings of items of the *material medica* was a deliberate effort to bring greater exactitude of number, weight, and amount to bear upon the procedures of the medical profession".[68]

Sydenham, who was no quantifier of mechanical or chemical phenomena (not even a believer in experiment *per se*) nonetheless helped bring about greater systematisation of medicine. Like Boyle's, his contribution was ambivalent; subsequent medical statistical writers often referred to him for his urge for classification of disease (in order to have uniformity of treatment and discussion), for scrupulous and detailed observation, and for keeping careful case histories. He himself did not undertake classification, nor convert the systematically collected facts of his own practice into statistics and he left his conclusions as general impressions without precise numbers of patients and diseases.[69] As part of his Hippocratic outlook, Sydenham maintained that study of epidemic diseases required close observation of the weather – a study whose potential Boyle greatly improved by his experiments with barometers and other instruments. This problem attracted a broad segment of the British scientific community from the late 17th century onwards. It remained important in Europe – especially in France and Vienna – and overseas throughout the 18th and early 19th centuries.[70]

John Locke started a project of collecting world-wide information about diseases and weather. This idea was later implemented on a national scale in an immense summary planned and executed by Vicq d'Azyr (1748-1794) and the Société Royale de Médecine (1774-1794).[71] Headed by the *inspecteur général* Jean Colombier (1736-1789), the French military doctors emulated them and projects were also carried out by the first Vienna School.[72] The Paris project was stopped only in 1826 and the analysis of the reports had to be abandoned in 1833.[73] Yet with the conquest of new colonies in Africa, "medical topographies" again played and important part in contemporary medical writings, just as the new concepts of statistics had taken shape with England's emerging colonial impulse. The two developments seem to have stimulated each other.

Practical instruments to measure the weather, (rain gauge, thermometer, barometer, wind recorder, hygroscope) in the hands of Wren (1632-1723) and Hooke (1635-1702), eventually put meteorology on a statistical basis, with tabulation of results, calculation and publication of averages. Analysis of these statistics in conjunction with observed disease phenomena was expected to elucidate causes and origins of epidemic diseases. As already recognised by Wren, the two sets of data would be medically useful only if they could be correlated with the hard facts of the bills of mortality, a thing which was usually not done. [74]

There were doubts on the value of this work right from the beginning. Sydenham admitted that he had never been successful in correlating his own data. But the conviction persisted in spite of inconclusive results and critical comments, e.g. by Thomas Short (1690?-1772), the vital statistician in 1767, and by travelling doctors.[75]

It is notable that James Jurin (1684-1750), who tried to produce the first mathematical proofs of the value of inoculation in the 1720s,[76] at the same time and from the same podium i.e., the Royal Society, revived the interest in the disease-weather relationship when the initial 17th century impetus had somehow decreased. [Jurin was trained as physician at Leyden, possibly by Boerheave himself. He was also an accomplished mathematician; actually a pupil of Newton.[77]] As from 1723 the habit was, if anything, reinforced by the publication of weather conditions in Edinburgh from 1731-1735 in the influential *Medical Essays and Observations* (1733-1744, and reprinted unchanged in a fifth edition in 1772). This was an essential part in the plan of this new publication.[78]

Besides the investigations into environmental phenomena and the role of statistics in public health, but a few 17th century physicians did attempt to adapt the method to clinical medicine. Most of them were looking for absolute medical truth and did not see that there could be a use for probabilities or consensuses. Trust was certified by testimony of witnesses, a technique still found in many 18th century writings. Neither the statistical nor the statical minded physician found it easy to break through the encrusted ways and ideas of the classicist's traditional medicine. "Both sought to substitute factual realism for theoretical romance in a profession which, by and large, still thought of such factualism as and attribute of clerks rather than physicians."[79]

Yet this thesis proposes to enquire into the use of statistics as a tool in clinical medicine, chiefly in the limited field of therapeutic research. It will concentrate on the period after 1750 when the value of much received medical doctrines was doubted and scientific methodology was eagerly debated in Britain.

C. THE POSSIBILITIES OF THE "OBSERVATIONIST" DOCTOR

1. THE NUMBER OF OBSERVATIONS

Whatever forms the doubt concerning traditional medical theory took[80] "Observation" made in the real, rather than deduced from the supernatural world, became the key word. For the description of phenomena, the *quality* of observation was decisive. Yet with time there was also a certain trend to lay weight on the *quantity* of observations in order to "increase truth", or to "ascertain" it, to use contemporary wording. This represented a shift away from the search for absolute truth to the acceptance of probabilities and consensuses. John Gregory (1724-1773) taught his Edinburgh students:

"The advancement of the sciences … requires only an attention to probabilities, to leading principles… a quick discernment where the greatest probability of success lies, and habits of acting in consequence of this with facility and vigour."[81]

This fact is also illustrated in *Table 1* which summarizes the results of a rough attempt to analyse quantitatively the contents of four successive important British medical periodicals: The *Edinburgh medical Essays and Observations*, (1733-1744) the *London Medical observations and Inquiries* (1757-1784) the *Memoirs of the Medical Society of London* (1787-1805) and the *Medico-Chirurgical Transactions* published by the Medical and Chirurgical Society of London (1809-1829).

For analysis I grouped the articles according to contents into two categories:

1) Clinical observations (including occasional autopsies)
2) Purely qualitative descriptions of phenomena, e.g. of climate, chemical substances, plants, diseases without details of particular cases, as well as anatomo-physiological or chemical experiments, and miscellany such as programmatic articles.

In most cases one can allocate the articles unequivocally to one of these categories. In rare cases of overlapping my choice had to be arbitrary, but I do not believe this affected the results generally. The "clinical observations" were further subdivided according to whether the argument was based upon one, two, or more cases which the author has actually witnessed or had been reliably informed about. Short extracts were made of the papers falling into the last group.

The distributions of the two main categories within the four journals could, of course, be compared only with great caution and many reservations. Yet in one category, namely "clinical observations", valuable conclusions may be drawn if similar editorial policies are assumed. It also contains a sufficient number of entries in each journal.

Of the 128 clinical-pathological observations published in the Edinburgh Essays, 113 (88%) were based on one case, and only eleven (8.5%) on more than two, and the number of cases examined was never high.[Three papers of 3 cases each, two of 4,5 and 8 each, one of 6 and 14 cases each.]

The later London *Observations* contained 208 clinical and pathological papers. The proportion of single-case papers decreased slightly to 78% whereas that of papers based on two or more increased to 8% and 14% respectively. The number of observations in the latter group was also much greater than in the preceding series of *Essays*. [Three papers of 3 cases each, two of 4 and 6 each, 4 of 7 each, one of 8,9.13,17,20,35 and 130 cases each. 4 papers contained statistical calculations based on bills of inoculations or mortality.] Their occurrence was equally distributed in time.

The first volume contained a table listing 130 cases, noted between 1744/5 and 1754/5, in which Ipecacuanha was given in small doses to test its value as an emetic. It gave date, name, age, dose of the drug, times of vomiting and of stool. The drug had failed in only 15 cases so that the author, Samuel Pye (†1772), could safely conclude that it was effective for all ages, in both sexes and at any stage of any disease.[82] In the third volume Alexander Russel, (1715-1768) from St. Thomas's Hospital reported all his 17 cases in which he had used a new drug in venereal nodes from 1764-1766. There was only on unsuccessful case, which he felt duty-bound to mention.[83] The fourth volume contained Charles White's table on amputation (see p.??).

Several papers were published concerning the use of sublimate for the cure of syphilis. This was especially a domain of military surgeons. John Pringle (see below) collected by correspondence with six of them first in winter 1756/57 and then a year later in order to assure the duration of the cures, results stated in the following terms: There were "35 cases without failure nor relapse"; "approximately 60" with two failures; "35 new cured without relapse except two; seven and eight cases without relapse and also "many", and "eight or ten" cases.[84] The Army, no doubt, was a source of recorded mass-observations.

The *Memoirs* of the London Medical Society included 202 clinical-pathological papers. Their proportional distribution among the three subgroups also barely differed from the London *observations*. There were a number of therapeutic-statistical papers on random series of hospital patients by Lettsom, Falconer and James Currie, which will be analysed in following chapters.

The trend to increase the number of observations before publishing a new argument shows a gradual shift away from the single case, especially as a basis for therapeutical practice. This had been apparently a desideratum of Sydenham,[85] and surely of Clifton and Alexander Monro *primus* (see below). Gregory, in his lectures on the duties and qualifications of a physician, taught that hitherto the emphasis on single cases had been one of the chief hindrances to scientific advance.[86]

The trend appears even more patent in the second and third decades of the 19th century as viewed through the contents of the *Medico-chirurgical Transactions*. The single-observation papers decreased to 67% and 56% of the clinical cases respectively, and the papers based on more than two observations increased to 23% and 31%. They included statistical analyses by a number of military surgeons and hospital doctors to be discussed in later appropriate chapters.

This summary, whilst showing a clear tendency to broaden the basis of judgement by increasing the number of observations before publishing an opinion does not imply that there was yet any numerical analysis of these more numerous facts, either for nosography or in therapeutic trials. But at least as far as therapy was concerned, one might expect some numerical analysis.

There grew form the 1770s onwards, a consciousness that observations, even numerous observations, were not sufficient, but that what one really wanted was to subdue them to the test of "arithmetic".[See below (p.??) for the introduction of the word "statistics.] This may be illustrated in a clash in London around 1780 between two Scots, Donald Monro and John Millar, a clash which also shows many of the social and institutional issues involved in the rise of clinical statistics.

2. The Analysis of Observations: The Monro-Millar Dispute

Donald Monro (1728-1802) was the eldest son of Alexander Monro *primus*, the first professor of anatomy and physiology of the Edinburgh medical school. After his training there, Monro *fils* settled in London. He became physician to St. George's Hospital in 1758. From the end of 1760 to 1763 he was temporarily commissioned as (chief) physician to the British military hospital in Germany. His *Account* thereon (1764) became a classic and was translated into French[87]. It contained no precise indications of his success in, day, "fevers" of dysentery, although the keeping of records was one of the duties of his subordinates, the hospital mates.[88] These records had included name, date of entry and exit, diagnosis prescriptions and the event (cure or death). Monro had relied in the first instance on bleeding and blistering in these cases, as recommended by authorities such as Huxham, Pringle and Lind. If this had not helped, he had given Peruvian bark "to above a hundred and fifty" during his attendance in Germany. "Although it did not answer in every case, yet it was found to have better effect than any other remedy that was tried."[89] Selected case-histories had illustrated the general success of these practices.

This "success" was questioned by John Millar (1733-1805) in 1777. He, too, had been an Edinburgh pupil of Alexander Monro, but once in London did not move in the same circles as Monro Junior. *Munk's Roll* does not mention him as having been even an LRCP. He had no traditional hospital attachment, but he became the first physician to the Westminster Dispensary when it opened in 1774. In this year he was also the first president of the Medical Society of London.[90] In a report containing fully detailed, tabulated data of the practice at his Dispensary for all diagnoses, Millar vehemently accused the Army physicians and, in particular D. Monro, of statements inconsistent with the returns as available at the War Office. Because they showed a general mortality in the British Military Hospital of more than 50% [91]Millar concluded:

"Great deference is due to these authors, to the application of their preventive outlines…but…no reasonable deference to the opinions of the most eminent writers…., no consideration [of any sort] could justify the suppression of an enquiry concerning a matter of so great an importance: upon a whole, it must be allowed, that diseases have not in general been treated with success."

This statement appeared quite correct in consideration of Millar's own overall mortality rate at the Dispensary of 1 in 110.[92]

To such criticism the distinguished Monro (FRS. 1766, and Fellow of the Royal College of Physicians *speciali gratia* 1771) could not remain silent. In a postscript to the second enlarged edition of his book (1780) he claimed that the hospital returns from Germany published by

Millar, were “fictitious, and perhaps the most unfair and disingenuous that ever blotted paper”. He proceeded to show their incomparability with those from the Westminster Dispensary. This charity had a selective admission policy, whereas an Army physician had to take any soldier into his returns.[93]

The two editions of Monro’s book illustrated the general trend to enlarge, and at the same time to render more objective clinical evidence already shown in my analysis of the four journals. This trend can be traced in the work which Donald Monro did in between. During his London practice in the 1760s he reported on ten cases of aneurysm from his ward at St. George’s Hospital. In another paper ‘On the use of mercury in convulsive diseases’ he quoted two military surgeons: one had saved only one case of tetanus out of more than forty. The other, who had given mercury, had cured all his thirteen cases to whom he had been able to prescribe it early, and claimed to have had only a “few” deaths when exhibiting it later.[94] Finally when Monro was recalled to active service in the Army during the American War, somehow forced by Millar (1777, 1778) as it would appear, he took pains to collect some returns. He inserted in the second edition of his book in a still unsystematic way, the number of sick troops at different periods in the general hospital, which he directed in 1778-1779, and in regimental hospitals, broken-down according to diagnosis. However one could not determine precisely the success of his practice for the controversial disease of “fever”, since the number of feverish patients was admittedly only estimated in these same hospitals for the epidemic in autumn, although for the healthy summer period a list was subjoined.[95] Monro wrote further: “ I wished in this new edition…[also] to have given the regular hospital returns of the hospitals I attended in Germany which would have shown the exact number of sick…., and the number who died”. But he had only “some memorandum accidentally preserved”.

Unable, or unwilling, to adduce the contested returns in his defence against Millar’s accusations, Monro pretended that they were non-existent at the War Office, having “lately” been destroyed by the *ad hoc* inspector of hospitals. He even went as far as to allege that Millar had forged the figures concerning his British military hospital in Germany.[96] Millar promptly retorted that Monro had not looked for the returns in the right place. He added in his privately printed Reply (1783) that Monro had overestimated the incidence of fever for the autumn 1779, too, in order to make mortality from it appear small,[97] -besides the fact that there were still 52 feverish patients left in hospital at the time of the writing of Monro’s book.

The truth may have lain somewhere in between, since the official mortality figures for 1761 were doubtful: in his *History of the Army Medical Department* (1974) Cantlie writes that "General Conway [1721-1795]… at headquarters reported dreadful confusion in the hospital statistics over a period of two years". He had therefore in 1760 and 1761 respectively an officer appointed "to take charge of all hospital returns… and to put some degree of accuracy into the work of the hospital clerks".[98]

3. The "Observationist" and the "Arithmetic Observationist" Doctor

The Monro-Millar dispute shows how strict verification of empirical results led to numerically stated observations and eventually to their rational analysis with the help of arithmetics. Indeed, rational empiricism at its best. William Black stated frankly in 1789:

"The great utility of medical arithmetic was an accidental discovery, at least to me, about eight years ago: for in the course of many preceding years attendance on medical lectures, at different universities, I never once heard the subject mentioned. I then found in London a violent literary warfare, respecting the advantages and disadvantages of general inoculation."[99]

(Black intervened with the use of all available statistical sources in favour of it in his *Observations…on smallpox* in 1781). He attempted, as stated in the under-title of this book, "to demonstrate in what manner London may save near two thousand, Great Britain and Ireland between twenty and thirty thousand, and Europe about one hundred and ninety thousand lives annually".

Let us now come back to the Monro-Millar dispute. This was certainly partly a personal issue between the two men. But there was also a methodological issue, and it is for this that this quarrel serves to distinguish two ideal types of doctors using different approaches in observationist medicine.

Donald Monro may be said to represent the pure observationist clinician becoming more frequent perhaps after 1760, when the restricting and unsettling influence of the various "systems" ceased to hamper the thoughts of physicians. But as yet the fruits of the study of morbid anatomy, [Morgagni's *De sedibus*….(1761) appeared in English in 1769, and

Matthew Baillie's *Morbid Anatomy* in 1793.] physiology and chemistry were not within their reach at first, using such means as they possessed, they became remarkably acute observers, and among them reckoned some of the greatest clinicians Britain has produced.[100] Many of their observations of facts filled the pages of the medical periodicals. Some authors recorded them in a new type of book such as *Medical facts and experiments* (e.g. by Francis Home, a colleague of Alexander Monro *primus*) (1759), *Medical cases* (e.g. selected form the Public Dispensary at Edinburgh, by Andrew Duncan) (2nd ed. 1781), or *Cases in surgery* (e.g. 44 successful operations done at Guy's Hospital, London, by Joseph Warner) (1754) and *Cases and practical remarks in surgery* (e.g. drawn from the Shottesham Infirmary, by Robert Gooch) (1758). For these authors the presentation of facts was the primary aim, any numerical analysis of them being secondary, nearly accidental, yet as our summary view of four medical journals and the particular example of Donald Monro show, they tended to quote and increasing number of observations as a basis for defining a new disease-phenomenon. William Heberden the elder based his famous account of angina pectoris (1772), one of the outstanding clinical contributions of the time, on twenty cases. From his posthumously edited *Commentaries* (1802) one can see that by 1782 he had recorded "nearly 100" cases.[101] (Besides, Heberden took an interest in vital statistics. He financed and wrote the preface to a quarto volume containing a collection of the yearly Bills of Mortality in London from 1657 to 1758[102]).

Monro's opponent John Millar equally pleaded for the supremacy of observation and recording over speculation as the only means for specifying new ontological entities, but in addition he willingly stressed the need of numerical analysis of these records to give them their true meaning. "Detached cases", he wrote in 1777, "however numerous and well attested are insufficient to support general conclusions" (see below). And he specified further that "the test of arithmetical calculation [ought not to be] evaded."[103] Therefore Millar may be termed and "arithmetic observationist" clinician.

Indeed, in 1783 he defended himself against Monro's accusations of faking returns, by asserting that "the stubborn evidence of arithmetical demonstration could not be shaken by argument." By then numerical reporting had been deliberately used so that Millar was able to quote them freely when drawing up a tabular view of "the comparative success of different methods of treating fevers."[104]

I shall use the distinction between observational and arithmetic observationist clinicians in the following chapters aware that it refers to ideal, exceptional types. But I shall describe a number of authors, who, even if they did not themselves use the term “arithmetic observationist” would have recognized themselves as belonging to a group who, taking the Baconian method of observation and inductive reasoning for granted, defended and used the application of arithmetic as the next step to gain certainty in medicine.

In therapeutics the arithmetic observationists’ *test* would involve mathematically the formation of sums, the calculation of averages (i.e. arithmetic means) and at highest, that of ratios, (e.g. success-to-failure ratios). In nosography the occurrence of certain symptoms would be expressed as a fraction of the number of cases studied. The arithmetic observationists would rely on the comparison of results numerically expressed with these simple means, if possible in tabular form, as they realised that this was the sole possibility fo fulfil, in a succinct form, the condition of including all cases having occurred during a given time. Thus William Black wrote in 1789:

“What tribunal can possibly decide truth in this clash of contradictory assertions and conjectures; or by what clue can medical wanderers find their way through the labyrinth of prognostics and therapeutics, except by medical arithmetic and numbers?… Perhaps some would here answer, the best authors should decide the controversy. Who are they, ancient or modern….? To borrow Molière’s satirical expression, Hippocrates often says Yes, and Galen flatly No. The system of medical arithmetic, although it may not show the best mode of therapy that may here after be invented, it will, however, by comparison, determine the best that has yet been discovered, or in use.”[105]

D. The Organisation Of the Army and Navy medical services

1. The Army

Even in the eyes of a contemporary, military medicine greatly contributed, from about the middle of the 18th century, to the gradual rise of a new approach to medicine,

“which, by dissevering all connection with the science of abstract quantity, and allowing medicine to rest on observation and experience alone, in the hands of Huxham (1738-48), George Cleghorn (1751), Sir John Pringle (1752), Donald Monro (1764), Francis Home (1759), Brocklesby 81764), the two Linds (1763-68) [James Lind "the father of nautical medicine" (see chapter four) and his homonym James Lind FRS (1736-1812) author of *A treatise on the putrid and remitting marsh fever which raged at Bengal in the year 1762* (ed. Elliot, 1776)] and Sims (1773) threw into the shade all other means of acquiring medical knowledge and regulating medical practice."[106]

This quotation is by John Thomson (1765-1846), an affectionate pupil of William Cullen (1710-1790) and his biographer, who was to become the first British professor of military surgery in Edinburgh in 1806. (see below). Only one of the nine personalities he mentioned, i.e. Sims, was not linked with military medicine and the six Scots among them had all studied in Edinburgh, the three non-Scots (Huxham, Brocklesby and Sims) at Leyden.

This is of course an arbitrary list, but it justified us in investigating whether the leading military doctors shared the opinion held by John Pringle, "the father of military medicine" and thus expressed by another friend of Cullen in 1771:

"[He] thinks the properties of diseases to be such as render them incapable of those methodical and strict arrangements which are applicable to plants; and the modern nosology, in consequence, fanciful and useless; and not only so, but hurtful also, by fixing the mind on the circumstances of collocation merely, and detracting it from more accurate investigations into what is in general so little known, the thing itself to be placed."[107]

Thus although not entirely free from conjecturing,[108] Pringle was interested chiefly in practical, tangible achievements, and for a variety of other reasons military medicine was potentially favourable for the application of statistics. There was, above all, the mass of data. Then it may be remembered that in the military context of the 18th century ordinary soldiers were regarded rather impersonally, and could therefore be represented by impersonal numbers. In contrast, the 18th century officer and private patients, attended by his own private physician, were considered in the Hippocratic-Galenic tradition as a very individual case indeed. Military doctors advocated their hygienic measures to the government as much for saving money for the State's sake, as for saving human lives in themselves. The framework of administrative and reporting regulations was potentially favourable. It would therefore be appropriate to outline briefly the development of the sanitary organisation of the Army and navy in the early 18th and early 19th centuries.

With regard to the organisation of the army medical service, France was by far the most advanced country by the 1790s. A standing sanitary corps for war and peace was created under Louis XIV in 1708 (200 sanitary officers, 51 fixed Hospitals all over the country).[109]

The first sanitary regulation of 1718 enjoined the hospital physicians and surgeons to collect useful observations, especially on epidemic, contagious or extraordinary diseases, these data to be sent to the Secretary of War after verification by the inspector of the military hospitals. In 1763 a regular reporting system between the hospitals and the inspector was to be installed and the results of this correspondence to be published. In fact two volumes of *Observations de médecine des hôpiteaux militaires* came out in 1766 and 1771. In 1780 questionnaires on medical topography were sent out and an active correspondence was ordered – (under threat of dismissal) – between the hospitals and their general inspector in Paris. The latter was to make a choice of the most valuable contributions for regular publication. *A Journal de médecine militaire* appeared indeed from 1782 to 1789, being then interrupted by the outbreak of the French Revolution.[110]

Prussia, too, had a fixed sanitary corps since 1704 and both Frederick William I (1688-1740) (the soldier's King who increased his standing army from about 30,000 to about 80,000 men), and his son Frederick II, the Great, (1712-1786) (who used it) paid careful attention to military medicine, especially to surgery.[111]

There was (just as in Austria where a sanitary corps was created under Joseph II(1741-1790)) a system of regular administrative returns and sick lists but no obligation or directions as to their use for scientific purposes (see below). This was left to the personal initiative of the individual surgeons or physicians. Such was inevitably also the case in the British Army, for its sanitary service was organised by the state only after 1796. Prior to this date, it was left chiefly to the commanding officers and regimental surgeons and there was hardly any functioning central organisation.[112]

In peace time the medical care was centred in the regiment, regimental infirmaries being rather *ad hoc*, since many of the troops were billeted in private dwellings. The regimental surgeons usually bought their commissions and thus did not have to pass a qualifying examination.

Furthermore, they were rather isolated from their colleagues, except in the summer months, when there were combined encampments of regiments for exercises. In 1762 there were only two permanent barracks in the country, which had, accordingly, also permanent infirmaries. As from 1781 the first permanent military hospitals of the Army were established at the ports, where the troops embarked and where invalids from overseas were landed. Some of them were temporarily closed at the beginning of the 19th century but reopened again after 1810, when an effective central Medical Department was instituted. In 1812 there were six such hospitals at Chelsea, Plymouth and Gosport, on the Isle of Wight, at Bognor (Selsey) and at Deal.[113]

The Army Medical Board of Controul [sic] (a physician general, a surgeon general, the physicians to military hospitals, the principal surgeons of hospitals and the purveyor general), created in 1756 as means of central co-ordination, virtually ceased to exist after the outbreak of the Seven Years' War. Until its revival in 1793, the physician – and surgeon-general directed their branches independently and besides had private practices. [However John Hunter held the combined posts of surgeon-general and inspector-general of regimental infirmaries for three years until his death in 1793.] From 1794 till 1815 the Board was several times reorganised, resulting in 1810 in a standing organisation headed by one responsible director-general, assisted by two principal inspectors, all engaged full-time.[114]

At the outset of the wars with revolutionary France in 1793, the regimental (or front-line) infirmaries were still under the colonels of their units, who ran them as they wished. The central Army Medical Board, recreated in 1793, could not interfere, and co-ordination was often difficult. Its members took collective responsibility only for the fitting out of the general rearguard and the "marching" supportive hospitals for expeditions overseas, the selection of staff and the supply of medicines. Yet once off shore the principal medical officer had complete and independent control. Finally in 1796 the Government agreed to provide medicine both at home and overseas through the apothecary-general and also to pay for the running of all hospitals (regimental and general). This marked the beginning of a state-organised Army medical service; the Board followed this in 1798 by new 'regulations to regimental surgeons for the better management of the Sick.'[115]

Little could be expected in terms of statistics from the regimental infirmaries under these conditions. Nevertheless individual achievements can occasionally be quoted. And, there had been returns from the general hospitals, at least during the Seven Years' War as illustrated above in the Monro-Millar dispute (p.??). This issue shows, however, that prestigious physicians considered them as belonging to the clerk's and administrator's domain rather than as a possible means of advancing once peace was resumed; Cantlie remarks that in none of the wars of the 18th century "do any official reports appear to exist which describe the activities of the medical service."[116]

Because of the loose sanitary organisation in the British Army, the doctor's duties were not precisely regulated[117] until 1798 nor appended to text-books as they were in Germany and France. Experienced surgeons such as Brocklesby (1722-1797) and Hamilton (1749-1830) published them in the form of mere recommendations in 1764 and 1787 respectively. The latter was yet another Edinburgh trained Army surgeon who set up practice in London. With reference to John Millar, he thought that a regular journal of every case should be considered as a necessary part of a surgeon's duty.[118] They should be handed in to the physician or surgeon general for examination whose salaries, he claim, were sufficient to recompense them "for this doubtlessly additional trouble." Such a control would raise the professional standards of Army medicine by excluding the bad elements from the service. And, such strict observation would prevent surgeons from going on "in the same thoughtless routine, tread[ing] the same beaten track of bleeding, blistering, vomiting and purging indiscriminately from habit, more than from reason… whether it is likely to do good or harm."[119]

This method of observation and note-taking, Hamilton further claimed, would also help to distinguish the effects of medicines from the symptoms belonging to the natural course of a disease itself. He envisaged numerous trials, or experiments, always under the same conditions and comparing subjects as similar to each other as possible. As examples he stated Fowler's and Withering's trials of arsenic in intermittent fevers (see below p.??). He recognized however that a purely expectant therapy consisting merely of cleanliness had succeeded in 85 children seizes by such a fever in an Edinburgh charity: Medical practitioners were as often apt to err by doing too much, as by doing too little.[120]

In fact, when in 1786 both the physician general and the surgeon general changed, after having held their positions for 40 and 38 years (!) respectively, their successors ordered half-yearly returns (it was peacetime!) of the sick with remarks and observations on diseases and methods of cure.[121]

The new, but still not full-time, physician general, Sir Clifton Wintringham, (1710-1794), a colleague of Pringle form Flanders, and his surgical colleague, Robert Adair (t1790) were both nearly 80 years old when appointed.

It is doubtful whether the first holder of the new post of deputy surgeon general, John Hunter (1728-1793), then at the height of his civil career (and who did the work of Adair who was then ill), ever received many such returns. The post was not very onerous in peace-time when there were only 133 medical officers in the service.[122]The returns were anyway never centrally analysed or officially published, allegedly because of a change of government policy. This was true also for the time when Hunter was surgeon general (1790-1793).[123]

In 1795 an independent Army Medical Board was created in Ireland, on the 1793 British model. The unsettled state of the country (French interventions, rebellions), made it necessary to maintain a force of up to 50,000 men on the Island, which resulted in the gradual establishment of fourteen small general hospitals throughout the country from Belfast to Cork. The Board under its director, George Renny (t1847), who held the post from 1795 till 1847 – set up rules and regulation for regimental surgeons and mates and introduced a system of accurate medical reports which included a list of the prevailing diseases. This was a system which proved highly informative so that the Commander-in-Chief, the Duke of York (1763-1827), ordered its adoption in England. In fact, the first regulations of the English Board in 1798 also demanded daily registers of the sick, including details of their treatment. It issued special forms, particularly for periodical numerical abstracts (see below). In 1800 and 1806 such regulations were issued for the general and regimental hospitals respectively.[124] However the Board did not have the power, before 1810, to enforce this new duty by the individual regimental surgeon. This depended entirely on the whim of their medical superiors and on the military commander's readiness to support them, a situation which persisted until James McGrigor's (1771-1858) appointment as director general of a strengthened Army Medical Department in 1815.

A separate but very well organised medical organisation within the Army was the Medical Department of the Ordnance i.e. of the Royal Artillery. It came into existence in 1797 and comprised two senior posts, both held after 1804 by John Rollo (†1809). He adopted the 1798 regulations concerning the reporting of cases with a clear view to their use for medical improvement. He propagated them, since he made his new model hospital at Woolwich and the other larger stations in Britain (Chatham, Plymouth) teaching hospitals.[125]

2. The Navy

The development of the naval medical service was similar to that of the Army.

From 1702 and throughout the 18th century it was under the control of the Sick and Hurt Board of the Navy. This was composed of medical and lay members varying in number according to war requirements.[126] In 1795 the old Board was reconstructed under the name of the Commissioners of Sick and Wounded, composed of two physicians and one non-medical member. To these Lord Spencer (1758-1834), the First Lord of the Admiralty, delegated the direction of all medical matters in the Navy. As one of the physicians soon retired, his colleague, Gilbert Blane succeeded him alone, and for the first time a medical man was responsible for the Naval Medical Service. This remained so after Blane's retirement in 1802, when the Service had been thoroughly regenerated abuses modified or removed, and the way was left clear for progress on prudent lines by equally able successors.[127]

What would correspond to a permanent general hospital in the Army in peace-time was instituted much earlier in the Navy. The sick were boarded in private lodging houses and/or special naval beds contracted with civilian hospitals scattered throughout the country. The cure of the patients was undertaken by a surgeon-agent in contractual capacity with the Sick and Hurt Board. For administrative and medical reasons the Board tended after 1739, when the naval wars began, towards the introduction of its own naval hospitals. The problems were overcrowding at the major ports, bad nursing and desertions. The latter was the strongest argument used by the Sick and Hurt Board before the Government.[128]

The decision to establish such hospitals in Portsmouth, Plymouth and Chatham was taken finally in 1744. (Greenwich Palace, acquired in 1694 as an accommodation for the old and infirm seamen, is not to be looked upon as a hospital in the clinical sense). Haslar Hospital in

Portsmouth received its first patients in 1754 and was completed in 1762. At Deal, Woolwich and Yarmouth the contract system continued. By the end of the Napoleonic Wars, every overseas base had a hospital of some sort. In the out-stations particularly, there was also collaboration between the Army and the Navy.[129]

Both Haslar and Portsmouth Hospitals offered *a priori* large facilities for clinical research. Haslar had, in 1755, room for 800 patients, and by 1780 for 2000. In Plymouth there were 1250 patients by 1795.[130] The use James Lind (1716-1794), the chief physician of Haslar from 1758 to 1783,[131] made of these opportunities as reflected in his investigations of scurvy and fevers will be discussed in the chapter on naval medicine.

Medical returns from ships had the same fate as those in the Army from foreign expeditions, until the right man came into the responsible position. The duties of the naval surgeon, laid down by the Admiralty in the first printed regulations for the cure of sick on board and the conduct of surgeons in 1731 (and valid until 1808), included indeed the keeping of a day-book from which two journals were to be composed –

"The one of his physical practice in diseases; the other of his chirurgical operations and, at the end of the voyage, to deliver the first to the physician of the Commission of sick and wounded or [if there be none, i.e. in peace-time] to the physician of Greenwich Hospital, and the latter to the governor of the Surgeons'Company who are to examine the same and certify their judgement thereupon."

When a patient was sent ashore to sick-quarters or hospital the surgeon was required to send a written account of his case along with him.[132]

Yet two facts are interesting in this context. Firstly, none of these surgical journals nor of the physical journals were deemed by the 18th century authorities of sufficient importance to be preserved, so that the earliest (though not regular until 1809) surviving journals in the Public Record Office date only from 1793.[133] That was the time when Robert Robertson (1742-1829), keen on records and statistics throughout his long career as active naval surgeon on sea (1760-1789), became senior physician to Greenwich Hospital (1790-1807) and when Blane had joined the Sick and Hurt Board (1795).

Secondly, as in the Army, the recognition and utilisation of their unique opportunity for, in Lind's words, "inspecting Nature, and examining diseases under the varied influence of different climates, seasons and soils"[134] was left to the surgeon's private initiative: despite the occasionally enforced obligation to record, there were no official guidelines for any analysis of the material until the end of the century. Similarly, some of the scarce clinical innovations were undertaken by the Sick and Hurt Board only as a result of the advice of its subordinates, like Lind, and not at the instigations of the Board itself. With the exception of Blane, its commissioners were a singularly undistinguished lot throughout the 18th century.[135]

In 1806, under his successor, the new 'Regulations and Instructions relating to His Majesty's Service at sea' enlarged the duties of the naval surgeons to report cases and was designed to obtain regular information on the results of their practice and the state of health of the men (see below).

Again as in the Army, all the great naval medical men of the 18th century were thus individualists motivated by humanity and by love for observations of fact, thereby emerging from an unknown mass of surgeons who "… held warrant rank only and were often known for their tendency to drunkenness and debauchery"[136] [A warrant in the Army or Navy was often considered an easy way to practice freely at home, for after 1749 any surgeon who had served in the Army or Navy for three years at least was allowed to enter free practice outside London without further exams. This led to a decrease of standards in the country, for at the outset of a war the entry examinations tended to be rather perfunctory! This way was especially chosen by Scotsmen for whom it was otherwise very difficult to set up practice in England.[137]]. Of the approximately 300 naval surgeons listed in 1779 only four had ever published anything.[138] Yet among these were the great pioneers in their respective fields.

John Pringle (1707-1782) gave the first impetus to the study of military medicine as a distinct branch (1752), followed by a number of British writers who advanced the knowledge of medical conditions as then existed in the Army.[139] British naval medicine in this period is not less remarkable since it was through the works and writings of men like Lind, Blane, Robertson, Trotter (1760-1832) and others that scurvy, the most typical of the sea diseases, was brought under control by the end of the century. The same doctors and surgeons, as well as enlightened captains and officers like James Cook, were also responsible for certain advances in hygiene in order to check the ravages of typhus and other “fevers” associated with warfare throughout the 18^{th} century.[140]

All these pioneers realised in their way that in the Army and Navy a great number of sufferers from disease and accident were under such conditions of command, control and observations, as to constitute a unique group of public life where scientific principles of curative and preventive medicine might be applied.[141]

From a study of early representative works on military medicine from Britain, Austria, Prussia and France, Bruppacher estimated that about 40% of their content dealt with preventive and hygienic measures.[142] [For this estimation, Bruppacher did not take into account Van Swieten’s little treatise on camp-diseases.] 18^{th} century military medicine was to a great extent a branch of the rising social and preventive medicine of that time and as such employed the numerical presentation of its successes.[143] A contemporary account of Pringle stated: “General Melville … when Governor of the Neutral Islands [Guadeloupe, Grenada and other islands in the West Indies, (Singer, p.243)] (1760-1775)… in consequence of the instructions he had received from Pringle’s book and from personal conversation with him…was the happy instrument of saving the lives of seven hundred soldiers”.[144]

Such statements were in turn helpful for the institution of medical reform, including that of the organisational structures.[145] Yet curative medicine, too, marched in parallel: as John Millar wrote in 1783:

“When the success of the various methods of treating diseases was first compared, by arithmetical calculation, there were few records on which the comparison could be founded…[I] was, therefore necessarily led to consult the army returns, as being particularly adapted to that purpose.”[146]

In the following chapters I shall investigate to what extent this was recognised and arithmetical calculations used, and with what consequences, especially in the treatment of “fevers” and scurvy and also in surgery. As “fevers” will appear, somewhat artificially broken down, in my three chapters on civil hospitals and dispensaries, on naval and on military medicine, I shall briefly outline in the following section the main issues in this complex topic of fever during the period in question.

E. On fevers

The 18th century struggle against fever has been compared, *mutates mutandis* with our present day efforts against cancer and arteriosclerosis. Both are the great killers of the times. A great deal is known about them, yet there subsist great disputes regarding their nature and treatment: "How many kinds of tumours are there? Just how, in essence, dies one kind differ from another? How to classify them? What are the causes of tumours? What is it that makes a tumour malignant?"[147]

If we replace "tumour" in the above quotation by "fever" these were the questions 18th century doctors asked. And despite a huge mass of relevant partial truths their, as our ignorance, was enormous, and the problem was consequently seen as complex and confused by many contemporaries.

For the historian it is still extremely difficult to get a coherent view of the problem. Yet there are chapters in two recent books in English where it is ably treated from the clinical point of view and that of public health.[148] Therefore I shall restrict myself to a short summary relying, if not otherwise stated, on theses works by King and Felling.

Fever was, for the 18th century physician, a disease rather than a mere symptom. Hermann Boerhaave (1668-1738), the outstanding physician of the earlier 18th century, whom Haller (1709-1777) called the *communis Europae praeceptor*[149] defined fever as a triad of rapid pulse, shivering and heat, with only the quick pulse being present from the beginning to the end. Fever was a fundamental phenomenon affecting the whole body. This definition, simple as it was, was sufficiently broad to allow for a great variety of complaints to be classified under its heading. Moreover, the pathophysiological mechanisms supposed to underlie the increased pulse were speculative and ague, as also was the long list of remote causes of fever. These causes had to be envisaged in terms of fluids and system which would be capable of determining the state of the whole constitution, i.e. the circulatory and/or nervous systems.

Richard Mead (1673-1754) a fellow student of Boerhaave at Leyden, and as physician to St. Thomas's Hospital in London his friend and correspondent was perhaps the most important British fever expert of his time.[150] He directed his attention to contagion as a possible remote cause of fevers, because he rejected the traditional explanation of fevers being due to acts of God. Contagion was for him the passage of some unidentifiable chemical substance from one person to the other. Fevers were initially classified on a basis of time, the degree of continuity of a fever being more important than its overall duration. There were *intermittent* fevers, i.e. a succession of paroxysms or fits with a complete remission in between. [According to the duration of the interval the physician distinguished between tertian, quartan, or double tertian fevers.] In *remittent* fevers there was a slight, but incomplete remission. In *continuous* fevers there was no remission at all. These three categories remained throughout my period, sometimes being lumped together, sometimes separated.

As today, the 18th century doctors could not stand idly by; they had to treat their patients. Their therapy in the earlier part of the 18th century reflected their pathophysiological theories of fever and the speculations of its remote causes. For Boerhaave, therapy consisted chiefly in the elimination of the morbid matter through saliva, vomit, urine or faeces, and cutting short of the initial irritation by moderate bloodletting. Suitable attention was paid to the six non-naturals, i.e. air, food and drink, motion and rest, passions of the mind, *retenta* and *defecta*, sleep and vigilance. Attention to all these factors resulted in a fairly sound hygienic regimen. As he did in his theory, in his therapy Boerhaave followed Sydenham and the classical authors. This can be seen, for instance, in his stressing the individual approach to each patient, an approach which was facilitated in turn by the absence of precise categories of fevers.

Among the generation of Boerhaave’s pupils certain less vague clinical groupings were arrived at, not least as a consequence of mass observations in the Navy and Army. One influential authority on fever was John Huxham, (1692-1768)[151] who though never a regular naval surgeon accompanied the fleet on many occasions, and whose practice at Plymouth was largely among the seafaring population.

Huxham distinguished between intermittent fever (ague, or malaria), and the low nervous fevers, which when aggravated were called putrid, malignant or pestilential; the latter was accompanied by petechiae. Against this type of general fever he opposed local inflammatory fevers. Inflammation was also a common factor in diseases, but it was definitely local rather than general, e.g. pleurisy, (peripneumony) (inflammation of the lungs) and peripneumonia notha (bronchitis). Although his classification was still rather confused, Huxham’s skill as and observer was apparent throughout his *Essay on Fevers* (1750), where he also quoted classical

Greek and Roman authors frequently; of the modern authors he too favoured Sydenham. His view also had therapeutic consequences, for he recommended Peruvian bark for the intermittents in accordance with Sydenham, whilst defending more copious bloodletting in the inflammatory and the other "general fevers". Since for him the cause of all fever was the heat of the blood, venaesection was a fundamental therapy.

A more definite step forwards was made by Sir John Pringle, another of Boerhaave's successful pupils. He did not worship at the Hippocratic shrine but tried to advance knowledge, within the existing framework, by his own observations; and he was not afraid to suspend judgment and to acknowledge his ignorance instead of filling in a gap with ingenious speculations, as Boerhaave would have done. Pringle deserves credit for his practical insight into, and concrete directions for, the hitherto academic or abstract fashions of "heat", "moisture" and so on, in relation to fevers, as he realised that remitting and intermitting fevers were rarely met with in camps and prisons. He gave an excellent description of the malignant continuous fever in these conditions i.e. camp, jail, ship, or hospital fever, which probably corresponds to typhus (this latter Hippocratic term was first given currency in 1769 by Cullen) but he did not consider this as a separate entity (although he apparently degenerate into that grave state. It is not irrelevant for my thesis that Pringle's contribution was perhaps more one of attitude, general approach and method, than of detailed contents or discoveries. I shall therefore discuss his methodology to some extent below.

It was the great Edinburgh clinician and teacher William Cullen (1710-1790) who aimed at making things orderly and neat. As a systematist he classified fevers into distinct categories, thereby envisaging them as separate entities. His *pyrexiae*, or febrile diseases, included most of the conditions now thought of as distinct infectious diseases which exemplified the various kinds of relationship between inflammation and fever. Thus, in a first attempt Cullen distinguished symptomatic fevers, accompanying topical inflammation, from essential or primary fevers. The latter were already divided into two groups, the familiar intermittents and the continuous. The latter subgroup was again subdivided by Cullen into three categories, *synocha*, *synochus* and *typhus*, terms which persisted into the 19th century. *Synocha* corresponded roughly to Huxham's inflammatory fever, with a strong hard pulse and no disturbances of the sensorium; *typhus* corresponded to Pringle's jail fever, with prostrative weak pulse, and delirium. *Synochus* was an intermediate category, for Cullen appreciated that the two former types were rarely pure and tended to flow on into the other. He considered the "yellow fever", of the tropics for instance as a variety of *typhus*.

Cullen also attempted to correct the theory of fevers of his forerunners. Influenced by the rudimentary neurophysiology of his time he substituted Boerhaave's "lentor of the fluids" by an equally hypothetical "spasm" of the arteries in explaining that pathogenesis of fevers. As to the many causative factors of the earlier 18th century, he dismissed a number of these by his emphasis on contagions. Indeed, impressed by the epidemic character of many fevers, he thought that" some matter floating in the atmosphere and applied to the bodies of men, ought to be considered as the remote cause of fevers".[152] These effluents were of two sorts, the *contagions* origination from human bodies suffering from a particular disease and capable of exciting the same disease in another person; and the *miasmata* arising from marshes and moist ground, i.e. from sources other than human. Cullen did not believe that there was a variety of specific contagions. There were the exanthemata (smallpox, measles) which *were* specific, but the chief problem was, as we have seen, the continuous fevers. And there he was unsure, attributing the differences between the various febrile states rather to circumstances of the environment (seasons, climate) than to distinct contagions. This view was challenged in the 1770s and 1780s because of similar description from very different climates by naval and military surgeons; and Cullen himself was not dogmatic, recognising that jail or hospital fever seemed somewhat different from other forms.

In terms of therapy Cullen was not dogmatic either, but successful.[153] He did not add any significant practical contribution. He knew that therapy would not attack the contagious material directly. Symptomatically the spasm of the arteries had to removed, which would be accomplished by the familiar remedies: rational hygienic measures, bloodletting practised with discrimination, purges and blisters. This was followed by correction of the prostration (or "debility") as well as by combating putrefaction with stimulants including Peruvian bark, wine and "antiseptics".

These were, interestingly enough, the same principles that remained *en vogue* until well into the 20th century. Bloodletting had, as we shall see, its ups-and-downs and finally lost its popularity in the 1850s, but until the discovery of the antibiotics, the inability directly to combat the contagion remained.

Of Cullen’s many pupils, John Brown (1735-88) and Benjamin Rush 81745-1813) became vastly influential in Europe and America. Both were brilliant, but complex and controversial characters. They lacked their master’s scientific outlook, i.e. his respect for truth, patient observation, and just reasoning. Their ideas therefore tended to be extreme. Both became entrapped in their own theories, especially in the morass of their concepts of excitability, excitement, direct and indirect debility, which led them to a reduction of distinct categories of fevers. For Brown, Cullen’s essential fevers were all “asthenic” disease, or diseases of direct debility, calling for a stimulating therapy of wine and opium. For Rush, there was only one fever, since there was only one cause, i.e. irregular action (or convulsion) of the vessels (see above).[154] Both Brown’s and Rush’s simplifying recommendations appealed to many of the poorly trained practitioners. Yet in their study of febrile diseases, both authors faced the past rather than towards the future.

On the other hand, Colin Chisholm (†1825) and Robert Jackson (1750-1827) (see below, P. ??) two other Edinburgh students of the time, [Whether or not they were Cullen’s pupils is difficult to say.] who became military surgeons in the last decades of the 18th century, had a different outlook. Again, as with Pringle, their advances lay more in attitude and method than in lasting contributions. With both, fevers existed in discreet forms, as they both denied a uniform cause for all fevers. Their pathophysiological and causative explanations for fevers centred on the question of contagion. With Chisholm for instance, Cullen’s animal contagion and marsh-miasma became sharply distinct from each other, the first being responsible for “malignant pestilential fever” (typhus?), the latter for endemic diseases such as the yellow fever of the West Indies. Both authors liked out for contagious agents. Chisholm launched the idea of two different gases, azote and hydrogen, intended as an object for further study. Jackson thought of a still misunderstood quality to act on various bodily constitutions which he in turn correlated with laboratory observations of particular appearances and reactions of the blood. These patho-physiological researches and the inferences drawn therefrom epitomised the new spirit of medical science, the spirit of eager youthful inquiry, of flexibility, in contrast to the restraints imposed by a system. This new spirit of empiricism, although repeatedly proved wrong, nevertheless persevered undaunted.

Thus by 1800 observationist doctors tended to favour the view that there were specific fever categories. Yet it must be pointed out that the specificity was not derived from the supposed specificity of a causative agent (which was still hypothetical) but rather from that of the clinical picture, particularly the cutaneous findings.[155] In viewing these 18th century attempts to discriminate fever entities such as typhus, malaria, and yellow fever, one must also bear in mind that these distinctions may be treacherous if we use the terms with their present-day specificity. Shrewsbury drew attention to the difficulties of identifying the clinical descriptions of the "plagues" of the past with bubonic plague rather than with typhus, typhoid, epidemic influenza, relapsing fever or even smallpox, if they did not concern a fully developed epidemic. Typhus could not be distinguished from bubonic plague until about the middle of the 19th century.[156] These difficulties concern also "malaria" and "yellow fever", (which is exemplified in the variety of names for these diseases in the 18th century) and contemporaries did not ignore them.[157] On the other hand, modern research confirmed to some extent 18th century descriptions of the favourable *milieux* for the development of typhus, yellow fever and malaria: typhus for instance *is* a disease of filthy and crowded areas, as existed in the 18th century in jails, hospitals, ships, camps and large parts of cities. Malaria *is* rather a disease of the countryside; the vector being zoophilic, it can maintain itself locally even in non-ideal conditions for the *anopheles* and the *plasmodia*.[158]

The specificity of fevers also became reflected in the therapeutic recommendations. For "typhus for instance, the picture of a contagious fever, associated with clinical debility and cutaneous eruptions, had been built up during the second half of the 18th century. Because of the weakness accompanying this and other contagious diseases, they would not bear bloodletting which was, on the other hand, practised by some authors in endemic diseases of miasmatic origin, such as the yellow fever of the tropics. Yet once more the views on fever changed. As hinted at above, specific identities were lost around 1810 during what Niebyl called the "British bloodletting revolution" (1977), which was linked to earlier drastic changes in America championed by Rush. Under the influence of Londoners such as Henry Clutterbuck (1767-1856), John Armstrong (1784-1829) and others, not only did specific fevers lose their identity, but even the varying "types" of fevers in different years or seasons tended to become less identifiable. Based upon autopsy findings of "inflammation" in cases of typhus (later in other forms of fevers, too) this was now seen as a congestive, inflammatory disease, thus overloading the patient's strength. This meant that the clinically observed debility was "indirect" and could be relieved by bleeding and purging away the excessive congestion. This view seems to have prevailed for typhus right into the 1830s in Britain. According to two contemporary sources, it was due to the spirit of rational empiricism that Brown's adherents had been here neither numerous nor influential.[159]

It is true, Marshall Hall (1790-1857) the (neuro)-physiologist and clinician who published his *Researches* on the curative effects of the loss of blood in 1830 and again in 1836, did not question whether and when bloodletting was indicated. He investigated instead how much blood a patient of a given *habitus* might safely lose. His chief result was the recommendation that the patient, when he was bled, should not be in a horizontal, but in a sitting position, so that the *deliquium* would appear earlier.[160]

Meanwhile, the nature (i.e. basically the contagiousness) and the cure of tropical fevers still remained controversial as reflected in the heated debates over the utility of quarantine in the 1810s and 1820s.[161]

It is against this changing background of identification, classification, patho-physiological mechanisms and causative explanations of fever, that the therapeutic issues which I shall discuss within the next three chapters of this thesis must be seen.

F. REFERENCES TO CHAPTER TWO

[1] Bostock 1833.
[2] Shryock 1948, pp.53-54, 66; Bullough and Bullough 1971, p.13.
[3] Hillary 1761, pp.xii-xiii, 23,36, 61; see also this thesis p.??
[4] Shryock 1948, p.66.
[5] Aikin 1771, pp.73-81.
[6] Percival 1767, pp.1-54.
[7] Sims, 1774 passim; Lettsom 1775, pp. 44-46.
[8] Millar 1776; Kirkland 1783, passim ; Fordyce 1793, passim; Edinb. Med. surg. J.1; 425-429, (1805).
[9] Muellner 1966.
[10] Edinb.med.surg J.4; 208(1808).
[11] Buer 1926, p.125; Schryock 1948, p.67.
[12] Risse 1970;1971.
[13] Shryock 1948, p. 68.
[14] Vess 1975.
[15] Gelfand1973 A., pp. 147-303, 369-388; 1977.
[16] Shryock 1948, pp. 53-54; Underwood 1977.
[17] Cope 1969.
[18] Chaplin 1919, p.16.
[19] Mem.med.Soc.Lond.1; xi, (1787).
[20] Quoted by Shryock 1948, p. 60.
[21] Bynum 1978 a, pp.199-200; Webster 1978.
[22] Buer 1926, pp. 126-136, 257-258; Woodward 1973; 1974, p.147.
[23] Shryock 1948, p.46.
[24] Chaplin 1919, p.20-21; Cope 1964, p.74.
[25] Bynum 1978 B.
[26] Miller 1957, p. 146; Duinton 1961; Gunn 1964, p.100; Wyke 1975, p.73; Bynum 1978 B.
[27] Hartston 1963, p.755.
[28] Bloom et al. 1962, p. 214; Triolo 1969.
[29] Wyke 1975, p.73.
[30] Davis 1821.

[31] Kershaw 1913 ; Sorsby 1936; 1946.
[32] Bynum 1978 B.
[33] Shryock 1948, pp.41-55.
[34] Chaplin 1919, p.22; Hartston 1963.
[35] Gray 1952; Batty Shaw 1968, p.236.
[36] Dukes 1960.
[37] Chaplin 1919, p. 24; See also Radcliffe 1976.
[38] Rolleston 1930; Rooks 1960; Hunter and MacAlpine 1965; Batty Shaw 1968.
[39] Anning 1963, Vol.1, p. 19.
[40] Rolleston 1930, p.258; Power 1939, p.vii.
[41] Chaplin 1919, pp. 35, 139-141.
[42] Med.Obs.Inq. 1; vi, (1757).
[43] Shryock 1948, p.51.
[44] Underwood 1951, p. 265.
[45] Cassedy 1974, p.284.
[46] Shryock 1961, pp. 220-224; Lawrence, in preparation.
[47] Quoted by Cassedy 1974, p. 291.
[48] *Ibid*., pp.292-294; See also Lauer 1966.
[49] See e.g. Laslett 1971, pp. 117-118,130,132. For recent reference to the extensive literature on Graunt see Laslett 1973.
[50] Cassedy 1974, p.297.
[51] Greenwood 1948 (A), pp.2-14; Pearson 1978, pp.10, 124-127.
[52] Cassedy 1974, p.298.
[53] *Ibid*., pp.300-301.
[54] Buer 1926, p.15.
[55] George 1925, pp.12,43-45; Buer 1926, pp.10-35; Cassedy 1974, p.308; Cullen 1975, p.9.
[56] Cassedy 1974, p.312.
[57] Greenwood 1948 (B), p.27.
[58] Rosen 1958, p.176.
[59] Marmion 1949; Miller 1957, p.111-123; Razzell 1977, passim; Pearson 1978, pp.543-559.
[60] Greenwood 1948 (A), pp.56-66; Rosen 1958, p.114,174; Bradley 1971.
[61] Greenwood 1948 (B), p.25.
[62] Charles 1961, p.62.
[63] McConaghey 1967; Childs 1970; Waldron 1970; 1972.
[64] Newman 1932, pp.170-171.
[65] Quoted *ibid*., p.171.
[66] Viseltear 1968.
[67] Cassedy 1974, pp.309-310.
[68] *Ibid*, p.302.
[69] *Ibid*.
[70] Manley 1952; Miller 1962; Cassedy 1969;1974, pp.303-305; Lesky 1976, pp.27-28.
[71] Peter 1975, pp.85,87-95.
[72] Bruppacher 1967, p.80; Probst 1972, pp.90-93,100-101,191-194.
[73] Peter 1977, p.91.
[74] Cassedy 1974, pp.304-305.
[75] Miller 1962, pp.134,136.
[76] Miller 1957, pp.111-123; Pearson 1978, pp.556-557.
[77] Underwood 1977, p.128; DNB.
[78] Med.Ess.Obs.1; xv, (1733).
[79] Cassedy 1974, pp.303,310.
[80] Lawrence, in preparation.
[81] Gregory 1805, p.150.
[82] Med.Obs.Inq.1; 240-267, (1757).
[83] *Ibid*., 3; 2nd ed., 189-228, (1769).
[84] *Ibid*., 1; 365-412, (1757); 2; 70-99, (1762).
[85] Ferriar 1792, p.xx.
[86] Gregory 1805, pp.144-145.
[87] Wright S. Clair 1946, pp.62-68.
[88] Monro 1764, pp.397,398.
[89] *Ibid*., pp. 25-26.

[90] Hunt 1972, pp.15,65.
[91] Millar 1777, pp.34,75 (misprint, corresponds actually to p.61).
[92] *Ibid*., pp.34,76.
[93] Monro 1780, Vol.2, pp.338-39.
[94] Ess.phys. lit. 3 ; 2nd ed., 178-291,551-556 (1770-1771).
[95] Monro 1780, Vol.2, pp.200-202,204,215.
[96] *Ibid*., Vol.2, pp.323-340.
[97] Millar 1783, pp.15-16,33-34.
[98] Cantlie 1974, p.126.
[99] Black 1789, p.iv, 250; see also Abraham 1933, pp. 185-204 for the inoculation controversy.
[100] Chaplin 1919, p.39.
[101] Heberden 1802, pp.365-366; Crummer 1928, p.229; Livesley 1975, p.160.
[102] Greenwood 1948 (A), p.50.
[103] Millar 1777, pp.4,7.
[104] Millar 1783, pp.7,41.
[105] Black 1789, pp. vii-viii.
[106] Thomson, quoted by Singer 1949, p.154.
[107] Quoted by Singer 1949, p. 153.
[108] Pringle 1752, p.xiii; Singer 1949, pp.244-245.
[109] Bégin 1960, pp.3-4.
[110] *Ibid*., pp.38-39.
[111] Garrison 1922, pp.141-147.
[112] Bruppacher 1967, pp.15,38; Johnston's Roll 1968, pp. xxi-xlii.
[113] Cantlie 1974, pp.104,108,174-175,420-421.
[114] Chaplin 1919, pp.77-86.
[115] Cantlie 1974, pp.178-181,202,203.
[116] *Ibid*., p.130.
[117] *Ibid*., p.109.
[118] Hamilton 1787, pp.14-15.
[119] *Ibid*, 16,18.
[120] *Ibid*., pp.17-18,89-102,104-105,139-141.
[121] Reide 1973, p.333.
[122] Cantlie 1974, p.120.
[123] Millar 1798, pp.81-82; Stevenson 1964.
[124] Cantlie 1974, pp.111,204,259.
[125] *Ibid*., p.205; Rollo 1801.
[126] Lloyd and Coulter Vol.3; pp.3-5.
[127] Chaplin 1919, pp.105-106.
[128] Lloyd and Coulter Vol. 3; pp. 187,190-191.
[129] *Ibid*., pp.190-191,193-195.
[130] *Ibid*., pp.212-213,267.
[131] *Ibid*., pp.215,218.
[132] *Ibid*., p.23.
[133] *Ibid*., p.18.
[134] Lind 1757, p.vii.
[135] Lloyd and Coulter Vol.3, pp.4-9,18.
[136] Allison 1943, p.113.
[137] Lloyd and Coulter Vol.3, pp.13,2.
[138] *Ibid*., p.20.
[139] Chaplin 1919, pp.86-91.
[140] Lloyd and Coulter Vol.3, p.329; Schadewaldt 1955.
[141] Singer and Underwood 1962, p.181.
[142] Bruppacher 1967, p.14.
[143] Singer and Underwood 1962, p.181; See e.g. Cantlie 1974, pp.91-93.
[144] Quoted by Singer 1949, p.243.
[145] Cullen 1975, p.9.
[146] Millar 1783, p.3.
[147] King 1971, p.123.
[148] *Ibid*., pp.123-155; Pelling 1978, pp.1-33.
[149] Quoted by Underwood 1977, p.10.

[150] *Ibid*., pp.3,11; Lloyd and Coulter Vol.3, p.330.
[151] Underwood 1977, pp.178-181; Lloyd and Coulter Vol.3, p.330.
[152] Quoted by King 1971, p.141.
[153] Risse 1974.
[154] See also Butterfield 1946; Jarcho 1957.
[155] Niebyl 1977, p.466.
[156] Shrewsbury 1970, pp.13,124-125,127,128,141,152,303.
[157] See e.g. Lloyd and Coulter Vol.3, p.341; Black 1789, pp.43-59.
[158] Howe 1977, pp.68,79.
[159] Alison 1833, p.xcviii, Bostock 1833, p.lxiv.
[160] Hall 1830.
[161] Ackerknecht 1948.

CHAPTER THREE: DISPENSARY AND HOSPITAL MEDICINE

A. **INTRODUCTION**

I have in the foregoing chapter pointed out the increasing use of numerical argumentation for medical, and in the broader sense social, reformist goals during the 18th century. There was a "numerical" climate associated with the growth of trade, the rise of insurance business and even the change in state-administration, a climate in harmony with the spirit of the age, the general desire for efficiency.[1] How would this be reflected in the medical world?

The new charity hospitals for the poor were partly at least a result of the growth of philanthropy in the 18th century, itself rooting partly in the increasing wealth an prosperity of the middle classes. But philanthropy, too, changed its character during this time. The old paternalistic almsgiving became modern cold philanthropy with its strict organisation and its definite aims. As one historian put it: "Almsgiving was not merely an end in itself ...it must show results".[2]

New types of medical charities, the dispensaries and specialised hospitals of the second half of the 18th century, bore not only philanthropic, but also clearly social reformist features: the condition of the poor could be bettered if the most ravaging scourges according to the bills of mortality, namely smallpox, fevers and the mortality in childbed and early age could be checked. One might suspect therefore that these institutions would particularly insist on their results. This would neither imply that their annual reports could always be trusted, nor that they had a scientific aim or influence. Indeed, the main objective of many of these reports was the raising of funds from the public by showing the overall success of the institution concerned.

Bearing this in mind and the contemporary methodological and professional issues outlined above, I shall investigate in this chapter whether the growth of these new institutions was associated with the development of what is conveniently called "hospital medicine", i.e. the systematic and extensive investigation into the methods of treatment and the phenomena of disease. I shall first concentrate on some of the quantitatively most important medical field of the time, as mentioned above, namely "fevers", midwifery, paediatrics, and rheumatology, the case for smallpox having already been Made (see above, p.??). Then I shall also discuss the clinical evaluation of some specific drugs such as the extract from the foxglove and arsenic. As in other spheres, personality counted for a great deal in the medical world of the 18th century. I shall thus concentrate in this chapter on the individuals rather than their institutions. The first author I shall discuss, Francis Clifton, aptly illustrates that the main question of this chapter is not proposed haphazardly.

B. **PRELUDE**

1. FRANCIS CLIFTON

In Britain, constructive criticism of scholastic medicine never died completely since the time of Bacon and Sydenham. Admissions of doubts and ignorance were continually being made with openness by medical men.[3] For the purpose of this thesis Francis Clifton's *Tabular observations recommended as the plainest and surest way of practising and improving physick* (1731) are especially noteworthy.

Clifton (†1736) was a pupil of Boerhaave and became later and honorary Cambridge M.D. and physician to the Prince of Wales. In 1734 he left abruptly for Jamaica where he compiled a weather-disease account left unfinished at his death.[4]

In his programme for "improving physick", based on the recommendations of Hippocrates and Bacon, Clifton deplored the lack of men like Sydenham who had first put observational medicine in the Greek manner into action in Britain. He stressed the value of the Hippocratic method, yet did not rely slavishly on the authority of the Greeks. This is illustrated in Clifton's assertion that it was unclear "how far his [Hippocrates'] observations will hold good with us ....; for it does not appear, that any of our Physicians have made the experiment". Therefore, he argued, one had to write about the diseases of *England* "as ever Hippocrates did upon those of *Greece*".[5]

Influenced by Sydenham's "climatic" thinking, Clifton kept a daily register of the weather. Clinical recording he thought much needed since “the writers of observations, (which are very few, in comparison [to those who write on theories] they, for the most part, have trusted to their *Memories*, for almost all the cases they have left us: a very fallacious way of instructing, and by no means proper for a Physician.”[6]

The tables Clifton proposed for regular and objective recording of cases contained one column for sex, age, temperament, occupation and cause of disease, then two columns for daily entries of the symptoms, and one column each for the calendar dates, the remedies used, and the event. In this way even the busy practitioner could contribute to the observational sciences; and Clifton did not think the task too onerous:

“I know of none shorter to answer all intentions; and to do a thing of this kind by halves, is much the same with not doing it at all...By the help of *abbreviations* [it] may be considerably shortened...[and] the *Latin* tongue will be shorter than the *English*, and sometimes on Greek word will express that which requires many words in *Latin*....”

Clifton’s recommendations were appropriate, indeed. He added that he would never “write upon any subject, as a Physician, for which I have not *Tabular Authority*”.[7]

In another critical work entitled *State of physick, ancient and modern* (1732) he adapted his recommendations to hospitals:

“...three or four persons of proper qualifications should be employed.... to set down the cases of the patients there from day to day, candidly and judiciously, without any regard to private opinions or public systems, and at the year’s end publish these facts just as they are, leaving every one to make the best uses of ‘em he can for himself.”[8]

If such a programme were put into effect, diseases would be better understood and more easily cured, even if the *materia medica* were not to improve. He asserted, though, that if the latter should be reformed and put upon an observational footing, “everything would then be done, that the Art is capable of”.[9]

Clifton discussed the principles and guidelines for his programme so well that authors were still emphasising the same principles one hundred years later. They were the unreliability of memory as a basis of judgement, the necessity of regular and frank recording of all cases occurring, the practicality of compiling the data in tables and the importance of their periodical analysis and publication. A hospital run accordingly would correspond to the research institute envisaged by Bacon.

Whether Clifton had any direct influence on his contemporaries is difficult to determine. I did not find him mentioned in subsequent British publications, but Jean Colombier, the great reformer of French military medicine under the *ancien régime*, referred to him in his own similar programme in 1772,[10] and that is sufficient reason to mention his work.

2. THE EDINBURGH SCHOOL

a. Doctors Monro

Yet there were contemporaries of Clifton who put into action a programme at Edinburgh along similar lines. In 1726 a new medical faculty was established there with a view to creating a medical school modelled on Boerhaave's at Leyden. Thus a teaching hospital with six beds for the poor was opened in 1729. It had great success, and was incorporated in 1736 by a Charter from George II (1683-1760) as the Royal Infirmary of Edinburgh. A new building was erected between 1738 and 1748.[11] Alexander Monro *primus* held the chair of anatomy there from 1720.

A register of cases was always kept in the Infirmary, and it occurred to some of the five attending practitioners that their observations might profitably be published. As a result, in 1731 a Society for the Improvement of Medical Knowledge was formed with Monro as its secretary and as editor of its *Medical Essays and Observations* which first appeared in 1733. Five volumes (the last in two parts) were published, the last in 1744. The editorial policy encouraged the publication of unsuccessfully treated cases, anonymously if so desired.[12]

Several times in these Essays Monro warned, (as he had also done in his lectures) that people were too hasty in fixing general rules for the cure of diseases founded on one or two cases only.[13] As a matter of fact he himself lived up to his own warning, for of the eleven articles published in the Essays in which the argument was based on more than two cases (see above), seven were by him.

He was the first, for instance, to publish all his cases of amputation. In 1737 he reported on fourteen without death; by 1752, when the 4th edition of the Essays appeared, he announced 85 new cases which made a total of 99 cases with only eight deaths.[14] These included all the operations done on large extremities at the Royal Infirmary by him and his colleagues.

Monro also kept statistics on operations for breast cancer, which show that he did not neglect long term observations. He told his students that

"Of nearly fifty, which I have been present at the extirpation of, only four patients remained free of the disease for two years .... This disease does not always return to the part in the neighbourhood and sometimes at considerable distance. Upon a relapse, the disease in those I saw was more violent, and much quicker in progress than in others on whom no operation had been performed."[15]

Twice Monro published accounts of unsuccessful operations anonymously, as can be determined by comparing his original articles in the first edition of the *Essays* with those contained in his *Works* (1781) [Compare *Essays* Vol. 1 (1733), pp. 234-238 with *Works* (1781) pp. 463/64; and *Essays* Vol. 3, (1735) pp. 299-303 with *Works* pp. 533-534. He wrote two other anonymous articles: *Essays* Vol. 1(1733) pp. 305-320 (*Works* pp. 607-615) and *Essays* Vol. 4 (1737) pp. 418-425 (Works pp. 492-495)].

To get around the difficulty of the small numbers he attempted to collect more information by correspondence. For instance by this means he wanted to enquire about the value of Peruvian bark in the treatment of gangrene, (his letters yielded eight cases), and on the value of inoculation against smallpox,[16] (in response to a communication from the Paris Medical Faculty which he received in November 1763). In his answer of June 1764, he showed a quite modern approach to statistics, typical for the whole question of prevention of smallpox throughout the century. He was able to gather in this short time the results of 4851 inoculations from 89 private doctors all over Scotland; with additional cases from Edinburgh, he had 5154 cases with 71 deaths, a mortality of one in 73.[17] He was very cautious in drawing any conclusions from comparing smallpox mortality during the ten years before and after the introduction of inoculations (which showed no difference), for, it was necessary "to consider the different circumstances in calculations". Unfortunately he was unable to obtain any information on the morbidity and recovery rates of smallpox but he hoped to impress upon the clergy (who kept the parish registers) the public importance of such data.[18] Unfortunately his intention to publish these did not materialise in his lifetime. But his smallpox study, sent to Paris, was also published in Britain. Like the *Essays* it was praised throughout Europe and translated into French, German and Dutch.[19]

Thus some numerical analyses of systematic recording, though not of a sophisticated nature, were being propagated quite extensively during the 18th century by means of the *Essays*, and by their editor alone.

In Edinburgh itself Alexander Monro *secundus*, the third and youngest son of Monro *primus*, continued his father's habit of record-keeping on a private Basis. He took over the teaching of anatomy from his father in 1758, and he was made professor of medicine, anatomy and surgery in 1777. According to one historian Monro *secundus* "was extremely methodical, keeping careful clinical records of all his patients and indexing them. The index shows that from 1767 to 1811 inclusive he had 10,107 cases, recorded in 33 volumes".[20] This methodical type of observation (together with his predilection for physiological and pharmacological experimentation) was reflected in his lectures (as illustrated by a manuscript taken down in shorthand kept in the Royal College of Surgeons).[21] It may well have impressed some of his students who became equally methodical, arithmetic observationists. Since he taught for nearly fifty years, until 1806-1807, his students included Lettsom, Withering and Cheyne from the new charity institutions, Blane from the Navy, McGrigor from the Army,[22] who will all appear in this thesis.

b. <u>William Cullen and John Gregory</u>

Two of the most influential Edinburgh teachers besides the Monros were two pupils of Alexander primus, William Cullen (1710-1790) and John Gregory (1724-1773). Cullen first taught chemistry (1755) and *materia medica*, then theory (1768) and, after Gregory's death, also the practice, of medicine until his own death. He was open-minded and has been characterised by the 1830s already as having epitomised the spirit of rational empiricism.[23] Yet, albeit Cullen was an acute critic of other men's errors, he was curiously blind to his own unwarranted assumption as I have mentioned in my introductory remarks on fevers.[24]

His great emphasis lay on classification of diseases, for which he created a widely followed system. This may not only be seen as a mere compression of medical facts into an all-embracing framework,[25] but also as an attempt, perhaps unconscious, to establish a pre-condition for numerical analysis, i.e. orderly listing of diseases taken as separate entities.

John Gregory taught the practice of medicine only from 1766 till his early death, but his lectures were still edited in the beginning of the 19th century by his son and successor James Gregory (1753-1821). I have mentioned Gregory's methodological concern above (p.??). He, too, was clearly a rational empiricist.[26] Cullen's classification appealed to him for the clarity, simplicity and precision of its definitions of diseases according to symptoms, and advantage which was lacking with authors who took mere hypotheses and "causes" as their guidelines.[27] Consequently Gregory [Himself an offspring of a family of distinguished mathematicians.[28]] recommended the study of mathematics to his students - not so much as leading directly to important discoveries, but rather because it opened the mind and accustomed it to accurate reasoning.

But above all, Gregory sowed doubts, doubts on the value of traditional methods of cure on one hand, dismissing on the other hand the adoption of new ones without proper empirical examination. In this respect he thought the reliance on single cases so prevalent in his day, that it "leads [us] to neglect inquiries of more general utility".[29]

Cullen's and Gregory's appreciation of the collection of well-defined and extensive data is noteworthy. Yet this was an advance more in attitude than in practice. Their clear writing, critical acumen, and analytical powers needed new factual data, new techniques, new tools to become truly fruitful. Could not numerical analysis become one of these new tools? Indeed, if he took together the emphasis on direct observations and on the weather-disease-relationship of all teachers, and the specific features of Monro's, Cullen's and Gregory's teachings, the perceptive Edinburgh student of the second half of the 18th century might well have appreciated the potential value of the application of numbers to gain more certainty in medicine. It is no accident that many of the individuals considered in this thesis studied in Edinburgh.

What was needed, first, were sets of data, collected according to the rational empiricists' and observationists' criteria. These became available relatively easily in the new type of charities, i.e. the dispensaries and specialised hospitals, which were in turn required to prove their worth in the competitive world of 18th century charities. It is towards them that I shall now direct my attention.

C. EARLY REPORTS FROM GENERAL INSTITUTIONS

1. IN LONDON

a. George Armstrong and the Dispensary for Sick Children

In 1769 George Armstrong (the entry in the DNB gives no dates) opened a Dispensary for Sick Children of the Poor in London. It was the first public institution for out-patients in Britain since a similar attempt by the Royal College of Physicians (1697) had ended in 1725. In the prospectus Armstrong committed himself to keeping and regularly publishing as fair an account as possible of the success of his practice. "Whatever discoveries or improvements may be made from time to time in the application of medicine to these little helpless [patients] shall be faithfully communicated". Indeed accounts giving the overall mortality of his patients were published quarterly and annually. However Armstrong's plan remained largely unfulfilled, for his final report of the institution after its closure in 1781 did not reveal any analysis derived from the "particular account of all the children's cases, together with the method of treating them" except for some cases added to the 3rd edition of Armstrong's Textbook on *Diseases most fatal to infants* (1783).[30] [To gain supporters he had suggested that the preservation of children of the industrious poor guaranteed the future labour force and was thus of "essential benefit to the public".]

b. John Coakeley Lettsom and the General Dispensary in Aldersgate Street

In London the first general dispensary was established in Aldersgate street in 1770. Lettsom (1744-1815) and James Sims (1741-1820) were ist first physicians, and Nathaniel Hulme (1732-1807) was its man-midwife. Lettsom,[31] a Quaker, had graduated at Leyden (1767), but spent also some time at St. Thomas's in London and in Edinburgh (1768). LRCP in 1770, he set up practise in London, married a rich lady, and quickly rose into the established circles of the Society of Arts (1770) and the Royal Society (1771). He was less important as a medical scientist than as a promoter of medical science and of social reform He founded the Medical

Society of London, the Royal Humane Society, the Royal Sea Bathing Hospital and the General Dispensary, which I deal with in this section.

As outlined in the title of a pamphlet Lettsom published in 1775, he thought that an institution of a general dispensary type served to "the improvement of medicine in London on the basis of public good" in four ways. 1) It was successful in reducing mortality, which he proved with comparative figures.[32] 2) It might check the proliferation[33] of medical quacks, against whom Lettsom fought persistently.[34] 3) It indicated the possibility of establishing medical schools with clinical teaching, for which facilities were still very restricted in London.[35] For such schools Lettsom devised a detailed plan, including the writing up of reports by the students.[36] 4) It permitted theory to be brought to the test of experience on a large scale and it might result in the (in)efficiency of certain remedies in certain diseases being demonstrated clearly.[37]

These guidelines, especially the last, were clearly derived from Lettsom's own experience at "his" Dispensary as described in his *Medical memoirs* thereon, published in 1774. He wrote that previous to his election as physician

"A painful sensation was ... excited in my breast at the loss of patients by the usual ro[u]tine, when I reflected that another method of treatment might probably have proved successful. By my election to the General Dispensary, a more extensive field of practice afforded me daily opportunities of ascertaining the doubts, and clearing up the difficulties, under which I had laboured...."[38]

Concerning the treatment of "fevers" for instance, Lettsom and Sims had been criticising the current anti-phlogistic therapy with emetics and bleeding for some time.[39] Now Lettsom was able to claim that this old practice, frequently marked with fatality, had given way to an almost uniform success by the use of Peruvian bark and ventilation.[40] And this was no haphazard conclusion arrived at in casual way.

"From the useful hints suggested by my ingenious friend Dr. Percival [1740-1804, see below]... I was induced to keep and exact register of the diseases and deaths which fell under my observation in the ... Dispensary, agreeable to the following tables which include a period of twelve months."[41]

From those tables, indeed, one could see that he had lost only a few of his fever-patients from April 1773 till March 1774 [i.e. 3 out of 22 from febris intermittens, 3 out of 65 from febris nervosa, 8 out of 192 from febris putrida, none out of 82 from febris remittens and none out of 29 from febris simplex].

This dispensary report contained, as had been the case in the reports from the great London hospitals,[42] monthly figures of patients admitted and discharged as “cured”, “irregular”, “improper” or “dead”, from which an average annual proportional mortality was calculated (1:37.5 in the present case). Applying Percival’s suggestions for the improvement of the bills of mortality to his own dispensary practice, Lettsom drew up two additional tables i.e. he broke up all cases he had seen in diagnostic categories and he computed their total incidences in one year. In a third table he listed - again in monthly distribution - the numbers of fatal cases of any disease; he even specified with symbols the marital status and age of each deceased.[43]

In London official Bills of Mortality according to diseases had been issued regularly since 1657; in 1728 they also began to give age, and sex.[44] However, they were justly deemed inadequate, for the burials of conformists only were recorded and the diagnoses were made by nurses or ignorant domestics. Lettsom asked for his tables to be compared with the official ones to show their striking differences.[45]

Lettsom’s monthly arrangement of mortality and morbidity reflected the climatic pathology still *en vogue* at that time, but it was not specially commented upon in the text. He also gave a series of illustrative single cases, especially, as he stressed, of unsuccessful ones.[46] His reliance on numerical presentation of therapeutic results was also documented in his reprinting tables of death-rates before and after the introduction of inoculation for smallpox in the way that Alexander Monro had done.[47]

The success of the Dispensary was apparent when the number of consultations (1700 persons per year) and the overall mortality was considered. The latter was 1 in 33 patients, which was excellent when compared with 1 in 5 at the Paris Hôtel-Dieu, 1 in 13 at St. Thomas’s and Manchester Infirmaries respectively.[48] (In-patient hospitals, of course, treated different types of diseases).

c. John Millar and the Westminster General Dispensary

After this successful start, dispensaries sprang up all over London.[49] Lettsom became physician at two others and also a Governor to the Westminster General Dispensary, opened in 1774.[50] John Millar (1733-1805) (DNB) was appointed physician, Robert Bland (1730-1816) man-midwife. Millar reported on his practice in 1777, Bland in 1781. Little is known about Millar, except that he graduated M.D. at Edinburgh, and must therefore have studied there for three years at least. As he was born in 1733, Alexander Monro primus and perhaps William Cullen were among his teachers. And, prior to his appointment at the Dispensary, Millar had already written two books.

The first, *Observations on the asthma, and on the [w]hooping cough* (1769), dedicated to Queen Charlotte, established the grounds on which Millar based his belief in the assessment of valid therapies with the help of arithmetics (see above). He started off with good empiricist rhetoric, claiming that he had “no favourite theory to support nor any medical sect to defend”.[51] Observations of the course of a disease and effects of medicines by a single physician alone could not be sufficient for establishing its complete history and a certain method of cure. That was why doctors had long since instituted societies for collecting and publishing medical observations, Millar wrote in an obvious reference to the Edinburgh life he had known. “By these means,” he thought, “most diseases have been fully and accurately described and their treatment perfectly ascertained”[52] and there was no reason not to pursue in this way for the remaining diseases. As an example, he quoted[53] from a wholly statistical paper given by Matthew Maty (1718-1776),[54] the foreign secretary of the Royal Society, on the advantages of very early inoculation (1767). On the very page quoted by Millar, Maty had written how easy a task it would be fully to prove his particular point, if the London Bills of Mortality were not arranged in “the most absurd manner”.[55]

Thus, Millar, at this early stage must have had some affinities with the application of arithmetic to a clinical topic, and with the problems involved. This fact is further supported by his acquaintance with the work of George Armstrong from the Children’s Dispensary and with that of William Hillary (see below, pp. ??)[56].

Millar's second work, *Observations on the prevailing diseases in Great Britain* (1770), was an important book, often quoted in its time. It might well have been instrumental in bringing this ordinary practitioner from Kelso, first to the chair in the new Medical Society of London (1773) and then to the new dispensary. Besides decrying the fancy of medical systems based on theory, superstition and ignorance as the major cause of the retarded state of medical science and praising observation as the panacea, his programme went one step further, a step wherein he was emulated by subsequent writers such as Lettsom and Sims. In his view, particular cases were different irregular and unconnected, so that it was not possible to reduce them to any standard. That was why epidemics had to be studied and their complete accounts to be compared.[57]

Aware of the ubiquitous specificity of measles and smallpox[58] he wondered whether it was true that the epidemics of one season really differed from those of another. "This question is important", he declared, for if the answer were yes, it would be "impossible that the healing art should ever arrive at any great degree of certainty".[59] On the other hand, should a resemblance be observed by comparing the popular diseases at different ages and in various climates, many useless differentiations might be abolished and the physician's attention directed to those features in which the diseases agreed. This in turn would allow more certain and extensive therapeutic rules to be established.

Sims used the same reasoning in his *Observations on epidemic disorders* in 1773. It corresponded, indeed, to an important step towards a concept of ontogenic disease entities. He expressed it thus:

"Causes of epidemics seem to pay little regard to the particular constitution of each person....[thus] the general method of cure admits but small variation on account of different circumstances of the patient."[60]

This anti-Hippocratic concept would also allow all cases to be used without discrimination in a numerical analysis. Millar may have borne this in mind when he wrote that the mere relating of cases would have little tendency to improve the science of medicine. Only an analytical review of them, especially of the fatal cases under consideration and of the various methods of cure, would render such reports useful.[61]

Since most of the acute popular (i.e. epidemic) diseases were accompanied by "fever", Millar dealt extensively with the treatment of some fever categories, the number of which he tended to reduce. A comparison of the description of the agues and remittent fevers by military and naval surgeons from different places on the globe led him observation showed him that remitting fevers might heal best without the traditional evacuating cures. Was it not related to the ague, in which the bark showed good effects, according to the accounts of Lind and Cleghorn?[62] By accident, he started prescribing it, against Boerhaave's authority, for remittents, too, and had several years' success; he discussed nineteen cases, wherein those treated with the new method survived, whereas the others died.[63] But the final vindication of his theoretical and practical views came only in 1777 from the analysis of his dispensary practice.

Meanwhile, in a paper read to the Medical Society of London in 1774, he attacked the current use of antimonial preparations, especially Dr. James's secret and much praised antimonial powder. This was recommended by its inventor for fevers, (and it has therefore been called the aspirin of the 18th century).[64]

Robert James (1705-1776),[65] educated at Oxford and Cambridge (M.D. 1728) was a LRCP who had taken out a patent for his anti-fever powder in 1746. As from 1748 he was illustrating its value by describing his successful cases in the numerous editions of his *Dissertation on fevers*. In the seventh edition (1770) he forwarded as further proof the decreased overall mortality from 1751 to 1763, compared with that from 1738 to 1750, when his powder had not yet been available.[66] His use of the official London Bills of Mortality was the same as that employed generally in the question of inoculation. [James recommended his powder also against smallpox]. But whereas 18th century authors on smallpox referred to a specific category (i.e. the pox) within the Bills, James cited only the overall figures of the Bills.

When analysing James's data, John Millar was acquainted with the numerical approach to the smallpox question (see above). He first remarked that the inaccuracy of causes of death, as diagnosed by chambermaids, would be the same for both the 1738-1750 and the 1751-1763 periods. Secondly, antimonials, albeit in another form, had already been used during James's earlier period, because of a strong recommendation by Huxham in his classic treatise of 1737. If a decrease of deaths from fever *alone* could be demonstrated "this", he said, "will be most desirable evidence".[67] In fact it *had* decreased in absolute figures during the period mentioned by James; but in the last ten years (i.e. from 1764-1773), when his powder was even more extensively used, it had increased again. Therefore Millar judged James's work valueless since it produced evidence directly opposed to that required if one accepted the facts advanced. Yet these facts which were, from his higher standpoint, to be rejected as inadequate, misapplied and misrepresented.[68]

From 1774 onwards Millar finally had occasion to put his programme into practice. His *Observations on the practice in the Medical Department of the Westminster General Dispensary...* (1777) suggests how he actually did it. In the plan he wrote: "Though the relief of Individuals was the professed and immediate object of the institution, more important and extensive purposes were intended to be accomplished," namely a better account of the prevailing diseases and the presentation of incontestable evidence of safe and effectual methods of cure. Since opposition was feared, a clerk was appointed (so that nothing might depend on the testimony of physicians) to keep the records and make out the returns.[69]

Such an account of an institution which was, in addition, open for public inspection, was for Millar part of the demystification, or the reformation, of medicine, just as the Reformation in religion had put an end to the withholding of the Scriptures from the laity. For in his opinion, “the priests of the Temple of Aesculapius [still] continue their oracular ambiguity, involve their art in tenfold obscurity, and in hiding it from others, conceal it from themselves”.[70] [See his collected works (1802-1803?) to which he set the title: *Observations on the change of public opinion, in religion, politics and medicine*.] He acknowledged the complexity, greater than in any other science, of asserting the “comparative merit of different medicines and methods of cures”.[71] This explained the success of mountebanks and even permitted an established LRCP to withhold the recipe of the medicine he sold and extolled. As with Lettsom, both practices were thorns in his side. To resolve such questions he believed that one needed demonstrations by “incontestable evidence”, declaring that “Error ought not to be sanctified by custom ... nor concealed by mystery and reserve; nor the test of arithmetical calculation evaded”. For this latter,

“detached cases, however numerous and well attested are insufficient to support general conclusions; but by recording every case in a public and extensive practice, and comparing the success of various methods of cure with the unassisted efforts of nature some useful information my be obtained; and the dignity of the profession may e vindicated from vague declaration and groundless aspersions.”[72]

There could hardly be a stronger plea for numerical analysis!

Millar published two tables of all cases treated in the first two years of practice at the Dispensary, compiled from monthly returns, featuring for each disease the numbers of patients admitted, cured, relieved, and discharged into other hospitals. Separate columns indicated the numbers of “improper subjects”, (i.e. those not likely to receive any benefit), of cases too advanced for treatment and of patients irregularly attending, and deaths. These latter groups (may we call them “controls”?) were further analysed according to diagnosis and “event”.[73]

Such returns, Millar thought, ought not only to be introduced in all charity establishments but also in the Army and Navy.[74] In a wholesale attack on military surgeons, (see below and chapter on military surgery) in which he used comparative figures (sometimes uncritically, a blunder which he must have known himself when one considers his criticism of James), he evidently intended to promote a change in the military handling of fever in the forthcoming American War. At the same time he hammered into people’s minds that arithmetical analysis of recorded data was the sole basis for evaluating a therapy. This he pursued with private means and unceasing insistence until 1802. Although he was verbose and repetitious, some of his passages were remarkably shrewd (see below p.??). He had positive numerical evidence for his fight against the advocates of bleeding (early and copious), blistering and purging in fevers. Perhaps it was his aggressive tone, perhaps his deliberate struggle against authority which incurred the wrath of some established authors, such as Donald Monro and Sir John Pringle, expressed in pamphlets and papers written against his publications of 1770 and 1777.[75] [Sir John Pringle was involved in this, it seems, although his brother had been cured of fever by Millar in 1762.][76]

The presentation of Millar's results gave, as I have noticed, easy matter for criticism, but such criticism was philosophical rather than based on factual evidence. After one year, opposition also arose against the whole system of record-keeping at the Dispensary and Millar lost his clerk Reide (who, once in the Army, continued with it), and in 1779 he wrote that the registers now kept were useless for scientific purposes.[77]

Millar was perhaps the most aggressive of the civilian physicians fighting for the overthrow of the anti-inflammatory therapy of fevers as based on the mere authority of Boerhaave and Pringle, and for its replacement by Peruvian bark, whose superior value was proved by numerical comparison. But he was not alone. Besides Lettsom, there was also, for instance William Rowley.

d. William Rowley and the St. Marylebone Infirmary

William Rowley (1742-1806) trained at London's St. Thomas's Hospital, started as and Army surgeon in the West Indies during the Seven Years' War. [Johnston's Roll does not confirm this indication of the DNB. Rowley was possibly a Navy surgeon at the same period.] In 1766 he set up practice in London He also became associated, as a man-midwife, with one of the lying-in charities and later with the St. Marylebone Dispensary opened in 1785.[78] As did Lettsom and Millar, he dedicated his first report, dealing with fevers, (1788) to the subscribers of the dispensary. It was his experience in Jamaica that had taught him that Boerhaave was in error when considering all fevers inflammatory and treating them accordingly with bleeding, vomiting, purging and antimonials. There *were* some inflammatory diseases, and they were localised,

[ophthalmia vera	pleuritis	cystitis
angina inflammatoria	nephritis	
peripneumonia	peritonitis].	

They were not contagious and could arise from sudden cold in predisposed persons. However, Rowley looked at the majority of fevers as general diseases. These were contagious and arose from "marsh miasmata", dirty clothes and close contact with persons.[79]

[scarlatina	febris nerviosa	febris petechialis
erysipelas	febris flava americana].	

He would allow antiphlogistic therapy only for localised fevers, recommending for the general fevers instead the use of bark, fresh air and cleanliness,[80] not unlike Lettsom and the originators of the fever hospital movement (see below). If one would adopt this method, which was "supported by reason and successful practical facts, the whole practice of medicine may undergo an entire revision, very conclusive to the future welfare of society, and to the honour of the art".[81]

The proof was simple: In 1793 Rowley compared the fatality of the common therapy with that of his method at the St. Marylebone Infirmary (as the Dispensary is now called). The former was between seventy and eighty percent, according to a "true statement of indisputable observation". In his own institution Rowley calculated less than eight percent. Therefore, he asked:

"Will any person presume to say, that there is not an extraordinary difference in the *dead list*? Will it not be acknowledged, that *saving* above *ninety lives* out of every hundred, by a new mode of treatment, is better than *losing* seventy or eighty...., by the old methods, however sanctioned?"[82]

As had Millar, Rowley persistently pursued his campaign against bleeding. His 1788 report was re-edited in 1793 and included in his collected works.[83]

In a new *Treatise* in 1804, Rowley had two more reasons to raise his voice: bloodletting had just started to become fashionable again and Britain was at war.[84] His tone reminds one often of John Millar, for instance when he wrote that “the field of slaughter.... was left to the furious venaesectionists .... and calomelists, with little or no opposition, and havoc, of course was dreadful.... but when the patients died, a prompt subterfuge was ready, namely that they were not bled or purged *enough*.”[85]

If we can trust his data he had excellent grounds for his opposition to bloodletting and allied therapy: By then Rowley had practised for nearly twenty years at St. Marylebone; from a register kept for many years by a colleague there he was able to claim that they had never lost above six in one hundred from putrid infection in the old establishment, whereas in the new one, with more fresh air and “sweet wards” they had lost but two out of between four and five hundred in a recent epidemic compared to hundreds in other institutions.[86]

Rowley wrote against authorities, past and present. According to him Boerhaave had been totally wrong. Cullen and Gregory had laudably a mixed practice, but relied still too much on the great Leyden master, whereas John Brown scarcely merited any attention from men of science and experience - except that his errors had proved destructive to an incalculable number of human lives.[87]

Rowley had some personal motive against contemporary authority, for he had been refused an M.D. at Oxford (despite holing an M.B. from there in 1788) and consequently the Royal College of Physicians had not accepted him as a fellow. As did other “marginal men”, like Lettsom and Millar, Rowley therefore fought for recognitions of his merits. “By their works shall ye know men, not by professions: and, by a *comparative view* of the malpractices, erroneously adopted, with the present improvements in curing [these] diseases,”[88] he wrote. Thus, as for Lettsom and Millar, the two sets of figures mentioned above were for him the only proof of the value of success, be it for his personal practice or for science generally: “If it be proved that thousands are destroyed by the malpractices; and as many preserved by judicious treatment, which ought the practitioner to follow?”[89]

Such reasoning would also have convinced a friend of Lettsom and Millar, William Black, who likewise attempted to increase accuracy in medicine by the use of numerical arguments.

e. William Black

Strictly speaking William black (1749-1829) does not belong to this section of the chapter on hospital medicine, for he was - as far as we know - not associated with any general charity institution. In fact he was one of two consultants to a midwifery charity and he is known as a vital statistician, especially for psychiatric problems, and as a medical historian.[90] However, he was a friend of Lettsom, Millar and Sims,[91] and amply used their hospital reports as well as Lettsom's register from the Aldersgate Dispensary in his scientific work. Therefore it seems appropriate to discuss his work in connection with the latters'.

Black was born in Ireland. He obtained his medical training in Leyden, where he graduated M.D. in 1772 with a thesis on the diagnosis and treatment of fevers. He then settled in London and together with Gilbert Blane was a physician to the General Dispensary for Poor Married Women opened in 1785. He became LRCP in 1787.[Glass (1973) was unable to find any biographical information more detailed than that given in the entry into the DNB.]

As mentioned in my general introduction, black's first publication *Observations, medical and political on the smallpox...*(1781) was prompted by a literary quarrel on the value of inoculation. As Black frankly admitted, the study of smallpox mortality had awakened his interest in the application of arithmetic to medicine in general (see above). Indeed, already in this book Black paid considerable attention to the broader subject of mortality statistics and to the defects of the London Bills of Mortality, on which much of his analysis was based. He reprinted tables by the early demographers, political arithmeticians and medical statisticians from Britain, France, Germany and Switzerland.[Short, Halley, Süssmilch, Price, Davenant, Jurin, and the collection of the London Bills edited in 1759 by C. Morris.] Furthermore he made short comments, disease by disease, as given in the London Bills grouped into four periods from 1701 to 1773.[92] He thought that he could thus acquire a "gross estimate of the proportional havoc by different diseases"; and that these facts would "naturally lead to a variety of reflections upon medicine and medical practice". He realised that from the Bills he could gain only "dubious and perplexed" data, so that it was impossible to form conclusions beyond *probable* calculations and propositions.[93]

Yet there were some reliable data on the incidence of "fevers" among hospital patients by Lind for the Haslar Hospital at Portsmouth, (see below) and Haen for Vienna hospitals, and for bladder stone by Dobson for various British hospitals (see below) which Black promptly included with praise.[94] The study of the bills of mortality drew his attention also to the confusion and imprecision of diagnoses such as consumption, fever, asthma, and rheumatism.[95]

It is pleasant for a historian to note that after a critical analysis of the *status quo* Black conceived a programme for the improvement of medicine from the study of its historical background. Indeed, as the title of his next book (1782) indicates, Black made an *Historical sketch* of the methods used in medicine and surgery, “from their origin to the present time” including the “principal authors, discoveries, improvements, imperfections and errors”. Black stressed that it had been the combination of observation, experiment and inductive reasoning which had been successful so far in medicine. This was in tune with a number of contemporary works, for instance those of his friends Lettsom, Millar and Sims from the Medical Society of London.

Again as Millar had done, he also concluded that, despite contradictory theories and inherent difficulties, a number of diseases were well known and distinctly described: “Amidst all the tumultuous anarchy of accessory or secondary symptoms [of fever], men of judgement can in most cases discern the true elementary type”. He asserted that, in addition, epilepsy, measles, smallpox, and the venereal disease showed that diseases, whether external or internal, whether acute or chronic, presented themselves over and over again in the same form as to their essential symptoms. This was also true for gout, scurvy, plague, stone, gangrene.[96] Moreover the causes of several diseases had been elucidated. One could “even *measure* with tolerable accuracy, the annual waste among the human species”, and in his opinion the effects of many medicines rested “upon proofs equally solid”.[97]

Yet with all these good natural histories at a physicians’ disposal he felt that, “our principal defect *at this day* is in remedies, remedies, remedies. In the more effectual means of curing the above diseases, we have not greatly outstripped the ancients”.[98] He lamented that therapy, designated the “end and essence of physic”, was still dominated by fashions and that readers were “frequently bewildered in ambiguity and uncertainty”; therefore it was hardly surprising that learned writers derided it or classed it with necromancy and astrology...[99] It was indeed easier to describe diseases than to cure them. But Black believed that statistics would bring the much needed certainty to both nosography and therapy, if the bills of mortality were conducted on a proper and larger scale. They would demonstrate the civil, and salutory state of man at all ages, the incidence of diseases, "the effects of diet, drink, and medical practice, the comparative salubrity and insalubrity of city, town and country air...and their effects upon different ages ....the comparative ravages of [diseases]; we could then, independent of venerable opinions, form prognostics upon mathematical grounds."

This was necessary to distinguish the frequent from the rare diseases, the important from the unimportant, which were neglected by the mere classification of the nosologists. As he put it most suggestively later: "in medical books, almost universally, the extensive desolution of the most rapacious tyrants and conquerors are confounded with the uninteresting history and petty deprecations of a robber."[100]

In order to compile a reasonably graduated list of diseases and establish valuable comparisons, he thought that the systems of the nosologists - albeit necessary for order and method - had to be derobed of their superfluous and exaggerated ramifications. (Thank God, diseases were not as numerous as the vegetable tribe, he said in obvious reference to the dependence of nosologists on botanists).[101]

Thus Black's idea of using arithmetic in medicine, both in nosography and therapy, loosely hinted at in 1781, was more definitely taken up in 1782. Then in 1788, in the annual oration before the Medical Society, it was explicitly the core element of a programme for the improvement of medicine. The lecture was first published in enlarged form in the same year, "at the unanimous request" of the Society.[102] In 1789 already, a second edition appeared in which the programmatic remarks were coherently arranged in a separate introduction. This book entitled *Arithmetical and medical analysis of the diseases and mortality of the human species...* was subdivided in two main sections. The first, concerning the bills of mortality, provided a general demographic background, and has attracted the interest of historians. The second section, was in fact much larger and showed the critical but constructive application of the programme to individual diseases - a continuation and elaboration from the book of 1782 with new material collected in the meantime. Thus this book is really a monograph more on clinical than on vital statistics.

I have already quoted, in chapter two, two passages from Black's introduction, setting out his programme and its personal history. Here I shall just complete them by following up the development of some of the ideas expressed already in his *Historical sketch*. Now his plan envisaged not only the study of the bills of mortality, "but likewise of the collected records of hospitals, dispensaries, and individuals". And this in fact was not sufficient, if restricted to one geographical area and a short time: "We should include an interval of many years, collective numbers, large groups of mankind, and of morbid cases".[103] As he had already departed from Boerhaave's ætiological views in his *Historical sketch*,[104] he now did so from Hippocrates' prognostics, which were anyhow confined chiefly to fevers in his opinion. He admitted:

"I am aware of the imputation of heresy, in calling the aphoristick prognosticks contracted and pinioned. Without medical arithmetic it is impossible to reach the 'grandeur of generality', the sublime of medical divination.... It is necessary in treating of morbid, prognostics, not only to ascertain the general danger, the absolute comparative mortality by different diseases, but likewise to enter into more minute detail, and to measure the proportions of cures, incurables, and deaths."[105]

Thus by medical arithmetic even practical branches of medicine, might be rendered as certain as any other branches whatsoever of philosophy or science. The prosecution of such a plan throughout Europe, and the combined information contrasted, assimilated, and harmonised, seemed alone to be lacking to emancipate the profession from metaphysical infatuation, and the sneers of conjecture.[106]

The dawn of medical arithmetic, he believed, could be found in Jurin's demonstrating "in numbers the comparative success under inoculation, and the natural disease". Since that time several fragmentary attempts had been dispersed in the miscellaneous writings of Thomas Short (1690-1772), a vital statistician. Black mentioned also that medical arithmetic had been prosecuted with indefatigable industry by Dr. Robertson of the Navy and Dr. Millar of London, concerning fevers in various parts of the globe, and the comparative success by different febrile remedies.[107]

With respect to fever it is noteworthy that Black, who had referred for its treatment in 1782 to the authorities of Lind and Pringle,[108] now completely changed his opinion, on the evidence of these numerical writers:

“The false lights hung out successively by multitudes of authors, and transmitted, in some degree, through the Boerhaavean school, to steer with the antiphlogistic compass and lancet in each hand, in the generality of fevers, have been the cause of numerous shipwrecks. Even that excellent modern author, Pringle, as Dr. Millar demonstrates, must, in this instance, be followed with extreme caution.”[109]

It is no wonder that authors who tore monumental authorities down from their pedestals had to face rancorous opposition. This did not surprise Black who was acquainted with the history of arts and sciences. He thought that such had been the initial reception of many other useful discoveries, and of the most enlightened reformer and benefactors of mankind... [The histories of the reception of Peruvian bark and of the concept of the circulation of the blood furnished Black good illustrative examples.]

“With respect to medical arithmetic, what time must yet revolve before ignorance and bigotry shall be enlightened, prejudices and inveterate habits done away with, envy, malevolence, and calumny silenced, I cannot determine.”[110]

Black himself by no means remained simply a theoretical writer. Going through the diseases as listed in the bills of mortality he inserted numerical comments on natural history and therapy wherever possible and available. He tells us; “I was anxious to determine with some probability the ratio of desolation [i.e. the mortality] in London, by each of the febrile *genera*; because it would be an important guide to the prevention and cure”. As a friend of Blane, he knew “that most hospital registers were particularly defective in this respect”.[111] So he was happy to be able to peruse the books of Lettsom’s Aldersgate Dispensary where registers were kept at least until 1788.[112] He realised that the distinctions of fevers described by the nosologists were not entered more precisely there as the three classes of intermittent, inflammatory and continuous fevers. From his own work, from the data from Millar’s Westminster Dispensary and from John Clark’s Newcastle Infirmary (see below) from Robertson, Sims and others he rejected “the supposed innumerable varieties of fevers,.... from which perplexity Sydenham could not altogether extricate himself”.[113] Moreover, the numerical reports of these authors also testified to the success of treatment without bleeding.[114]

Black's enthusiasm for "medical arithmetic" did not preclude a critical use of the few available data even of those of his friends. With hidden, yet clear reference to the Monro-Millar squabble, he wrote in 1789 that in contrasting the success of medical practice in different hospitals in Europe "domestic as well as military,... critics have forgotten to ascertain the diseases which were admitted or excluded, and the proportion of the former; consequently their inferences are imperfect and erroneous".[115]

As he had for fever, Black relied on the data from the Aldersgate Dispensary for a better description of true asthma, dropsy and jaundice. Otherwise he took the data where he found them: for whooping-cough he used the records of Armstrong's Dispensary for the Infant Poor. For palsy he included Charleton's data from Bath (see below), for urinary calculi those of Dobson, and finally for information about insanity, he studied the registers of all patients kept privately by the apothecary at Bedlam.[116]

This latter interest he developed particularly, prompted by a parliamentary enquiry into the curability of insanity because of King George III falling ill in early 1788.[117] In 1788 Black sent his first results in an open letter to the Prime Minister.[118] He was flattered by the interest shown in the data of the 1788 edition of his oration. He worked them over again and again for the second edition, where he presented them in nine tables.

"I may with safety assert, that mine are the only numerical and certain data that ever have been published in any age or country, by which to calculate the probabilities of recovery, of death, and of relapse in every species and stage of insanity, and in every age."[119]

This interest in statistics on insanity, the first attempt to apply the new method to psychiatric problems,[120] is especially remarkable in the context of Pinel's later use of it for the same purposes. I have not, however, been able so far to trace any reference to Black by Pinel. It may, however, be noted that Black, in 1810 would publish and extension of his work on insanity, based again on the bedlam registers (see p.??).

In 1788 he regretted not having space to discuss the surgical diseases, as his comments on internal ailments had taken over 400 pages. This is the more regrettable for us, because, Black said he had meant to gauge the success and failure of amputation, both after accident and for chronic diseases.[121] In 1789 he added more explicitly:

"This is a most important part of military surgery, and in great measure unexplored. Through abscess, ulcers, and sores of which there is such an overflow in our hospitals; and throughout cancer and lithotomy likewise, I should have applied the sure and certain test of medical arithmetic even taking account of the age of the patient." He proposed to treat all therapeutic issues together in a future volume, which, however, never appeared.[122]

As to the utmost political and military importance of recognising the services of military medicine (since more lives were lost by disease than by weapons) he referred to a book by Millar soon to appear, "from which the public and the author of these observations, will derive important instruction".[123] This remark shows that these two medical arithmeticians were in contact. Black had also received Millar's privately printed *Reply* (to Dr. Monro) (1783), for he recommended it – among a list of other works – to his readers. Millar in turn quoted the metaphor used by Black, that "when a mathematical reason can be had, it is as great a folly to make use of any other, as to grope in the dark, when you have a candle standing by you".[124]

Their common interests were not limited to medicine. As did Millar, Black wrote repeatedly on political and military (reform) topics during the revolutionary wars of the 1790s, illustrating thus the kinship of their medical arithmetic with Petty's political arithmetic. The D.N.B. suggests that Black may have had considerable influence in his time.[125] Indeed, his two medical books soon had second English editions, and his critical medical history was translated into German (1789) and French (1798). He also gave private lectures in the late 1780s, in which fevers figured largely.[126] It is easy to understand that he would have professed his creed in medical arithmetic on such occasions. Black and Millar earlier on were the most clear and outspoken protagonists of "medical arithmetic" among a group of London doctors who all became associated with one particular professional society, Lettsom's Medical Society of London.

f. The Medical Society of London

The group I described in the foregoing section centred on the Medical Society of London, founded in 1773.[127] Lettsom, its founder and several times its president, [1775, 1809, 1813.] and Millar, its first president, [1773-1775], not only read enthusiastic papers on arithmetic as a basis of judgement in therapy, but put their plans into practice in a way perfectly adapted to the standard of clinical medicine of their time. James Sims (1741-1810),[128] another of the Society's presidents, took up several of his predecessor's ideas. As had Millar, he also stressed the importance of mass observation and of simultaneous untreated cases as a standard of comparison for medical treatment.[129] During Sims's long-lasting presidency (1783-1808) William black was invited to give the annual oration which led eventually to his *Arithmetical and medical analysis* (1789). Yet my discussion of the papers published in the Society's *Memoirs* (1787-1805) showed that they were comparable to those of the preceding *Medical Observations* of the Society of Hospital Physicians (1757-1784), at least as far as the quantitative basis of argumentation was concerned (see above p.57). This would suggest on the one hand that the scientific activities within the two societies were not different. On the other hand, in Millar's words the Medical Society's "professed design ... [was] to encourage the dissident, to aid the diligent", indicating that some of the members saw themselves as outsiders.[130] Indeed one form of "dissent", the doubt over the value of traditional therapy, was finding open expression within this Society. The members present at Black's annual oration of 1788 also heard of a possible new solution:

"Physicians have been too long running astray in speculative or frivolous employments of philosophick drudgery.... Medical arithmetick establishes on a solid foundation a multitude of the fundamental principles...of medical architecture; and erects platforms for compleating the entire superstructure...In its most extensive application.... [it] may be termed what trigonometry, geometry and the telescope are to the arithmetician and astronomer, or the compass and quadrant to the navigator."

And, taking his distances from the continental learned systematists, he added: "By this criterion we should prove our superiority over the physicians of the Continent".[131]

2. In the Provinces

a. John Clark at Newcastle-upon-Tyne

John Millar's ideas also found supporters outside the Medical society of London. Among them were two naval surgeons, Robert Robertson and John Clark,[132] who confirmed his observations that remittent and continuous fevers did not change with climate. In this section, I shall discuss some aspects of the work of John Clark (1744-1805),[133] Robertson being dealt with in the chapter on naval medicine.

Like Millar, Clark was Scot who had studied in Edinburgh with John Gregory. He then served as surgeon on a ship of the east India Company (1768-1772) before settling at Newcastle-upon-Tyne. He founded a dispensary there in 1777 – against the opposition of the older established Infirmary (of which he eventually became one of the physicians, too). As did Percival in Manchester, he founded a literary and medical society at Newcastle. His first scientific book, *Observations on diseases in long voyages*...., appeared in 1773. [Further editions appeared during the war years 1792, 1806].

While with the H.E.I.C., Clark had kept meteorological and case registers "and it served to beguile the tediousness of many a vacant hour at sea, to collect and arrange them".[134] Having initially seen three fever patients becoming unconscious after copious bleeding as recommended by Pringle, he decided no longer to rely upon any system, but upon his own observations only. He soon changed to Peruvian bark, and was now able to report twenty illustrative cases from a variety of climates and countries; these included fatal cases, too, for he did not consider the citation of successful cases alone to be a sound basis for evaluating a therapy. Moreover Clark gave a succinct numerical statement of all cases of fever and dysentery with their events over a certain period. Whilst in India, Clark compared to overall mortality of seven ships, where no bark had been used, with that of his own ship "Talbot Indiaman". Mortality was generally slightly lower on the latter. When comparing the "Talbot" (11patients dead out of 108) with another ship sailing at the same time and by the same route (40 dead out of 117) the difference was especially striking. Admittedly, some sick seamen had died on both ships from other ailments and accidents, too, but nevertheless fever and dysentery had been the most prevailing disorders.[135]

In all his later publications, Clark never failed to draw attention to the indispensability of numerical returns for the improvement of medical science.[136] In these he readily acknowledged his indebtedness to Lind and Blane, Price and Percival, as well as to his "ingenious and accurate" friend Haygarth of Chester, and the "penetrating genius of Dr

Millar".[137] And what is more, he set an example himself: firstly in his determination to prove the usefulness of his dispensary at Newcastle against the opposition of the staff of the Infirmary, and secondly to analyse the *success* of his practice. This he deemed especially necessary for the "revolutionary" treatment he recommended in continuous fevers, i.e. bark and abstention from bleeding. He asked whether the statistics on inoculation were not a good example of how to evaluate results of therapy.[138]

His first report from Newcastle was included in his *Observations on fevers...* (1780), dedicated to John Gregory.[A third unaltered edition appeared in 1809.] It had evolved from his work in the East Indies: there were not only 48 detailed cases for illustrating his therapy, but Clark felt that "in order to determine its success from the result of general practice, it will be proper to give an account of the proportional number of patients who recovered, to those who died". During the two years from October 1777 to 1779 he had 203 cases of continual fever, 196 of which were cured, one discharged "for irregularity" and six died (fatalities which were analysed in detail).[139] Similarly Clark gave the results of all his scarlet fevers attended with ulcerated sore throats in both the dispensary and in his private practice.[140]

[Dispensary:	95 Patients, 81 cured, 1 discharged for irregularity, 13 died.
Private Practice:	36 patients, 32 cured, 4 died].

Moreover, in a programmatic appendix to his book containing "remarks on the method of improving medical returns" he included five tables summarising his practice at the dispensary during the first two years. Three of those leaned on Lettsom (i.e. listing all cases according to diagnosis [Classified according to Cullen's and Gregory's nosologies.][141] and event on a month to month basis), one on Millar (the results given with the additional category of "relieved" i.e. of incomplete cure). The table of the fatal cases included sex, marital, status, and age of the patients (Lettsom), and furthermore the day in the course of the disease on which death occurred. Additionally, there were two tables of deaths in each season in Clark's practice, and, in order to increase the number of observations, in the practices of all other physicians working at the Dispensary. Nevertheless Clark admitted that two years were too short a time for any conclusions as to the disease-season relationship.[142] Another original feature of his compilation was his breaking down of all the cases from the general table according to sex and to age.[143]

These tables clearly show the overlapping of the Hippocratic and Galenic pathogenic concept of disease with the new ontogenic approach. This was an attempt to fulfil the requirements of Hippocratic medicine, regarding each case separately, and those of arithmetic analysis of mass observation – a question still of foremost importance to the clinical researcher today.[144]

As would black, Clark pointed out that the reverend Price, (1723-1791) and doctors Haygarth (1740-1867) and Percival had shown the advantage of correct bills of mortality for political economy as well as for medical science; and that it was astonishing that so material a defect in the registers, as better specification of disease or the ages of those who died, should have been overlooked for so long in these bills. This also applied to their reports of great hospitals, from the registers of which such information "might be extracted... with little additional trouble". From a medical point of view, in Clark's opinion, accurate returns not only of the dead, but of the sick "properly executed, in a tabular method", would produce even greater advantages, for the understanding of diseases and thence of their cures. Hitherto results had been indicated "so exceedingly vague[ly], that it is impossible to judge of the success of the practice".[145]

Clark's connections with the East India Company allowed him, when preparing a second edition of his earlier book on *Diseases in long voyages* (1792), to examine the Company's surgeons' day-books, which had started in 1770. His motivation was to verify the success of the treatment of fevers he had recommended in 1773. A young physician went through the returns for 1770 to 1775 for fever only, since every journal had to be looked at, partial extracts deserving no confidence in Clark's view.[146]

In total he could report on 189 cases in which treatment and "event" could be precisely traced: 84 had died, 105 recovered. In all of the latter Peruvian bark had been prescribed; in quick recoveries bark had been prescribed alone, in slow recoveries combined with antimonials and bleeding. In many of the fatal cases, the bark had also been given, but in only two cases at the onset of the disease, and in all the other fatal cases only one to two days before death. Thus early administration of the bark seemed the successful therapy.[147]

At the same time Clark suggested an improvement in the Company's day-books which had consisted only of a chronological recording of cases. Now he also favoured monthly analyses, grouped according to diagnoses, and a similar but longer summary at the end of each voyage. [A specimen was attached as Table II at the end of the book.]. This would give the ship-surgeon and his chiefs insight into the morbidity and success of treatment, but more important it would allow a central report containing the material from all provenances to be drawn up. The periodical publication of such a report would encourage the medical officers, and treatment would attain the highest possible perfection, enabling an immense number of lives to be saved for the community.[148]

This was exactly the method used since the 1770s by Robertson in the Navy and by Lettsom and Millar in dispensaries. Is it astonishing therefore that Millar, who grasped evidence for the validity of his arithmetic method where he could find it, repeatedly expressed his satisfaction for Clark, whose work had been so singularly parallel to his own?[149]

b. Thomas Percival at Manchester

As stated above, Clark, Millar, and Lettsom recognised their indebtedness, from the methodological point of view to two earlier pioneers of social medicine, or medical philanthropy namely Thomas Percival and John Haygarth.

Percival (1740-1804)[150] was born at Warrington and was taught at the Dissenting Academy there by Pristley (1733-1804). He studied medicine at Edinburgh, London and finally Leyden, where he graduated M.D. in 1765. He then settled in Manchester. In 1767 appeared the first of a series of his *Essays medical and experimental* which must have attracted wide attention since they were translated into French and German and re-edited five years later. From the point of view of my thesis his programmatic dissertation advocating rational empiricism is noteworthy, as also is his assertion of the advantages of inoculation on statistical grounds.[151]

A second series of Essays (1773) contained the “proposals for establishing more accurate and comprehensive bills of mortality, in Manchester” to which medical authors such as Lettsom, Millar and Clark subsequently referred for the shaping of their hospital reports.[152] Percival himself had been stimulated by an actuarial treatise of his friend, the reverend Richard Price (1723-1791), a non-conformist minister and writer on morals, politics and economics,[153] who in turn would extend later Percival’s own census work on Manchester.[154]

Percival suggested specification of sex and age, of mortality grouped according to diseases, seasons and age-groups, and a reconsideration of the traditional list of diagnoses.[Furthermore should still-born children and those deceased before baptism be included on the list of christenings. This kind of census was already done in Austria, Germany and in the Republic of Berne.[155]] Even considering only Maty's article quoted above, Percival's suggestions were not, strictly speaking, original. [*The Philosophical Transactions*, too, had contained similar proposals 20 years earlier, even with a hint of their potential usefulness for physicians.[156]] Yet Percival himself undertook and financed such a census of Manchester in 1773. His paper thereon appeared in the *Philosophical Transactions* in 1775 and 1776.[157] He compared Manchester itself with its environs, much to the disadvantage of the town, although the climates in the two areas were the same.

Greenwood suggested that Percival was perhaps not an outstanding medical statistician, even by the standards of his time, but that he realised the importance of the method.[158] I might add, however, that he was influential, for his suggestions were adopted by his friends, in Chester, Warrington, Liverpool and York, who arranged similar censuses.[159] Moreover they were taken up by clinicians as outlined above. Indeed he was a personal friend of his former Edinburgh classmate William Withering (1741-1799) of Birmingham, Haygarth of Chester, Matthew Dobson (1735-1785) and James Currie (1756-1805) of Liverpool, and of his Manchester contemporary White.[160]

In 1781 Percival was one of the founders of the Manchester Literary and Philosophical Society (see above) and afterwards its president for 21 years. He is best known for his creation of a board of health in Manchester, together with his collaborator John Ferriar in 1798, which produced the first regular health reports in the Kingdom.[161] He also played an important part in the opening of a short-lived 18th century venereal hospital (1774-1777) and of fever hospital in Manchester. In 1803 he again urged for a special venereal ward at the Manchester Infirmary.[162]

Percival's work on medical ethics (1792, 1803),[163] the outcome of some internal dispute at the Infirmary, later formed the basis of the code of ethics of the American Medical Association. Its Article XIV recommended medical registers more or less directly on the line of his proposals for bills of mortality of 1773. The aim, by then, was obvious:

"Physicians and surgeons would obtain a clearer insight into the comparative success of their hospitals and private practice; and would be incited to a diligent investigation of the causes of such difference."[164]

Unfortunately Percival was prevented from putting his own plan into practice. He had to resign in 1780, after two years, from his practice as a physician to the Manchester Infirmary because of an eye disease. The implementation of his plan was left to his younger friend and collaborator Ferriar (1761-1815) (see below), who had also contributed many ideas to Percival's writings on professional conduct.[165]

Percival, Ferriar, Black, and Lettsom have long been ranked among the British pioneers of public health and/or vital statistics.[166] Yet their contribution to the application of simple arithmetics is virtually unknown, as are the works of Millar, Rowley and John Clark along this line. This also is true for some other pioneers of social medicine such as Haygarth and Currie.[167] Their activities, associated with specialized institutions, I shall describe in the following section.

D. **WORK AT SPECIALISED INSTITUTIONS**

1. FEVER HOSPITALS (1784-1830)

a. John Haygarth at Chester

Percival had a warm friendship with John Haygarth (1740-1827), "clinician, investigator, apostle of sanitation" in Chester.[168] Some medical men of these towns and of Liverpool used to meet four times annually for 14 years at Warrington for discussions,[169] and it is therefore justified and convenient to speak of a "Warrington group". Haygarth had studied at Cambridge, Edinburgh and London and he was a physician to the Chester Infirmary from 1767 to 1798, during which time he constantly recorded all hi cases including the results of treatment. He then retired to Bath and started working up the histories of some 10,549 patients (see below p.??).[170]

In 1774, one year after Percival in Manchester, he undertook a census of Chester which appeared as ‘Observations on the population and diseases of Chester in the year 1774’ in the *Philosophical Transactions* of 1778. (The paper referred to by John Clark). Haygarth argued, like John Millar, that if the old theory of “constitution of the air” being responsible for the propagation of contagious fevers was not abandoned, “prayers to Providence” would be the only possible intervention for a physician.[171] He formed a smallpox-society in Chester and became a recognised authority on prevention of the spread of that disease. His treatise thereon (1784) was translated into French and German, and foreign governments asked his advice in a particular instance in 1791.

Similarly Haygarth was also active in preventing the spreading of epidemic fevers by admitting the early cases into special wards in the Infirmary, and by attempting to cure them rather than leaving them in misery at home. [Previously, fever cases had not been admitted to the Infirmary. The precise new regulations were thought exemplary by Haygarth’s friend John Howard (1726-1790), the great prison reformer, in his survey on *Lazarettos* (1791).[172]]

This was, in 1783, the beginning of what M.C. Buer labelled the “Fever Hospital Movement”.[173] Such institutions were established with the help of his friends Currie, Percival, and Ferriar in Liverpool (1787); and in neighbouring Manchester (1796), where “the advice of Haygarth supported by actual results secured in Chester, was most influential in bringing it about.”[174] In London a “House of Recovery” was opened in 1802, followed quickly by those in Dublin, Cork, Edinburgh, Leeds, Stockport, Newcastle-upon-Tyne and many other towns.[175] The plan for the London institution had been prepared by Currie. It was published by Lettsom, who later served on the committee of this fever hospital. Lettsom, who later served on the committee of this fever hospital, Lettsom in turn was considered by Haygarth on of his “most intelligent friends”[176] (together with the physicians William Falconer of Bath, William Saunders of Guy’s Hospital, William Heberden, Robert Willan, and the surgeon John Aikin, most of whom shall be discussed in this thesis).

For Haygarth the establishment of the fever wards was a large scale experiment designed to prove his theory of the contagious nature of fever; and to that end the publication of results in the form of periodical numerical reports was essential. He was proud of the success of this experiment, achieved on “scientific principles, by facts and conclusions from them”.[177] Moreover, the results yielded strong propaganda for the establishment of fever hospitals in other towns, especially, of course, in the hands of a man like John Clark of Newcastle (1802), of John Ferriar who "arranged" his account of the Manchester fever wards (see below).

These results fall into the category of public health statistics, for Haygarth attributed the diminution of mortality from fever in in the first place to the success of his preventive measures, rather than to specific treatment.[178] Their numerical presentation is perhaps less remarkable than the fact that Haygarth's belief into the contagiousness of smallpox and of continuous fever was based upon an inquiry in which he had cut his Gordon knot with the help of simple mathematical analysis of his own observations. With respect to typhus he had systematically studied fourteen families during an epidemic at Chester (Tables I-III); he found that only eight out of 188 comparable members of these families had remained uninfected, i.e. only one in more than twenty was naturally exempted from the disease, and he concluded that if two persons living together escaped such a fever, the probability that they had never been exposed to "an infectious quantity of the poison" was about 400:1, if three persons of a family had escaped, this probability rose to above 8000:1. He said he owed this calculation to a "mathematical friend", and he tried to make it persuasive by illustrating it with a day-to-day example. [He (wrongly) said that the first of these probabilities corresponded to that of consecutively removing two black peas from a box containing 38 white ones and two black ones. The second probability corresponded to that of consecutively removing three black peas from a box of 57 white and three black ones.[179]] Such was the basis "computed arithmetically by the doctrine of chances, according to the data"[180] which had led him to advocate the immediate isolation of smallpox and fever patients, as a means of hindering the spread of the infection.[181] "I request the reader's particular attention to this point, as many of the following arguments principally depend upon it", Haygarth stressed.[182]

In a later section of this chapter, I shall discuss Haygarth's numerical therapeutic trials and nosological work which exhibited the same features. Interestingly, these were not actually a result of his practice at the fever hospital, but of methodical recording of all his private cases.

b. James Currie at Liverpool

After Chester, the next facilities for fever patients were opened in Liverpool in 1787 in the form of two fever wards at the Infirmary and later at the Workhouse.[183] Their physician James Currie (1756-1805), another Scot, was younger than Percival and Haygarth, but was nevertheless their friend (from the Warrington meetings); he also met and corresponded with John Clark, and was a member of the Medical Society in London.[184]

Currie used these wards for therapeutic experiments: He developed two new aspects of the clinic of fever, later adopted by Bateman in London, and elsewhere, namely the diagnosis by the use of the thermometer, and the treatment of elevated body temperature by cold water baths. The principal view of his *Medical reports on the effects of water, cold and warm as a remedy in fever, and febrile diseases* (1797) [This work was in its 4th edition by 1805, and also translated into French and German.] was precisely "to establish the use of [this] new and powerful remedy in fever".[185]

In his *Medical reports* Currie set himself against the nosological discussions of the systematists, which seemed to him to be disputes of mere words; instead, he preferred to read in the "volume of nature"[186] and clearly his *Reports* were influenced by the clinical writings of his older friends. He arranged the monthly admissions at the Public Dispensary since its opening in 1780 in tabular form, and he made a similar table for the occurrence of typhus. Numerical "proofs" of the utility of the Manchester Fever Hospital (opened 1796) and of the fever wards that he had directed in his own Infirmary in Liverpool (since 1787), and of the Liverpool workhouse (since 1783) rounded off this report. Currie wrote that he had got the idea of cold bathing for fevers from the Army surgeon William Wright (see below), who had described three successful cases (among them himself!). In 1787, the year of the opening of the Liverpool fever wards, Currie successfully tried it seven times though losing one patient (who had also been treated with bark!). From then he always used the bathing therapy whenever the patient's strength was not too reduced. By 1797 he had recorded 153 cases in which he attributed the cure chiefly to this remedy. But later,

"Having satisfied myself of its extraordinary efficacy, and of the precautions necessary in using it, I have found it the shorter method as well as the more instructive, to record the instances [only] in which, it has proved unsuccessful."[187]

Indeed, the results in his fever wards in the four years from 1793 to 1797 were good: 530 patients, 51 of whom died, i.e. an average mortality of 1 in 10½ (16 of whom, having arrived moribund, died within the first 24 hours).[188]

In a second volume bearing the same title (1804) Currie kept to the same pattern of presentations, i.e. a general numerical survey and ample illustrations by detailed cases, especially by all his unsuccessful ones. By then he had also used bathing for dysentery (110 cases in 1801 with ten deaths)[189], and tetanus.[190]

Currie admitted in 1804 that in all these cases other remedies had been used too, “for it would be unjustifiable for the sake of experiment to neglect any means of safety”, but they had been of the most simple (unspecific) kind.[191] Typical of the higher rated work of his time was his own recognition of the insufficiency of his efforts and of the need for further investigation. Currie deemed his Observations not extensive enough to provide “a complete view of the subject of which they treat”, and, although “in this rapid sketch assertions are sometimes given instead of experiments and proofs...., it will be found that the issue is clearly formulated so that it can be easily brought to the test of experiment”.[192]

Such honesty showed itself again in his quoting the only negative account of the effects of bathing he had ever read (by McGrigor) and which, he emphasized, should not be concealed.[193] Some other literary evidence however, some of which was numerical, permitted him to conclude on an optimistic note. For instance he cited evidence from two epidemics of yellow fever in Philadelphia. The first epidemic (1793) had been the famous instance when William Cobbett (1762-1835) an English pamphleteer and politician used statistics to prove the fatal effects of Benjamin Rush’s method of bleeding and purging.[194] During the second epidemic (1798) the morbidity was four times less, but the absolute mortality the same. For Currie this was not due to insufficient bloodletting as Rush pretended, but to the fact that no cold bathing was practised..... .[195]

It is perhaps not inappropriate to conclude this synopsis of Currie’s work with such a speculative statement to round off the picture drawn of him. As indicated above he was himself aware of the limited value of his evidence, yet with that presented his contribution stood higher than that of many a contemporary. James Hamilton (1749-1835), a physician to the Royal Infirmary at Edinburgh for instance, was as thoroughly traditional in his treatment of fever, as in the type of evidence he cited of its efficacy. He presented only successful cases to prove the *Utility.... of purgative medicine in several diseases* (1805). For him, the value of the cold bath was ascribable only to its purgative effect.[196] But, as indicated in my introductory remarks on fevers, the “bloodletting revolution” supported Hamilton’s views of the value of evacuations with its own statistics, so that by 1829 his book was in its eighth edition. By contrast, Currie's cold water bath was abandoned even by his friends from the London fever Hospital, despite its initially enthusiastic receptions and its documented efficacy.

c. John Ferriar at Manchester

Finally Manchester, the city of Percival, the *spiritus rector* of the "Warrington Group", acquired its fever hospital, too. It was opened independently of the Infirmary in 1796. Backed by Percival, its chief promoter had been John Ferriar (1761-1815), a Scot who had graduated in Edinburgh in 1781.[197] In a numerical account he showed the success of this institution: the absolute number of fever cases in a given area of that town had apparently diminished fifteen-fold during the first eight months of its existence (July 1796-March 1797) as compared with eight months (October - till May!) in the three preceding years. Unfortunately, Ferriar gave no later data of morbidity although the work included the annual mortalities at the fever hospital from 1796-97 to 1805-06. Their huge variation (from 1 in 5,5 to 1 in 20) was explained by differences in the weather, the "prevailing epidemics" and the admission policy,[198] and explanation which one might also turn against the statistics forwarded as proof of the utility of the institution...

Although Ferriar worked according to Haygarth's guidelines, he deserves credit as promoter of such fever hospitals: he had supplied the plan for Currie's in Liverpool, and that of London became consequently also closely modelled upon the Manchester institution.[199] I have outlined Ferriar's research programme aiming at more objectivity in Manchester in Chapter one. However, the idea of the usefulness of separate fever wards prevailed too much to allow him a critical evaluation of his results concerning typhus. As to some of his other clinical investigations, I shall discuss them in a later section of this chapter. Here it is convenient to make a halt and to consider the men behind these provincial fever hospitals as a distinct group.

d. The Warrington Group

The architects of the early fever hospital movement thus knew each other and met quite regularly, at Warrington in 1780s "in order to explain our professional difficulties and success, and to consult together what might be the best remedies for such dangerous disorders as had occurred in our practice".[200] This kind of informal gathering reminds of those of the Lunar Society of Birmingham,[201] with which the Warrington Group was also associated through at least one of its members. The name "Warrington Group" which I suggested recalls the well known 18th century Dissenting Academy of this town. This is not inappropriate because Percival had been one of its pupils and he, Ferriar, Haygarth and Currie were Unitarians.[202] From the religious point of view there were thus similarities with the Medical Society of London where besides Lettsom, a Quaker, the Scottish element dominated. Most of these Scotsmen may be supposed to have been Presbyterians. But the parallel can be drawn even further. The Warrington Group, too, included physicians and surgeons alike. This reflected the Edinburgh medical scene where both groups had studied at university. Amongst the "Warrington surgeons" were Whit from Manchester, Edward Alanson (1747-1823) from Liverpool, and John Aikin (1742-1822) from Warrington itself.[203] Indirectly associated through a lasting friendship with Percival was Edward Rigby (1747-1821) from Norwich. As Aikin he was a Unitarian who had been educated at the Warrington Academy.[204]

But it was of course the common interests which made the Warrington Group the provincial counterpart of the Medical Society of London. And indeed, there were very strong links between the two: Percival, Haygarth, Currie, and Rigby[205] had been made members of the stable London Society, as had Withering, who was indirectly associated with the Warrington Group, too through his friendship with Percival. Percival and Currie at least had given papers personally in London in 1794 and 1790 respectively.[206] One practical proof of the efficiency of these cross-links was the London Fever Hospital as hinted at several times already.[207]

e. The London Fever Hospital

The London Fever Hospital was founded in 1802 upon plans drawn up by Currie. Numerical evaluations also went on there as shown in Thomas Bateman's *Succinct account...* (1818). Bateman (1778-1821), trained in London and Edinburgh, became a pupil and admirer of William at the Carey Street Dispensary and a member of the Medical Society of London. With Willan on the Board he became, in 1804, physician to the Fever Hospital.[208]

Bateman's *Account* contained mortality rates from his own and five other establishments. We find data from Alexander Marcet (1770-1822) and John Yelloly (1774-1842), from their wards at Guy's and the London Hospital respectively, in Bateman's list. This is not surprising, when their work on lithiasis is borne in mind (see below). Both had actually been among the members of the original committee of the London Fever Hospital, which had established the rule that its physicians should keep accurate registers of all in-patients and of the remedies employed.[209]

Bateman's Account did not specially mention the treatments adopted by the various doctors he listed. From a comparison of simple mortalities during an equal period of time it would not appear that his own practice had been particularly successful. [Mortalities from 1817/18 were e.g. Bateman 1 in 13½; Marcet (Guy's Hospital) 1 in 15; Yelloly (London Hospital) 1 in 17½; Westminster Hospital 1 in 19]. But he asserted that such a comparison had to consider the different ages of the patients, (children under seventeen years exhibiting a lower mortality from fever than adults over fifty). This alone could explain the differences, that the adult - to - children ratio was two to one in one fever hospital, but four to one in his own.[210] Furthermore, the success depended on the early start of treatment, as had been shown numerically in the reports from Irish fever hospital. In Bateman's hospital only 124 out of 678 patients had been admitted prior to the sixth day of the disease; on average his patients had been received on the eleventh day. If one also made allowance for the eleven patients already moribund on their arrival and the three who died from another disease, then only 36 had died of fever in his hospital. This gave a "normal" ratio of 1 in 18. These factors, Bateman concluded, unlike Ferriar before him, were probably more likely explanations of the annual fluctuations of mortality within the same hospital, than were seasonal epidemiological variations.[211] [He himself had observed variations from 1 in 6 to 1 in 13½]. This recognition accorded with a tendency towards an ontological view of disease (mentioned above).

Bateman realised that the consideration of the age of the patients was of great importance when groups of patients were being compared.

As would Cheyne and the other researchers at Irish fever hospitals, Bateman also used the numerical method to establish the diagnostic features and the natural histories of disease. For instance, in his case it served to differentiate a simple from a more severe and complicated form of "typhus". For both forms he recorded numerically objective signs such as pulse per minute, temperature in degrees Fahrenheit, appearance of symptoms (vomiting, diarrhoea, deafness) and clinical progress (in absolute numbers out of 678 patients studied). He also drew up a percentage distribution of the various durations of the disease.[212]

[37% convalescent between 7th and 14th day
36% convalescent between 14th and 20th day
11% dismissed as cured before the 14th day
12% dismissed as cured between the 14th and 20th day
41% dismissed as cured between the 21st and 30th day
only 8% died].

In the light forms of "typhus", corresponding perhaps to our influenza, 60% improved after a spontaneous free perspiration, and they recovered usually quite unequivocally without any very active assistance from medicine.[213] The severe forms, characterised by relapses, additional pectoral disorders, and/or inflammations of the tonsils and parotids, more often ended fatally. The cure used for them was bleeding and purging, as Currie's cold water bath had been gradually abandoned by Bateman.[214] The fact that Hamilton's book on purging was in its eighth English, and third American edition by 1829, and was translated into French, German and Italian is probably less a reflection of its intrinsic merits than of the "bloodletting revolution" which began in Britain, well before the era of Broussais in Paris.[215]

f. The Irish fever hospitals

In this reverse of practice in fevers, statistics played a great role, less for its initial motivation than for the *post hoc - propter hoc* proof of its validity. The *Edinburgh Journal* for instance reprinted and commented at length on the results presented from Irish fever hospitals *pro* and *contra* bloodletting.[216] One Thomas Mills, an Edinburgh M.D., analyzed the records of the Dublin Fever Hospital. He wanted to compare mortality rates from the different physicians an d at different times with his own practice there in 1813, in which he had been using the lancet. His analysis convinced him of the superiority of bleeding, but his opponent also used the Hospital's records, with more objectivity, to argue against it.[217] However, their voice was shouted down by the chorus of bloodletters from both specialised and general hospitals, which soon attained an even greater international scale.[218]

One of the isolated voices against bloodletting stated that he had found petechiae in 386 of his 540 cases, which suggests that his "typhus" may have been the real typhus and certainly a more serious disease than a common fever.[219] Such a statement constituted a very early application of the numerical method to the pathography of fevers in Ireland. There were more to come soon. No less than nine hospital reports from Irish fever hospitals (seven from Dublin covering the years 1813 till 1826) continued this type of numerical accounting of "fevers". They were published in the five volumes of *Transactions of the ...College of Physicians in Ireland*, which had been founded in 1816 especially as a forum for hospital reports and pathological researches, based upon wide experience.[220] Simultaneously the *Dublin Hospital Reports* were established with the aim of setting up standards for comparison on the practice of large hospitals. This series published another three hospital reports by John Cheyne (1777-1836) of the Hardwicke and Whiteworth Fever Hospitals in Dublin (1817 and 1818) which contained statistical accounts of the frequency of certain temperatures in fevers and their relation to mortality.[221] The rest of the contributions to this journal were mostly of single cases, as many potential collaborators now sent their full reports to the *Transactions*.[222]

As an example of such accounts I shall describe those by John Cheyne, a Scottish M.D. He had been a surgeon under Rollo's superintendance in an artillery regiment from 1795 to 1799, and must thus have been accustomed to statistical reporting (see below, p. ??), when he became a physician to the Hardwicke Fever Hospital, Dublin in 1816. At any rate, he immediately set out to record all his cases. His first report contained the 780 cases of the year 1816-17 summarised in two tables, with emphasis on the fatal ones which were all succinctly described. The treatment by bleeding was not questioned (for this was the time of the "bloodletting revolution"), but Cheyne recorded the number of bloodlettings, leachings and cuppings relative to the number of patients admitted and the number of deaths each month; at the end of the year these monthly tables were summarized.[223]

The next report for the year 1818 illustrated even more the "admirable opportunities which hospitals afford of investigating disease."[224] Now Cheyne also indicated - always in appropriate tables - the relative occurrences of temperatures in fever, at intervals of one degree from 97º to 109º Fahrenheit (out of 250 cases), frequencies of respiration (in 171 cases divided into 16 groups ranging from 20 breaths per minute to 60), and pulse rates (for 237 cases divided into 39 groups ranging from 52 beats to 180 beats per minute). 40 patients with a temperature of over 104º were listed in a separate table with their pulse and respiration frequencies and their cutaneous and internal symptoms; and their "events" were recorded and analysed. [An increase of pulse in parallel to that of body temperature was observed in 23 out of 32 cases, whereas as increased respiratory frequency occurred only in 9 out of 22 cases with increased body temperature over 104º F.] Finally Cheyne attempted to relate mortality to the body temperature on the day of admission. The mortality dropped with increase in that temperature of the day admission. The mortality dropped with increase in that temperature, being one in twelve (n=83) for temperatures between 97º and 100º, one in twenty-five (n=127) for temperatures between 101º and 104º, and one in forty (n=40) for temperatures between 105º and 109º.[225] A table containing the anatomical-pathological findings of all deceased cases completed this hospital report.[226] Which surely represented high standards of numerical investigation, and well before Louis's work in the 1820s!

g. Scottish Fever Hospitals

The *Edinburgh Journal*, from its first volume (1805), regularly published the quarterly reports of the London Carey Street Dispensary; these reports were sometimes signed by Bateman. They were later replaced by those from the New Town Dispensary in Edinburgh, begun in 1817,[227] listing the number of patients admitted under each diagnosis and giving explanations on therapy and results in the accompanying text.[228]

Edinburgh had no fever hospital when it was seized by an epidemic in 1817. Massive hospitalisations in the Royal Infirmary became necessary. At that moment it was realised that no data were available on admissions and cures of fevers during previous years comparable to those of the specialised institutions in Dublin, Cork, Manchester and London.[229] This was quickly corrected and dispensary-like data from the Infirmary and from an ad hoc fever hospital were published. At least seven Scottish authors wrote treatises like those of Bateman and Cheyne in 1818-1819; [not included nine Edinburgh Dissertations thereon from 1818-1821, which I could not examine.] these were reviewed in the *Edinburgh Journal*.[230]

In 1818 the reviewer considered bloodletting as the unanimously accepted therapy.[231] But in 1821, taking into account more detailed and numerically tabulated facts from the *ad hoc* fever hospital in Edinburgh the reviewer was beginning to doubt its usefulness. Bloodletting might suspend the symptoms of the fever for about a week; but, what was this good for, he asked, if they returned, albeit less severe, in the third week? Such instances were considered by the bloodletters as fevers "cut short, followed by relapse". Yet, he said, one could also look at them as fevers "suspended and protracted", especially when the relapses were as frequent as approximately one in four. Clearly, for the reviewer numerical statements had supported his belief that the bloodletters had been misled by their speculative opinions, or by their partiality for a particular practice, and that general, unspecific treatment had been neglected. He felt too that "there was still very much to learn concerning the combination of anodynes, stimuli, and tonics with the depleting practice,"[232] a field in which numerical analysis could be a powerful tool.

2. Numerical Investigations at Midwifery Hospitals

Besides the military tradition of bloodletting in "typhus", to which the English and American Bloodletting Revolution had its ties at the beginning of the 19th century, there was also a definite, though more obscure English tradition of copious bloodletting in cases of puerperal fever.[233] This leads me to the reports from the special lying-in charities. [A number of 18th century essays on obstetrics were reprinted in 1849, edited by F. Churchill, on behalf of the Sydenham Society.] Indeed, the first epidemic of puerperal fever was noted in London in the early 1760s, about a dozen years after the opening of such institutions.[234] Thomas Denman (1733-1815) was one of the prominent British obstetricians of the 18th century. He had been a naval surgeon during the Seven Years' War. In 1768 he directed a private lying-in house. He recommended bleeding without giving numerical details.[235] John Leake (1729-1792) and Nathaniel Hulme (1732-1807), writing from the New Westminster and the City of London lying-in Hospitals respectively in 1772, did the same.[236] However Leake at least kept statistics of admissions and mortality of the institution he had created himself. With the help of the bills of Mortality, from puerperal fever, for 1768-1771 he "proved" numerically that its cause could only be the malignant constitution of the air.[237]

At the same time as Percival (see above) Leake thought it "a public misfortune that those bills still continue to be kept in such a manner as to defeat their original intention and to render all calculation in this matter vague and indeterminate".[238] But his recommendation of bloodletting in puerperal fever - a debilitating disease in a most debilitated state already compounded by unavoidable blood-loss was not founded on actual trial. Rather, he rationalised such observations away with the classic distinction common to the later bloodletting revolution; namely that the *post-partum* debility was not due to "nature exhausted" but to "nature oppressed", i.e., by an excess of arterial activity. There was thus only "apparent debility" which required copious bloodletting to decrease this excess activity.

Furthermore, because of its alleged dependency on climate, prevention was less important in this “fatal disease”.[239]

This approach changed with Charles White (1728-1813), the eminent Manchester surgeon and obstetrician.[240] A friend of Percival and hence related to the “Warrington Group”, he saw the cause of this fever less in general bad air, than in specific overcrowding, dampness and bad ventilation of the rooms where women were about to deliver.[241] Thus he proposed that specific prevention before and after parturition should eradicate this fever, from which he had never lost one patient “to the best of my recollection”....[242]

As a proof he gave the decreased general mortality figures of the Manchester Infirmary after it had moved into new, well ventilated premises. Unlike for the cases of amputation, he was unable to give direct figures for lying-in women, since they were not admitted into the infirmary.[243] (A lying-in hospital was founded in Manchester upon White’s insistence in 1790 only).

As for the cure, if ever necessary, White was not unscientific. He recommended application of cold or warmth (internally and externally) according to the rise or fall of the thermometer. He also recommended lemon juice or vegetable acids against inflammation and putrefaction, based on the patho-physiological explanations of their effect by Pringle, Macbride and Lind, besides purging and increased cleanliness and ventilation. If the patient got worse despite these measures Peruvian bark should be tried. Bleeding seemed to him not only unnecessary but dangerous.[244]

White’s book (1773) reached its fifth edition in 1791, [It was also translated into French (1773) and German (1775) and reprinted in America (1793).] and until then most writers on puerperal fever, be they attached to a special hospital {as were Joseph Clarke (1758-1834) (1790) in Dublin, John Clarke (1761-1815) (1793) and Robert Bland (1782) in London} or not {as were Thomas Kirkland (1721-1798) (1774), William Butter (1726-1805) (1775)} opposed the use of the lancet and favoured bark. Bark was being recommended at the same time for most other “fevers”.

The latter two authors gave no numerical accounts,[245] whereas their younger colleagues Robert Bland, Millar’s London educated colleague at the Midwifery Department of the

Westminster Dispensary, and Joseph Clarke, an Edinburgh graduate, who became physician to the long established Dublin Rotunda Hospital (opened in 1745),[246] published extensive statistical reports on the practice of their institutions. They were read in the Royal Society in 1781 and 1785 respectively,[247] and both illustrated the success of their author's insistence on prevention. From 1774 till 1781 Bland registered only five puerperal fevers (four deaths) out of 1,897 deliveries. In Clarke's institution, from 1752-1784 only 229 women had died in childbirth out of 19,786 deliveries (1 in 87). Inspection of his further statistics kept during his septennium as master of this specialised hospital in Dublin, from 1787-1793, shows that his argument against venaesection and vomiting in 1790, for which he relied on his records, was soundly based.[248] Despite an epidemic of puerperal fever in Dublin in 1787-1788 he had lost less than 1% out of the 10,387 women from what might correspond to that disease, (i.e. 32 of peritonitis, 21 of synochus or typhus, 15 of hectic fever). [249]

Since his assistantship Clarke had stuck to White's methods, which had apparently been successful in both prevention and treatment. The mortality of children within the first sixteen days had fallen from 1 in 6.5 to 1 in 28 with a mortality of mothers remaining at 1 in 86.[250] His statistics, in fact were evidence in favour of prevention and general management and only indirectly for the value of his method of cure, for he gave no morbidity statistics for the disease itself.

Such direct numerical "proofs" were forwarded in 1795 by Alexander Gordon (1752-1799),[251] who was ironically not in favour of White's or Clarke's cure but supported the older method of antiphlogistic treatment; (he referred especially to Cleghorn's recommendation). Gordon had studied in Edinburgh and became a naval surgeon during the American War (1780-1785). He then studied for some time in London at the Middlesex Lying-in-Dispensary amongst others. Thus he brought with him the military tradition of blood-letting in fever, (especially strong in the West-Indies[252]), and the predisposition for record-keeping, when he became physician to the Aberdeen Dispensary in 1785.

Indeed, when again called to active duty in 1795 he had prepared a statistical, tabular account of this institution covering his stay there (1787-1794). This account was modelled on those of Lettsom, Millar and Robertson. His figures showed that his mortality from fevers had been low in his fist years. [1 in 48 lying-in women (n=290); 1 in 25 (n=382); 1 in 43 (n=348) for 1786-87-88 respectively.]

In 1789 puerperal fever broke out in Aberdeen. Gordon accurately noticed its propagation by personal contagion rather than by a noxious atmosphere. He presented its course in a table of all his patients in the Dispensary and in his private practice, with dates of illness and names of physicians and midwives attending.[253] This table also gave the event of each case. On this table his whole argument relied, for "noting ...can be a stronger proof of the truth of my doctrine, than the success of my practice".[254]

Out of 77 cases of puerperal fever, Gordon had lost only 28, which was much better even than the mortality indicated by Leake. But even better, if one subtracted the 27 cases in which he was called too late, (i.e. after 24 hours), only five died out of the remaining 50! It was not only bleeding, but copious bleeding (20-24 ounces at one time) assisted by purging which was the pre-requisite of such success. Since Gordon had discovered this (upon dissecting one patient), he had not lost one single patient out of 30 treated early enough in this manner. All patients treated otherwise, early or late had died.[255] In contrast to White, Gordon had only a few words - one page out of 55 - for prevention. He considered it to be of no avail. He would have liked to recommend blood-letting for prevention too, (the best cure being the best prevention), but thought it better to limit himself to purging - in order not to deprive himself of the only certain remedy: "The cure is severe", he wrote, "but it is only short, for the patient is cured in a few days, or not at all".[256]

This treatise was the green light to a "bloodletting revolution" in puerperal fever, too. William Hey (jun.) (1772-1844), one of the surgeons at the Leeds Infirmary and Leeds Fever Hospital,[257] described it with the highest praise when reporting on puerperal fever occurring in Leeds and its vicinity in 1809-1812. In opposition to current views he was convinced by Gordon's arguments: "I determined fully to adopt [his] plan of treatment till experience should teach me the necessity of deviating from it".[258] He added further numerical evidence in favour of bleeding: before he had used this method he had lost eleven out of fourteen patients during this epidemic, afterwards only two out of 36.[259]

John Armstrong (1784-1829),[260] physician to the Sunderland Dispensary and later to the London Fever Hospital, was one of the leading bloodletters for several diseases, and started a series of books on the practice of bloodletting with a treatise on puerperal fever (184). He referred very favourably to Cleghorn and Gordon.[261]

During an epidemic in Sunderland in 1813 he had collected 43 “distinctly marked cases” from five practitioners, of which only five had died. (Three of these five had only been purged, whereas the 38 favourable cases had all been copiously evacuated, and 29 had also received mercury). One of his correspondents “made a point of bleeding till the patient was likely to faint; a circumstance which, I conceive, is of some consequence in checking the inflammatory action”.[262]

In a second edition of this book (1819), Armstrong inserted a Letter by Joseph Clarke, reducing the whole question of such alleged successes to that of defining what puerperal fever really meant. Clarke quoted his updated statistics from 1757 till 1816 and declared he had encountered symptoms of peritoneal inflammation only, *post partum*, but hardly any of those signs of low malignant (typhus) fever, which Armstrong lumped together under the common term of puerperal fever. For his “peritoneal” puerperal fever, Clarke still favoured delaying of bloodletting, for only 875 women out of 84,390 delivered at his institution had died. Yet he positively felt bound to admit bloodletting “on your authority” for typhus.[263]

Joseph Clarke and Robert Bland had used their statistical reports as illustrations of the large practice of their charity and as raw material for statistical clinical research rather than as a proof of specific therapeutic success. This may, on the other hand, explain their interest to the Royal Society. Indeed Joseph Clarke’s first series (1757-1784) showed a mortality of mothers of 1 in 87. The mortality of children was much higher, i.e. 1 in 6.5 within the first 156 days, with a preponderance of the male over the female in a proportion of four to three, the reason for which he elucidated in the following manner: he compared the average circumference of their heads and the weight of twenty new-born males with those of twenty females and found that the former were bigger in both respects. Thus males were generally more liable to injuries at delivery and to malnutrition, and came into the world less perfect, each specific case depending of course on the health and vigour of the mother.[264]

On this issue, the statistics of Robert Bland at the Westminster Dispensary had already yielded results. Bland recorded 1,897 observations there, since its foundation in 1774. He had kept records, arranged in ten volumes, concerning personal and family antecedents of the women under parturition and especially concerning all accidents in their previous and their current deliveries. He wanted to establish precisely the proportions of natural, uncomplicated deliveries to complicated ones, {merely estimated earlier by William Smellie (1692-1763)[265]}, as well as the frequencies of various presentations of the foetus, and of certain complications during and after birth. In addition, he was interested in the proportions of male to female children, of twins, of triplets, and of children with monstrosities, and of mortality in the new born. For this he sent out more than 1,400 letters to mothers and found out that 1 in 16 had buried their children before the end of the second month, with a higher proportion of male children among them.[266]

Extensive numerical research continued steadily in this department with Samuel Merriman (1731-1818) and later Augustus Bozzi Granville 81783-1872). In the Dublin Rotunda Hospital it was Robert Collins (1801-1896) who carried on the tradition of the numerical accounts of the masters' septennium. The work of these authors concerned statistical distributions of different types of labours and foetal presentation, mortalities according to age and previous history of the mothers, comparative frequencies of abortion at different period of pregnancy, and its causes.[267] They also discussed the incidences and treatments of various complications of birth and of diseases of women and children.[268]

Merriman's attempt, for instance, to find the best means of saving the mother's and child's lives when an ovarian tumour or distorted pelvis impeded parturition, by numerical comparison of the results of various operations, was exemplary for the time.[269] In the latter case, 23 caesarean sections collected from the British literature had saved only ten of the 46 lives involved. An alternative operation, Sigault's section of the *symphysis pubis* (for which Jean-René Sigault (b.1740) was awarded a pension and a medal from the French government after one successful case in Paris in 1777[270]) had saved fifteen children and thirty mothers in 44 operations. Induction of premature labour in the eighth month, however, had preserved nine children and all mothers out of 33 cases. The surgical interventions could therefore only serve "to caution us against the inconsiderate and hasty adoption of modes of practice, unsupported by just reasoning, and unsanctioned by experience".[271]

Merriman read an account of the history of premature labour, and of statistics thereon from earlier British authors, to the Medico-Chirurgical Society of London in 1812. Disapprovingly he cited passages from three famous French obstetricians who had rejected the introduction of premature labour with theoretical argumentation alone, for they acknowledged little or no actual experience of it.[272] [In fact, the practice of premature labour was generally accepted in

Europe (161 cases published until 1831) with the exception of France until 1831; Paris resisted until 1840 even[273]].

Collins, Joseph Clarke's son-in-law, showed the complete disappearance of puerperal fever after introducing regular disinfection of wards with "chloride of lime" ($CaOCl_2$) in addition to the scrubbing and white-washing used already by Clarke and Labatt. His general mortality, too, decreased afterwards to 58 out of 10,785 (or 1 in 186), the lowest of any hospital up to that time. It increased again after termination of his mastership in 1833.[274]

With the opening of special lying-in wards and hospitals all over Europe in the later 18th century[275] reports containing descriptive statistics of presentation, malformations and sex distribution of the children were drawn up.[276] They reflected an increasing interest in "vital statistics", and the elements for them were quite precisely assemblable. The accounts of the labour and mortality of mothers seem, however, to have been initially a British venture. They were designed to render precise Smellie's early estimation of the occurrence of "praeter-natural" labour, in order to set out definite indications for the use of the obstetric forceps (1753). Statistics from British hospitals also furnished evidence for the value of preventive, and finally of therapeutic, measures. This illustrates the transition from preventive to clinical medicine in the use of this method, which I envisaged in my introductory chapter (see p.??).

With respect to therapy, statistics seem to have been more readily used in Britain than in France; for instance, there was the introduction of the artificial early delivery by Denman and especially by his pupil Merriman in London at the beginning of the 19th century and its rejection on purely authoritative grounds in Paris until 1840.[277] However the history of Sigault's operation (see p.??) shows that even in Paris it was rejected in 1789 on the basis of 33 cases collected from the literature which seemed to favour caesarean section.[278] As for amputation and lithotomy, for operations for breast cancer, and for cataract (see below p.??), the results of obstetrical operations could be observed and compared more objectively than those for the treatment of (puerperal) fevers, where there were many different and subjective diagnoses and where there were not enough objective criteria of cure.

I found a further example illustrating this dichotomy, related to midwifery, in the work of Edward Rigby the elder (1747-1821) from Norwich.[279] Apparently independent of a previous French author[280] Rigby differentiated the haemorrhages under parturition into "accidental" and “unavoidable” ones, the latter corresponding to the presence of a *placenta praevia* (1775). In the latter he recommended immediate delivery by art (turning, extraction) as the sole successful therapy, whereas natural delivery could be waited for in the cases of accidental haemorrhage. This recommendation was first based on 36 cases; by 1789, when the fourth edition of his *Essay on the uterine haemorrhage...* appeared, Rigby analyzed 106 cases (collected from 1769 to 1788) as follows: Of 42 cases of *placenta praevia*, 31 had been delivered immediately and ended with recovery of the mother; nine were delivered late, after the onset of the haemorrhage, and they had all died, and in two there had been a spontaneous abortion of a six month old foetus. The 64 cases of “accidental haemorrhage” had all spontaneously terminated well. Rigby willingly acknowledged the priority of his French colleague, yet remarked that the latter’s description had been based only upon two of his own observations.[281] His *Essay*, dedicated to Charles White (who had read the manuscript and encouraged its publication[282]) brought Rigby European fame.[283] It went through several English and American editions and was translated into German and French.

Although Rigby stated the results of the treatment of the two types of uterine haemorrhages so precisely, he lacked rigour in his assertions of the superior value of red Peruvian bark over the common, pale bark in the cure of intermittent fevers. His *Essay* thereon (1783) imitated a previous one by the well-known William Saunders (1743-1817), a Scot, who had become a physician to Guy’s Hospital London, was more concerned with the experimental elucidation of the mechanism of action of the bark than with its clinical trial. Thus he thought three selected cases out of an “experience” of many hundreds were “sufficient to authorize…[the] opinion” that red bark had superior febrifuge qualities than yellow bark. But to be sure he re-printed (on eighty pages) eleven letters from nine authors who claimed the same in general terms and with selected cases.[284]]

In 1789 Rigby became a member of the Medical Society of London,[285] later an F.R.S. and F.R.C.S. Nevertheless he remained attached until his death in 1821 to the Norfolk and Norwich Hospital. Characteristically for this Unitarian reformer he had been associated with it from its opening in 1771. Since then this institution had kept exemplary records of all patients (repeatedly exploited by writers interested in lithiasis.[286]) Yet Rigby had collected his 106 midwifery cases exclusively from private practice, as such patients were not admitted into the general hospital. Indeed, association with a hospital or dispensary was often helpful and favourable for numerical research, but it was by no means a necessary condition for the procuration of data!

3. EVALUATION OF CURES AT BATH

Another specialised 18th century institution whose physicians laid great emphasis on numerical evaluation of results was the Royal National Hospital for Rheumatic Diseases founded in 1738 at Bath. The reason for this may be found in what has been called the "balneological war", a prolonged argument over the uses and abuses of Bath waters during the 18th century.[287]

a. Rice Charlton

In 1770 Rice Charleton (1710-1789), an Oxford M.D., who was physician to this national establishment, asked a question often encountered in the literature with which my thesis has been concerned:

"Whose opinion is to be trusted if, after experience of ages, the question of usefulness or detriment of a remedy still persists, if for instance [Sydenham and Mead] the most eminent physicians of the last and the present centuries, are dramatically opposed?"

The answer was, (and it is worth noticing the spelling), that "the most respectable authority must give way to the force of *F*acts".[288]

In his *Inquiry into the efficacy of warm bathing in palsies* (1770) he set out to resolve the dispute from precisely kept records of all patients between May 1751 and May 1764. In a table he broke down the 1,053 patients admitted under the general diagnosis of palsy into twelve different species, [e.g. 45 general palsies, 283 hemiplegias, 144 palsies of the lower limbs, 3 dead palsies, 5 shaking palsies, 247 palsies from cyder...183 without diagnosis], for each of which he gave the number admitted, cured or benefited (813), and not improved (240). The latter was further subdivided into patients "improper" for treatment, those who were discharged "for misbehaviour" or at their own request, and those who had "eloped". He commented judiciously that the benefit of the Bath waters was so great "that, it is almost unnecessary to take notice of an error in this calculation tending to their disadvantage", namely that the last four groups had not had the trial of the waters.[289]

Charleton also used arithmetic for other questions; for instance, "The evidence which the table of Paralytics affords" would, he hoped, best determine the question whether warm bathing would incite rapid return of apoplectic strokes in patients with hemiplegias, as was often presumed. This Charleton did by deducing the mortality rates, which were 1 in 22 for all his patients, and 1 in 21 in the hemiplegic, some of whom had actually died of smallpox. Therefore the cure at Bath was "preventive" rather than an accelerating agent for the return of strokes, for "more might have died of apoplexy without bathing".[290]

This method of determining the probability of a cure being really effective, albeit defective in its lack of appropriate controls, was in its use of simple arithmetic as remarkable as the method of Millar and John Clark. In its presentation it reminds one of Jurin's proof, that the fatality of inoculation smallpox was very much less than that of natural smallpox, in the first half of the 18th century. Jurin's study was probably more generally known than that of Bernoulli's and d'Alembert's more sophisticated (contemporary) mathematical approach to the question of how many years would be added to the average human life span if smallpox were extinct.[291]

b. W. Falconer

A later physician at Bath, William Falconer (1744-1824) continued and elaborated Charleton's counting method. Born in Chester, he had studied in Edinburgh and Leyden, and graduated from both universities. He became a physician to the Chester Infirmary in 1767, in the same year as Haygarth. They practised together until Falconer moved to Bath in 1770, but they did not lose contact.

In Bath Falconer became a physician to the National Hospital in 1784, and thence he drew the material for his most high ranking work. [Earlier publications had dealt with the Bath waters in a non-quantitative way. They also included *in vitro* experiments with substances thought to induce dissolution of bladder calculi.[292]] A register was duly dept there since 1772.[293] In 1790 Falconer published a *Practical dissertation on the medicinal effects of the Bath waters* in which he compared Charleton's 1751-1769 results with those for 1775-1785. He began his

Dissertation with an appropriate introduction concerning the mischievous influence of preconceived theories, concealment of unsuccessful cases and need for mass observation; for instance, he declared that if the mineral waters were "described as doing good only without power of doing harm, we may be satisfied either that the account is misrepresented, or that their qualities are too insignificant to merit notice".[294]

Then Falconer indicated the probability of the success of the waters by giving the simple ratios of those who had not benefited to 1) those who admitted and 2) those who had not benefited. The latter ratio could be enormous for instance for "chronic rheumatism" (6.3877 to 1), as for "ischiatic complaints of the hip" (3.175 to 1) whilst for "white swelling of the knee" it was small (5 to 1).[295] [The total numbers of cases of palsies, chronic rheumatism, ischiatic complaints of the hip, white swelling of the knee and leprosy were 730, 362, 167, 12 and 196 respectively.] In other diseases, where there were presumably not enough cases, no numerical accounts were given. A table of the age-distribution of idiopathic palsies and a list of conditions for which the Bath waters were not indicated[296] completed this notable *Dissertation*, which was in its third edition by 1807.

The "propriety" and success of the waters in the different types of palsy being sufficiently accounted for, Falconer used the registers of the whole hospital practice (not only of his own practice) for two even more elaborate studies. These concerned first *Rheumatic cases* (1795) and then the closely related topic of *Ischias; or the disease of the hip joint* (1805).

The first study analysed 444 cases which occurred from 1785 to 1793; they were divided into 154 "cured", 167 "much better", 65 "better", 53 "no better" and 5 dead, each category being well defined. After crossing out 38 cases where the patients had had no real trial of the waters, 424 remained. For each of the five categories the average duration of stay was calculated; they were also grouped according to those who could stay was calculated; they were also grouped according to those who could have been described as benefitting or not benefitting. The proportions between the sexes, and the age-groups were presented in detailed tables for each of the five categories, as well as their distribution according to the month of the year and the average stay of the patients admitted in each month and in each season.[297]

Comparison with Charleton's former account (1770) showed that Falconer's registers gave "a full and decisive testimony", independent of the period chosen, for the proportions of the patients “cured”, “much better” or “benefitted” to the whole were “nearly on a level”: 1 to 2.7532 as compared with 1 to 2.818, 1 to 2.5381 with 1 to 2.4864, and 1 to 1.0984 with 1 to 1.1438 respectively. (These were Falconer’s own calculations!)[298]

Numerical indications also proved that the disease was by no means more frequent in women than in men; on the contrary, the proportion of men to women was as 1.9041 to 1. That the successful results were rather in favour of the men Falconer demonstrated by a simple “rule of three”: if the proportion of “admitted” to “benefitted” were to be equal in men as in women, namely as 146 to 130, it should be as 278 to 247.53. But the real number of men benefitting was 256. Thus there was a difference, but it was not considerable as it did not amount to a difference of proportion greater than 31 to 30.

Indeed, falconer’s was an astonishing attempt to quantify what “success” and “difference of success” meant. The tables of the ages of those who were admitted and of the results showed, furthermore, that persons between twenty and thirty-five years were also represented, and almost most likely to receive relief from the waters. More than half of all cured patients were from this age group, an two-fifths of the two other categories benefited. The proportion between those who received relief and those who received none was nearly 22.5 to 1. This proportion also showed that spring and summer were the seasons when success was most likely, for then it was 12 to 1, as compared with 10.393 to 1 in autumn and winter.

Falconer’s analysis of 556 cases of Ischias (1805), admitted between 1785 and 1801, was very similar. It included a numerical comparison with the results that had been obtained between 1761 and 1773 in the same establishment and published by Charleton.

Two remarks may be made marginally. Firstly, the cure with Bath waters included bleeding, vomiting (antimonials) and blistering as well as warm bathing.[299] It follows that the effect of the waters *per se* was difficult to evaluate. Both Charleton and Falconer tried to circumvent this by showing in their case-histories how medical treatment previous to the cure at Bath had often been ineffective. Secondly, it is worth mentioning that Falconer, who corresponded with Lettsom on various issues,[300] was a regular contributor to the Memoirs of the Medical Society of London. In 1787 he had been the first winner of an annual award sponsored by Lettsom. In 1789 a numerical study on palsy based on one hundred consecutive cases from the register at Bath, appeared. The findings were stated in percentages. In 1792, Falconer was listed as a corresponding member of the Society, and in 1802 he won another of its awards reserved for non-fellows.[301]

It is patent from the foregoing two sections that there were doctors at dispensaries and specialized institutions, who, both in theory and in practice, had crossed the threshold to which the combined teachings of, say, the important Edinburgh masters may have led them. They availed themselves of the possibilities offered by these new institutions. They collected large sets of data and analyzed them with the "new" tool of arithmetics to achieve precision in nosography and therapy. In the next section I shall show that this technique was also used for the systematic empirical investigation of the medical effects of certain drugs, in order to define precisely when they might be useful. I shall further discuss some numerical nosographical studies.

E. THERAPEUTICAL TRIALS AND NUMERICAL NOSOGRAPHY AT VOLUNTARY HOSPITAL

1. WILLIAM WITHERING AT STAFFORD AND BIRMINGHAM

One of the most outstanding and lasting contributions to the *materia medica* of 18th century was doubtlessly digitalis. The first systematic *Account of the foxglove and some of its medical uses with practical remarks on dropsy and other diseases* (1785) by William Withering (1741-1799) is a medico-historical "classic",[302] and, not surprisingly, Withering has been the subject of several studies.[303] Let us note that he was born in the English Midlands, studied in Edinburgh under Alexander Monro *primus* and *secundus*, and under Cullen (M.D. 1766) and settled in Stafford in 1767. Having little to do in his practice at first, he occupied himself with botanical studies and also kept a climatologic journal. In fact he became a celebrated botanist. In 1772 a hospital was founded in Stafford of which he was appointed physician. In 1775 he moved to Birmingham where he acted as chief physician to the General Hospital when it was opened in 1778. There he became a member of the Lunar Society in 1775.[304] He also was a member of a local Society for the Abolition of the Slave Trade. Lettsom was keen on having Withering as a corresponding member of the Medical Society of London when he had read his *Account* in 1787. At this time Withering was already F.R.S. for three years.[305] A friend of Percival since their Edinburgh days, they continued correspondence, and Withering also knew Thomas Fowler.[306]

Before 1779 Withering had records of nineteen cases of "dropsy" treated with digitalis. But, as was the case with Rigby's and Haygarth's, his systematic study of the foxglove in dropsy was based on private rather than on hospital practice. Of the 163 cases collected by 1785, only seven had come from the Birmingham Hospital.[307] He introduced his cases as follows:

"It would have been an easy task to have given select cases, whose successful treatment would have spoken strongly in favour of the medicine, and perhaps been flattering to my own reputation. But Truth and Science would condemn the procedure. I have therefore mentioned every case..., proper or improper, successful or otherwise."

In this approach may be one reason why he succeeded in deciding upon the types of patients who would benefit from digitalis. This is the more remarkable as virtually nothing was known then of the pathology of different kinds of oedema. In assessing the value of his treatment Withering relied upon the clinical methods available, that is close observation of the patient, assisted by counting the pulse and measuring the urinary output as an objective check of the validity of subjective improvement. So he thought a case most promising

"....... if the pulse be feeble or intermitting, the countenance pale, and the lips livid, the sin cold, the swollen belly soft and fluctuating, or the anasarcous [oedematous], limbs readily pitting under the pressure of the finger, [for then] we may expect the diuretic effects to follow in a kindly manner."[308]

He compared these parameters with the patient's previous condition and sometimes observed relapses on discontinuing the drug.

Considering the clinical methods at his disposal he could only be cautious in drawing conclusions from an inquiry as objective as his attempted to be:

"No general deductions, decisive upon the failure or success of the medicine, can be drawn from the cases I now present ....[for they] must be considered as the most hopeless and deplorable that exist .... lost to the common run of practice."[309]

He defended his limiting his descriptions to his own cases only: people might doubt the impartiality of his account, and he admitted that, had he reported the cases sent to him by fellow physicians, his book would have been seemingly free from any predilection and the critics

“would ...close the book, with much higher notions of the efficacy of the plant than what they would have learnt from me [But] the cases [I have received] are, with some exceptions much too selected.”[310]

Thus Withering dismissed the habit of increasing the number of observations by adding experience from others, as erroneous and misleading if those others did not give all the details of their whole practice, successes and failures alike.

As a careful observer and thinker he realised this fundamental truth often reflected in the history of medicine:

“It is much easier to write upon disease than upon a remedy. The former is in the hands of nature, and a faithful observer, with an eye of tolerable judgment, cannot fail to delineate a likeness. The latter will ever be subject to the whims, the inaccuracies, and the blunders of mankind.”[311]

2. John Ferriar at Manchester

Withering’s *Account of the Foxglove* aroused widespread interest and reaction.[312] One contribution was by Ferriar, who dedicated his *Medical histories and reflections on dropsy* (1792) to Percival, his friend and senior at the Manchester Infirmary. In fact his series of *Medical histories,* the fourth volume of which came out in 1813, contained a continuous account of cases of dropsy in his hospital with a clear programme closely following that published by Thomas Fowler at nearby Stafford in 1785 (see above p.??) and corresponding to that advocated in Percival’s medical ethics.

Ferriar emphasised the importance of reporting clinical data without intervention of “personal considerations“. As for the value of unsuccessful cases he referred to Francis Home, the military surgeon of the Austrian War (see below). In his opinion, the keeping and periodical analysis of a journal with indications of the success of treatment was the absolutely necessary basis of reasoning and acting for a physician, who wanted to avoid false conclusions from memory lone and „who would do justice to his patients".[313] Thus, on the treatment of dropsy he wrote: "I do not remember, to have seen any comparison instituted among the various methods of reducing the swelling by increasing the quantity of urine in this disorder".[314] Therefore he presented 47 cases, 24 treated with digitalis, ten with cream of tartar, eight with Calomel, the others with various remedies. The overall results gave the number of patients cured, relieved, not relieved and dead, distinguishing between four categories of "dropsy" (i.e. anasarca, ascites, hydrothorax and combined cases). He specified these in a table. He found from his figures that digitalis was the most favourable agent in general, but for hydrothorax - admittedly based on four cases only - cream of tarter represented the best treatment.[315]

Ferriar continued to report on dropsy in the same comparative wax in the second volume of *Medical histories* (1795) in which he included all his former cases, too, for "more conclusions".[316] He also wrote a separate *Essay on the medical properties of the digitalis purpurea* (1799), in which he recommended its use combined with cream of tartar. Similarly in 1813, when he reported on a new remedy for dropsy (Extract of Elaterium), twenty selected desperate cases had given him a "nearly uniform successful" result, but .... he had joined another active diuretic with it![317]

Thus Ferriar carried out a valuable research programme, which may even have impressed Pinel (see above), with some perseverance in cases of dropsy. But he was not critical enough in the interpretation of his results. A similar dichotomy between programme and execution also applies to the work of another eminent provincial physician: Thomas Fowler.

3. THOMAS FOWLER AT STAFFORD

Withering's indirect successor at Stafford and an acquaintance of his was Thomas Fowler (1736-1801) who was also a correspondent of Willan in London.[318] He had been an apothecary in York before taking up medical studies in Edinburgh in 1774 at the age of 38. He was active at the Stafford Infirmary until 1791 when he went back to York and became a Physician to the "York Retreat", the famous Quaker asylum.

Between 1785 and 1795 he published a series of three *Medical reports* concerning the effects of tobacco, arsenic, bloodletting, sudorifics and blisterings in defined diseases according to a clear programme, for which he referred to Bacon's plan for the improvement of universal science.[319] Some of these books were re-edited in English and translated.

The programme as stated in 1785 advocated the following.

1) Recording the effects of the drug upon every patient;
2) Study of the medical effects of natural products, for these were considered less variable and uncertain than those of the druggists;
3) In order to avoid prolixity of repetition, the reports had to consist of only a few detailed cases, but they were to include in an abstract (tabular) form an account of the whole practice;
4) Finally, if the plan were successful it should be extended to other medicines.[320]

Fowler also emphasized the necessity of distinguishing between the "operational effects" (e.g. vomiting, diarrhoea) and the curative effects of a medicine; this would also allow the physician to differentiate between a poison and a good medicine with "side effects". He thus declared;

"It becomes highly requisite that the Public should speedily be made acquainted with such effects as far as they are known; together with such Precepts, Cautions and Restrictions, as may tend to unite the greatest Degree of Safety with its Efficacy. Nothing of this sort has yet been done."[321]

Such a task would be difficult. Therefore, he advised, that

"the memory must be assisted: Art and Numbers must unite their effort for a considerable length of time; a series of cases must be treated, with a constant view of the Investigation .... uninfluenced by Theory, custom, or Authority. [And] Collaboration with other workers may even become indispensable to achieve these premises."[322]

The *Medical reports on the affects of tobacco* (1785, 1788) were presented according to this programme. The proportional occurrences of "operational effects", such as sensation of heat, vertigo, nausea and diarrhoea, were given out of 400 cases. Similarly for the curative effects: of 79 cases (dropsical), 28 were cured (particularly those with oedema of the legs), 32 relieved and 19 not relieved. The proportion of the dysuric benefitting from the preparation of tobacco was seven out of eight.[323]

The most impressive feature in his second work in this series, the *Medical reports on the effects of arsenic* (1786) was a table of all the 247 cases of ague in which the arsenic solution (still prescribed as *Solutio Fowleri* in dermatology) had been administered. Of these 247 cases, 144 patients had been cured without any relapse, 27 with relapses, by the solution alone: In 51 patients fits had been totally suspended, 20 patients had been relieved and only five not relieved at all. Of the latter three groups 45 persons could be cured eventually by the assistance of Peruvian bark. The 31 remaining patients were either "irregular" or still under treatment. Fowler also quoted a letter from withering who had used the remedy in 48 patients. 33 of them had been cured with the solution alone, and without complication; three had complained of side-effects which needed "a little soluble tartar", and only twelve patients received no benefit. Thus Fowler claimed that in cases where the bitter tasting Peruvian bark was difficult to administer, (e.g. in children) or refused because of too many disagreeable "operating effects" (such as vomiting) arsenic was a valuable alternative.[324] It was a reasonable claim from his point of view, though his methods of observation appear no to have been inadequate for the assessment of a remedy for such a notoriously intermittent and relapsing disease.

The third book reported on *Effects of blood-letting, sudorifics, and blistering in the cure of the acute and chronic rheumatism* (1795). Encouraged by the success of his former two books Fowler delved into the nearly 5,000 cases of different diseases he had recorded during his ten years practice at Stafford, and he chose about 500 cases of both chronic and acute rheumatism for his next object of study. He undertook it "for the purpose of illustrating on a more enlarged scale than in his former specimens the plan of the celebrated Lord Bacon for the improvement of physic". He also hoped to convince those who were suffering because of their blind belief in the healing-power of nature, that there was value in the art of medicine. Fowler pointed out that although the tree remedies mentioned in the title were by no means new, it had not been possible hitherto to discriminate their effects from the efforts of nature, their operational from their curative effects, and the effects of one from those of the other remedies. This would need the prescribing of only <u>one</u> at a time, possibly in one dose and one form of prescription, „ a vastly ignored fact".[325]

An examination of his 109 detailed cases (with four different treatments for acute and six for chronic rheumatism) reveals that despite this rational programme the groups were not strictly separated; for many a patient received one or more medicine belonging to other groups under study. Fowler broke down his overall results into six categories, namely: cases cured by one particular method only; cases cured “chiefly” by one particular method; and cases that were 1) much relived, 2) moderately relieved, 3) only slightly relieved and, 4) not at all relieved. Thus a table of 78 cases, backed up his conclusion that bleeding was only an auxiliary in acute, and even less auxiliary in chronic cases. 81 cases supported his questioning the curative effects of blistering and 180 cases strengthened his recommendation of sudorifics, other than Dr. James’s powder.[326]

Moreover, in his studious endeavour “to render his hospital practice subservient to medical improvement”,[327] Fowler went a step further by using his numerical notes for clinical research into a fuller distinctive description of acute and chronic rheumatism. He gave precise proportional data for the periods of marked pain, for age, sex and seasonal distributions, for the occurrence of concomitant affections of the brain and sensorium, and for the fatalities in 87 cases of acute rheumatism and 401 cases of chronic rheumatism, in particular for lumbago. He thereby discovered that one fourth of the acute cases were related to exposure of cold or imperfect cure of acute rheumatism.[328] It was precisely the same type of study, but on acute rheumatism which was published ten years later by John Haygarth.

4. John Haygarth at Bath

During his retirement at Bath after 1798 John Haygarth decided, as had Ferriar, to work up his notes on 10,549 patients that he had collected since 1767. He had used a concise method of recording in Latin, which had been published in 1784 already.[329] Interestingly, Haygarth’s study was based on notes concerning his private patients only, because for lack of time he had not yet dealt with the even larger number of diseases from the Chester Infirmary. Even so, he had managed to group 271 cases of herpetic or scorbutic eruption, 383 of dyspepsia, 827 of syphilis, 914 of hypochondriasis and 470 of rheumatisms (with the exclusion of sciatica, lumbago, “tic douloureux” and nodosities of small joints).[330]

It was on the 170 cases of acute rheumatism (i.e. accompanied with fever) that he wrote his first of a planned series of *Clinical histories* (1805). Abstracts of all these cases were drawn up in a table with 27 columns, eleven of which concerned their remedies (three of those columns dealt with the administration for the bark alone) and their outcomes (recovery or dead). Analysis of this table permitted not only a clear numerical comparison of the proportional success of treatment, with or without bark, as a replacement for bleeding, including details of all twelve unsuccessful cases. But the clinical history of the disease and its distinctive symptomatology could be similarly described: “The altar of truth should be built with unhewn stone”, Haygarth wrote. Several tables that he analysed gave the proportional occurrences of the disease according to sex, age seasonal distributions, and “cause” (as diagnosed by the patients themselves). The cause was mostly exposure to cold or moisture. The average period which elapsed before the appearance of the first symptoms could be fixed at between 48 and 72 hours. Concomitant diseases were listed in order of frequency as were the joints and/or muscles inflamed and pulse-ranges, too. The occurrences of pain and swellings (alone or together) of chills and sweat (alone or together) and their localisation in joints and muscles (alone or in both) were also given.[331]

A second Clinical history drawn from Haygarth’s records concerned the “nodosity of joints”, in which he described and differentiated for the first time what is now known as *arthritis deformans*.[332] This second treatise showed exactly the same methodological features as the first, with the exception of the giant 27 column table which was now dropped, for its printing had been “so tedious and troublesome a business... that ... no more shall be published”.[333]

Thus we can see that Haygarth used the numerical method both for a better clinical differentiation of diseases and for the evaluation of therapy. Usually it is Louis who is given credit for the application of the “*méthode numérique”* in both these respects (see p.??), for Louis used it especially in his anatomo-clinical research on phthisis published in 1825. In fact Haygarth had started to do such numerical research, also on phthisis, in 1777. In 1805 he wrote: „But after I had made considerable progress in this inquiry, I found the subject too melancholy, and could not assume resolution to proceed in this investigation“. Nevertheless, he was able to indicate the age and sex distributions and some of the “causes” of phthisis numerically.[334]

Haygarth was afraid that his array of factual detail might seem superfluous to many readers; but he felt that as the material was taken from nature it would perform the important and much-needed task of verifying the opinions of others. He also set out to solve the question whether scrofula and phtisis were the same disease, by the numerical method: of 10,549 patients there were 827 with phthisis and 71 with scrofula, but only four out of both groups had both diseases. Thus he concluded that they were not the same since there were 823 patients with solely phthisis and 67 with scrofula alone.[335]

This example shows clearly the limitation of the purely observational clinician even if he availed himself of the new methodology of the *méthode numérique*. Only the new sciences of pathological anatomy and bacteriology could finally decide this question - albeit with the help of the same *méthode numérique*.

A truly remarkable feature of Haygarth was his realization that the results of his inquiries were base on probabilities, which he attempted to calculate (or better, to estimate). In fact he had already used simple calculation of probability in 1784 in his *Inquiry how to prevent the small-pox* and in 1801 for deciding on the mode of propagation of fevers (see above). Haygarth brought yet another aspect into the discussion for the evaluation of therapy with his writing on the *Imagination as a cause and as a cure of disorders of the body* (1800). It was dedicated to Falconer "as a memorial of a mutual, cordial and constant friendship for thirty-six years". It contained a trial to which he had submitted "Perkin's metallic tractors" with the assistance of this friend. The "tractors" were metallic rods supposed to cure a great variety of diseases by some electrical influence, a treatment which was one of the legacies of Mesmerism. This treatment was recommended by distinguished doctors and an "Institute of Perkinism" was founded in London.[336]

In Haygarth's trial, wooden imitation tractors were first used on five patients and all but one were relieved. The next day, with genuine tractors, the same result was obtained. Haygarth aptly quoted Lind's comment on fictious scurvy remedies:

"An important lesson in physic is here to be learnt, viz. the wonderful and powerful influence of the passions of the mind upon the state and disorders of the body. This is too often overlooked in the cure of diseases."[337]

Here, as in every subject he touched, Haygarth increased accuracy and reliability of his statements by clear experimental design and numerical expression of the results. As with his approach to smallpox, fever and rheumatism he thereby shed new light upon a socially important question of the time.

This time it was the fight against quackery, which was on the programme of medical and social reformers. It had been a momentum in the writings of Lettsom, Millar and Fowler, and the regulations of the Medical Society of London stated that o proprietor of a secret medicine could become a member.[338] Other writers, like James Makittrick Adair 81728-1802), wrote popularly at great length against quackery and secret remedies. A combative Scot, (Edinburgh M.D. in 1766) he settled at Bath and undertook with Falconer also a series of laboratory experiments to disclose the nullity of a certain *nostrum.*[339]

Adair explained the flourishing of quackery by outlining the difficulties of attaining certainty in medical knowledge. Yet he did not show a way out of these difficulties. Haygarth did so with much persistence and success, as is attested by the reviews of his works in the contemporary literature (see below).

F. Conclusion

There is no doubt from the foregoing chapter that a number of doctors in the second half of the 18th century realized the need for adequate trials, on a comparative numerical basis, for the evaluation of their therapies. Hospitals and dispensaries, or merely the personal records of a physician, provide the necessary number of patients. In addition, a period spent in the Army or Navy, or an acquaintance with the business of census-taking, gave the doctors concerned some methodological guidelines. Together with the impact on observations of facts chiefly derived from the Leyden-Edinburgh teaching, this led to a quite extensive us of statistics by a number of physicians of independent mind who were acquainted with each other. Ferriar, Fowler, Haygarth, in addition to their deliberate therapeutic experiments, also used the numerical method to establish the diagnostic features and natural histories of diseases, just as the workers at the London and Irish fever hospitals and lying-in charities had done.

Admittedly, Bateman's differentiation between two fevers is difficult for us to understand, for we tend to distinguish those diseases by anatomo-pathological and microbiological criteria rather than by proportional listing of their occurrence was successful in other diseases and in obstetrics. This method is still current in modern textbooks and corresponded to a fundamental step away from the use of vague statement like "sometimes" and "often" to quantitative indications such as percentages. Cheyne's descriptions of the proportional occurrence of objective symptoms (such as frequency of pulse, breathing, and body temperature), were likewise important steps towards the modern approach.

This holds, of course, for their presentation of therapeutic results, too. As John Haygarth put it in 1805, this implied a cumbersome task of recording the observations, then arranging them into tables and analysing them.

“But”, he continued, “....I do not regret that I have printed [these tables], as they exhibit proofs and illustrations of the frequency of symptoms, and the degree of success with which the remedies have been administered, with more accuracy than any other arrangement with which I am acquainted”.[340]

The stress they laid on the method reflects the opposition they faced from the orthodox theorists who still tried to impose a priori system upon medical treatment. Not surprisingly, these arithmetic observationists often worked in the provinces, away from the fashionable systematists. And, (this holds also for London), it is equally striking that they all were medical and/or social reformers. As such they were associated with *newly* opened or uncustomary types of charities, the specialised hospitals and dispensaries, which had a certain propagandistic use for their results, i.e., to justify their existence. Also these institutions were under more direct medical control than the older general hospitals.

It might be argued that my sample was too selective, as it virtually excluded the work of the physician of the great London hospitals. As suggested by my example of the trials of various barks, comparative numerical work seems not to have been done there at first, partly perhaps because the variety of occurring diseases made it more difficult for a single consultant to set up proper statistics. There were after all, observationist physicians at the great hospitals, but generally no arithmetic observationists, as illustrated by the following example.

Around 1800 John Clark collected evidence in support for the promotion of a fever hospital in Newcastle. He wanted to compare mortalities, possibly from specific diseases, at new, clean and aired hospitals with those at older ones. Thus he chose surgical cases and fevers. For the former, he was able to list the overall mortality from fractures (sometimes those of the skull separated) and from amputations, at the new Artillery Hospital at Woolwich (see below) and at the Leeds, new Northampton and Glasgow Infirmaries, founded (or rebuilt) in 1793, 1795 and 1796 respectively. By contrast, the older institutions such as the great London Hospitals, the Salop, Worcester and old Northampton Infirmaries had figures only of overall mortality, possibly not even related to the numbers of admissions. Clark's own wards at the old Newcastle Infirmary were just the exception confirming the rule.[341]

The same was true for the mortalities from fever. The Edinburgh episode of 1817, i.e., an epidemic of typhus striking a town without any fever hospital but with a long-established general Infirmary, is revealing in this respect. There were no earlier data o proportional mortality for comparison available, for the numbers of admissions and cures were not known but only some absolute figures of deaths from fever *per annum*. The *Edinburgh Journal* printed in 1818 a table, embracing the years 1795-1817, and showing institutions in Ireland, Scotland and England keeping and publishing records of their fever patients. In Ireland there were three fever hospitals where doctors had worked out records for the years of their opening (Cork 1803, Dublin Cork Street 1804 and Dublin Hardwicke 1813). In Scotland there was only the Glasgow Infirmary (opened 1795). In England, the Manchester and London fever Hospitals opened in 1796 and 1802 respectively, had equally begun to keep records. As in the Edinburgh Infirmary, some of the doctors of great Hospitals started reporting on fever cases in their wards during the epidemic of 1816-1818, for instance at Guy's, the Westminster, the Middlesex and the London Hospitals. The latter was an exception, for John Yelloly had already started there in 1812 as he was generally interested in a quantitative approach to medical problems, as was Marcet (see lithotomy) who reported from Guy's. [By coincidence, both served with (among others) Bateman, Blane and James Currie on the council of the new Medico-Chirurgical Society after its split from the London Medical Society in 1805.[342]] The table also listed two London dispensaries, yet said nothing of hospitals like St. Bartholomew's or St. Thomas's.[343]

This state of affairs was noted by a select committee appointed by the House of Commons to report on the contagious fever which had reached London (1818). They wrote:

"Your committee cannot close this Report without expressing a regret that any hospital in the Metropolis should not possess a register of diseases: they trust this omission will speedily be rectified...[It] felt the want of that information, arising out of this strange irregularity, in not being able to ascertain the average fever cases that have occurred for some years in the Metropolis."[344]

The *Edinburgh Journal*, which published this extract, reacted equally decisively, and its analysis sums up the situation so well that it is worth being quoted fully:

"We wish [the Committee's censure] ... to be universally known, not merely by the medical attendants of all public institutions, but by the unprofessional governors, that, upon these general points of public interest, it is expected, that they at least record and preserve satisfactory documents. This we know is the practice of many hospitals, whose registers contain the information at present so much desired; but they should go one step farther, and publish an annual abstract of their practice, and render it accessible by purchase to the profession at large.

That almost all hospitals publish annual reports, we are fully aware, but, in many cases, they are intended only to furnish information as to the expenditure of the funds, and the names of the office-bearers, and as public acknowledgement of the support of the subscribers. Such reports, however, are of no use [professionally.]..., and except for purposes....mentioned above, do positive harm, by causing hospital reports in general to be neglected as utterly without value. This, however, our readers well know is not the case; and the reports we have of late years received from the Fever Hospitals of Dublin and Cork, are worthy of being imitated by hospitals of every kind, and in every place....We mention these reports..., not as being the only ones of the kind, but because they have been transmitted to us, and we regret that reports, containing valuable information, are often printed merely for local circulation to obtain funds, and thus are of less general use... We have only to add upon this subject, that the most valuable reports often proceed entirely from the professional zeal of the reporters, and are only occasional. This leads us to suggest, that the Governors of Hospitals should enjoin their regular appearance as a duty upon their medical officers; and we will venture to say, that where it has not yet been practiced, its good effect upon the institution in an economical, as well as a professional point of view, will soon be apparent".[345]

Truly, the institutional reports, intended to inform the subscribers of a voluntary hospital, were almost solely for administrative purposes and had a limited circulation (as pamphlets). Yet, in the more medically influenced specialised institutions they might also contain the number of patients treated according to diagnoses, with numerical statements of the results. Besides the examples quoted from midwifery and fever hospitals I have found other references to such reports, e.g., in some published histories of eye hospitals in London (founded 1805), Glasgow 81824), and Manchester (1814).[346]

Thus numerical statements of incidences of diseases, symptoms and of the results of therapy were current within these institutions, but their utilisation for scientific purposes was, and still is, a matter for the individual physicians attached to them. Evidence for such use may therefore be found in the published works of particular doctors, as shown by some early examples in this chapter, rather than in an institution's reports and minute books, which are often not preserved,[347] especially for defunct institutions. A systematic study of the scientific work in British eye hospitals, founded from 1805 onwards, might be rewarding from this point of view. [In 1821-1823 the Edinburgh Journal published, for instance, an excellent numerical report by a surgeon-ophthalmologist from the military ophthalmic hospital at Chatham with views on the success rates of particular cures in specific diseases.[348]] On the other hand the well-known contribution of some leading British physicians of the 19th century could be reconsidered with regard to their association with a specialised dispensary or infirmary. Thomas Addison (1793-1860), for instance, was in his youth physician to the "Universal Dispensary for Children".[349] John Bunnell Davis (1770-1824) was his senior there, and the founder of the Dispensary. He had been a temporary hospital physician to the troops during the Napoleonic wars. He published a tabular 'Outline of nosological arrangement of diseases in children, acute and chronic, according to the situation of parts,' and used it for his numerical reports he had drawn up from the institution. At first they were regularly published, with explanations of the therapies used, in the *Medico-chirurgical* journal and after four years they were collected in a book.[350]

Individual doctors, influencing certain dispensaries and hospitals, recognised well that the numerical approach had to offer various things to medicine, as a science. But it still took a long time to establish its practice at the great London hospitals, or even to make it administratively compulsory for their doctors. This is suggested by the vigorous campaign for publishing their results led during the 1820s and 1830s by Thomas Wakley (1795-1862), the editor of the *Lancet*. As a part of the original policy of this journal, this was thought to be advantageous for the general public as well as for medical science itself.[351]

In 1832 Wakley commented thus on the 'Clinical reports of the surgical practice of the Glasgow Royal Infirmary'.

"The plan, upon which these reports are conducted, would produce results extremely beneficial to the science of medicine, if imitated upon an extensive scale by the surgeons and physicians of our great hospitals. At present the aid which the curative art derives from those institutions, is limited almost entirely to the moderate and most imperfect communication of medical information to students alone. Sometimes, indeed, a knife-flourishing Cooper [i.e. (Sir) Astley Cooper (1768-1841) (see below)]... will favour the public with the results of his experience, but the history of the hospital cases which form the ground-work of all [the surgeons'] knowledge and bloodily-acquired skill, would for the most part remain wholly unknown and unavailable for public instruction, were it not for the stealthy exertions of the weekly medical press."[352]

G. REFERENCES TO CHAPTER THREE

[1] Buer 1926, pp. 47-62.
[2] *ibid*., p. 45.
[3] Neuburger 1950.
[4] D.N.B., Underwood 1977.
[5] Clifton 1731, pp. 4-5, 13-14, 17.
[6] *ibid*., pp. 3-4, 18.
[7] *ibid*., pp. 19-22.
[8] Quoted by Neuburger 1950, p.47.
[9] Clifton 1732, p.173.
[10] Bruppacher 1967, p. 68; Gelfand 1977, p. 401.
[11] Wright St. Clair 1964, pp. 40-41.
[12] Med.Ess.Obs. 1; 234, (1733); 3; 299 (1735).
[13] *ibid*., 4; 47-65, (1737); 5 (part 2); 495, (1744); Wright St..Clair 1964, p. 47.
[14] Med.Ess.Obs. 4; 342, (1737); *ibid*., 4th ed., 4; 276, (1752).
[15] Quoted by Wright St. Clair 1964, p. 45.
[16] Med.Ess.Obs. 4; 47-65, (1737).
[17] Monro 1781, p.693.
[18] *ibid*., p.485 (misprint, corresponds actually to p.685).
[19] Wright St.Clair 1964, p.50.
[20] *ibid*., pp.79, 162.
[21] Monro 1776-1784, pp.282-285.
[22] Wright St.Clair 1964, p..77,81.
[23] Bostock 1833, pp.lxiii-lxix.
[24] Crellin 1971; see also Cullen 1786, pp.xlii-xliv, xlvi for illustration
[25] Cullen 1786, p.xlvii.
[26] See also Bostock 1833, p.lxix.
[27] Gregory 1805, pp.154-155.
[28] See a family pedigree in Pearson 1978, p.210.
[29] Gregory 1805, pp.144-145,171,184.
[30] Armstrong 1771, pp.182,187,199.
[31] Abraham 1933.
[32] Lettsom 1775, p.49.
[33] King 1971, pp.44-54.
[34] Lettsom 1775, pp.37,51; Abraham 1933, pp.167-175.
[35] Chaplin 1919, p.137; Cope 1969.
[36] Lettsom 1775, pp.41-44.
[37] *ibid*., p.46.

[38] Lettsom 1774, p.11.
[39] Lettsom 1772; Sims 1773.
[40] Lettsom 1774, p.11.
[41] *ibid*. P.343.
[42] See e.g. Blane 1813 (a), p. 117-119 for London, and Med. phil. Comment 3; 98-102 (1775) for the Edinburgh Infirmary.
[43] Lettsom 1774, pp. 345-348 (Tables I-III).
[44] Shryock 1948, pp. 79-117.
[45] Lettsom 1774, p. 344.
[46] *ibid*., p. 308.
[47] *ibid*., p. 169.
[48] *ibid*., p. 49.
[49] Buer 1926, p.258.
[50] Hartson 1963.
[51] Millar 1769, p. iii.
[52] *ibid*., p. 4.
[53] *ibid*., p. 82.
[54] Underwood 1977, pp. 24, 153.
[55] Maty 1767, p. 300.
[56] Millar 1769, pp. 158, 161-162.
[57] Millar 1770, p. 1-4.
[58] *ibid*., p.10.
[59] *ibid*., p. 5.
[60] Sims 1773, p. 3.
[61] Millar 1770, p. 119.
[62] *ibid*., 252.
[63] *ibid*, pp. 72-75, 91-118.
[64] Lloyd and Coulter Vol. 3 1961, pp. 332-333.
[65] Crellin 1974.
[66] James 1770, pp.iv-vi.
[67] Millar 1774, pp.83´.
[68] *ibid*., p.85-86,95.
[69] Millar 1777, pp.231-232.
[70] *ibid*., p. 2.
[71] *ibid*., pp. 7-8.
[72] *ibid*., pp. 4-5.
[73] *ibid*., pp. 10-122, 37, 42-56.
[74] *ibid*., pp. 40-41.
[75] Millar 1778-1779, pp. 177-184, 233-2 53.
[76] *ibid*., pp. 184.
[77] *ibid*., pp. 233.
[78] Rowley 1804, pp. 53-54; Buer 1926, p. 258.
[79] Rowley 1804, pp. 4,12.
[80] Rowley 1788, p. 41.
[81] *ibid*., pp. 3-4.
[82] *ibid*., pp. 4, 18.
[83] Rowley 1793, Vol. 4, pp. 107-285.
[84] Rowley 1804, p. 36.
[85] *ibid*., p. 21.
[86] *ibid*., pp. 53-54.
[87] *ibid*., p. 11.
[88] *ibid*., p. 36.
[89] *ibid*., p. 29, 36.
[90] Greenwood 1948 (A), pp. 63-64; Neuburger 1950; Macalpine and Hunter 1969, p. 299; Glass 1973.
[91] Black 1782, p. 310; 1789, pp. 44, 250.
[92] Black 1781, pp. 119-130, 162-165.
[93] *ibid*., pp. 104-105, 202.
[94] *ibid*., pp. 153, 180 .
[95] *ibid*., pp.172,174,175.
[96] Black 1782, p. 280.

[97] *ibid*., pp. 228-229.
[98] *ibid*., p. 280.
[99] *ibid*., pp. 226-227, 230.
[100] *ibid*., pp.230,254,274;1789, pp.v-vi.
[101] Black 1789, pp. 269-272.
[102] Black 1888, title page.
[103] Black 1789, pp. v-vii, 261.
[104] Black 1782, p. 299.
[105] Black 1789, pp. vi-vii.
[106] *ibid*., pp. viii-ix.
[107] *ibid*., pp. i-ii.
[108] Black 1782, p. 250.
[109] Black 1789, 58.
[110] *ibid*., p. ii.
[111] *ibid*., p. 44.
[112] *ibid*., p.101.
[113] Black 1788, pp. 100-101; 1789, p.43.
[114] Black 1788, pp. 101-101; 1789, pp. 46-47, 51-52.
[115] Black 1789, pp. 115.
[116] *ibid*., pp. 101-103, 108, 117, 129, 170, 178.
[117] *ibid*., pp. ii-iii.
[118] Macalpine and Hunter 1969, pp. 297-299.
[119] Black 1789, pp. 130, 131-150.
[120] Macalpine and Hunter 1696, pp. 299.
[121] Black 1788, p. 407.
[122] Black 1789, pp. 251, 264-265.
[123] *ibid*., p. 250.
[124] *ibid*., pp. v, 264; Millar 1798, Vol. 1, part 2, p. 76.
[125] D.N.B., Vol. 2, p. 576.
[126] Neuburger 1950; Macalpine and Hunter 1969, p. 350.
[127] Hunt 1972.
[128] Dukes 1961.
[129] Sims 1774, pp. 80-81.
[130] Millar 1776, p. 28.
[131] Black 1789, pp. vii-viii.
[132] Clark 1773, p. x; 1792, p. vii.
[133] Bett 1955; Hume 1906, pp. 33-35; Hume 1951, pp. 24-31; Webster 1978.
[134] Clark 1792.
[135] Clark 1773, pp. 150-151, 261-263.
[136] Clark 1780, pp. 369-385; 1792, pp. 464-501; 1802, pp. 206-216.
[137] Clark 1780, pp. xiv, xviii-xx, 370; 1792, p. 258.
[138] Clark 1780, pp. vii-xx, 23, 362.
[139] *ibid*., pp. 189-196.
[140] *ibid*., pp. 333-335, 344.
[141] *ibid*., p. 371.
[142] *ibid*., p. 384.
[143] *ibid*., p. 378.
[144] Feinstein 1967.
[145] Clark 1780, pp. 369-377, 385.
[146] Clark 1792, pp. 464-465.
[147] *ibid*., pp. 465-469.
[148] *ibid*., pp. 500-501.
[149] Millar 1783, p. 21; 1798, pp. 12, 78.
[150] Fulton 1933; Brockbank 1934; Greenwood 1948 (B), pp. 33-37; Turner 1957, pp. 51-53.
[151] Percival 1767, pp. 1-54, 349-374.
[152] Percival 1773, pp. 239-252.
[153] Pearson 1978, pp. 349-421.
[154] See Phil.Trans.R.Soc. 65; 424-445, (1775).
[155] Percival 1773, pp. 2, 4.
[156] See e.g. Phil.Trans.R.Soc. 47; 333-340 (1775) .

[157] *ibid*., 65; 322, 325 (1775); 66; 160-167; (1776); Brockbank 1934, p. 9.
[158] Greenwood 1948 (B), p. 34.
[159] Phil.Trans.R.Soc. 47; 333-340, (1752).
[160] Percival 1773, p. x; Haygarth 1805, p. 182; Currie 1831, Vol. 1, p. 67, Vol. 2, pp. 47-96; Schofield 1963, pp. 126-127.
[161] Ferriar 1798, p. 101; Brockbank 1934, p. 2; 1950, p. 13; Greenwood 1948 (B), pp. 37-41.
[162] Wyke 1975, p. 75
[163] Koebling 1967; Leake 1975.
[164] Percival 1827, pp. 32, 36.
[165] Brockbank 1950, p. 37; Leske 1975.
[166] Buer 1926, pp. 122-123; Abraham 1933; Greenwood 1948 (A), pp. 63-64; Schipperges 1971, p. 1117.
[167] Buer 1926, pp. 122-125.
[168] Weaver 1928.
[169] Haygarth 1805, p. 182.
[170] *ibid*., pp. 6, 16.
[171] Weaver 1928, p.166.
[172] Howard 1791, p.208-209; see also letters from Haygarth to Howard in its appendix.
[173] Buer 1926, p. 193.
[174] Weaver 1928, p. 181.
[175] Clark 1802; Haygarth 1805, p. 2; Bynum 1978 B.
[176] Haygarth 1805, p.143; Weaver 1928, p.182.
[177] Haygarth 1805, p. 1.
[178] Bynum 1978 B.
[179] Haygarth 1784, p. 25.
[180] *ibid*., p.26.
[181] Haygarth 1801, pp. 33-38.
[182] Haygarth 1784, p.25.
[183] Buer 1926, pp. 203-204.
[184] Haygarth 1801, pp. 123-124, 127-128; Currie 1831, Vol. 1, pp. 67, 108-109, 365, Vol. 2, (contains letters to Percival from 1788 to 1797), pp. 47-96, see also this thesis pp.??).
[185] Currie 1797, p. 199.
[186] *ibid*., p. 43.
[187] *ibid*., pp. 4, 6-7.
[188] *ibid*., p. 217.
[189] Currie 1804, pp. 400, 402, 407.
[190] Currie 1792.
[191] Currie 1804, pp. 408.
[192] Currie 1797, pp. 43, 199-200.
[193] Currie 1804, pp.571-572.
[194] Shryock 1948, pp.119-121
[195] Currie 1804, p.615
[196] Hirsch, Vol. 3, p. 43.
[197] Brockbank 1950.
[198] Ferriar 1798, pp. 126-128; see also Bateman 1818, p. 80.
[199] Buer 1926, pp. 204, 206.
[200] Haygarth 1805, p. 182.
[201] Schofield 1963.
[202] Charles Webster, personal communication
[203] Haygarth 1805, p.182
[204] Cullingworth 1904, pp.41-42,48
[205] Rigby, 1822, p.xxxiii
[206] Mem.med.Soc.Lond. 3; 147, (1792), 4; 261, (1795)
[207] See also Murray 1801, Stanger 1802
[208] Rumsey 1826; Bynum 1978 B
[209] Stanger 1802, p.8
[210] Bateman 1818, p.81
[211] *ibid*., pp.80-81, 85
[212] *ibid*., pp.35-36,38,40,48,48-50,51,57,64,66,71-72
[213] *ibid*., pp.30,48
[214] Bynum 1978 B

[215] Niebyl 1977
[216] Edinb.med.surg.J. 9; 459-462, (1813); 10; 353-365, (1814)
[217] Niebyl 1977, pp.475-476
[218] Ackerknecht 1953
[219] Niebyl 1977, p.477
[220] Trans.Ass.F.Lic.K.Q.Coll.Phys.Irel. 1; vii-viii, (1817)
[221] *ibid*., *2*; 10-14, (1818).
[222] Dublin Hosp.Rep. *4*; iii-v, (1827).
[223] *ibid*., *1*; 58-59,64-116, (1818).
[224] *ibid*., p.3
[225] *ibid*., *2*; 10-14, (1818)
[226] *ibid*., pp.110-145
[227] Edinb.med.surg.J. 14; 532, (1818)
[228] *ibid*., e.g. *1*; 122, 246, 248, 386-389, 514-515, (1805); *12*; 245, 375, (1816); *13*; 117, 245, 398, 521, (1817); *14*; 120-126, 256, 389, 653, (1818).
[229] *ibid*., *14*; 402-403, 530-531, 534-535, 541-542, (1818).
[230] *ibid*., *14*; 71-84, 116-119, (1818); *15*; 143-153, (1819); *16*; 145-147, (1820); *17*; 620-634, (1821).
[231] *ibid*., *14*; 547, (1818).
[232] *ibid*., *17*; 634, (1821).
[233] Niebyl 1977, p.478.
[234] Churchill 1849, pp.6, 31
[235] *ibid*., pp.50-51
[236] *ibid*., pp.7,169
[237] *ibid*., pp.6,7,125,474; Rhodes 1977
[238] *ibid*., p.126
[239] *ibid*., p.169
[240] Cullingworth 1904; Newman 1932; see also p. 409 of this thesis
[241] Churchill 1849, pp.206,248
[242] *ibid*., pp.228
[243] *ibid*., pp.234-235,251
[244] *ibid*., pp.241-244, 246-252, 254
[245] *ibid*., pp.7,315,335-350
[246] Kirkpatrick 1913
[247] Bland 1782; Clarke 1788
[248] Trans.Ass.F.Lic.K.Q.Coll.Phys.Irel. 1;384, 399-402, (1817);Churchill 1849, pp.361-362
[249] Clarke 1817, pp.371,399; Churchill 1849, p.31
[250] Clarke 1788, pp.199-200; 1817, p.399
[251] Churchill 1849, pp.445-447
[252] Niebyl 1977,p.474
[253] Churchill 1849, pp.452-453,470,499-500
[254] *ibid*., p.475
[255] *ibid*., pp.451,461,474-475,477,481
[256] *ibid*., pp. 481, 486.
[257] Anning 1969, p.227
[258] Hey 1815, p.xii
[259] *ibid*., pp.168-169
[260] Boott 1833
[261] Armstrong 1814, pp.74,76-77
[262] *ibid*., pp.68-71,119
[263] Quoted by Armstrong 1819, pp.222-230
[264] Clarke 1788
[265] Merriman 1820, pp.304-305
[266] Bland 1782
[267] Granville 1819; Collins 1835
[268] Granville 1819; Merriman 1820
[269] Merriman 1820, pp.163-173,293-297 for distorted pelvis; *ibid*., pp.58-60, and Med.-chir.Trans. 10;50-76, (1819) for ovarian tumour
[270] Cutter and Viets 1964, pp.88-89
[271] Merriman 1820, p.169
[272] Merriman 1812, pp.131-132

[273] Fasbender 1906, pp.851-852
[274] Quoted By Cutter and Viets 1964, pp.121-125
[275] Fasbender 1906, pp.245-264
[276] See Merriman 1820, pp.178,303
[277] Fasbender 1906, pp.851-852
[278] *ibid*., pp.806-871; Merriman 1820, p.168
[279] Batty Shaw 1970, pp.247-248; D.N.B., Vol. 16, p.1187
[280] Fasbender 1906,p.746
[281] Rigby 1822, pp.xiii, 262-264
[282] Rigby 1775, Dedication
[283] Batty Shaw 1970, pp.247-248
[284] Saunders 1782, pp.54-59,61-65,78-158
[285] Rigby 1822, p.xxxiii
[286] See Chapter 6 of this thesis
[287] Neve, in preparation
[288] Charleton 1770,p.3
[289] *ibid*., pp.10-12
[290] *ibid*., p.96
[291] Greenwood 1948 (A), pp.64-65
[292] See e.g. Falconer 1787; and bibliography in D.N.B., Vol.6, 1029-1031
[293] Falconer 1790, p.106
[294] *ibid*., pp.vii, x,xiii-xv
[295] *ibid*., pp. 148-149,154-155,180
[296] *ibid*., pp.105,182
[297] Falconer 1795, Tables I and II
[298] *ibid*., pp.64-65
[299] *ibid*., p.30; Falconer 1805, p.21
[300] Pettigrew 1817, Vol.1, pp.176-178
[301] Mem.med.Soc.Lond. 1;pp.vi,xii, (1787); *ibid*., 2nd ed., p.xvii, (1792); 2;201-226, (1789); 6;v, (1802)
[302] Garrison and Morton, p.161, No. 1836
[303] Roddis 1936;1936; Peck and Wilkinson 1950; Fulton 1953; Posner 1975
[304] Schofield 1963, pp.122-125
[305] Peck and Wilkinson 1950, p.81
[306] *ibid*., pp.42-45; Withering 1785, p.205
[307] Withering 1785, pp.101-108
[308] *ibid*., pp.vi, 189-190
[309] Withering 1785, pp.vii-viii
[310] *ibid*., p.ix
[311] *ibid*., p.xix
[312] Encyclopedia Britannica, 3rd ed. 1792, Vol.6, p.24; Peck and Wilkinson 1950, pp.79-81
[313] Ferriar 1792, pp.xxii-xxiii; see also pp.?? of this thesis
[314] *ibid*., p.38
[315] *ibid*., pp.97-110,123-124
[316] Ferriar 1795,p.93
[317] Ferriar 1813, pp.40-41
[318] Fowler 1785, p.93
[319] *ibid*., p.xii
[320] *ibid*., pp.viii-ix
[321] Fowler 1785, preface, no pagination
[322] Fowler 1785, pp.xi,xiii
[323] *ibid*., pp.89,91-92
[324] Fowler 1785, pp.105,107,124
[325] Fowler 1795, pp.vii,ix
[326] *ibid*., pp.225-226,229-232,247-248,278
[327] *ibid*., p.ix
[328] *ibid*., pp.255-272
[329] Med.Obs.Inq. 6; 400, (1784); Haygarth 1805, p.6
[330] Haygarth 1805, pp.12-16
[331] *ibid*., 8, 20-27,34,46-49,56,74,81-106,121,125,156-173,177-178
[332] Weaver 1928, p.195

[333] Haygarth 1805, p.186
[334] *ibid*., p.33,35-36
[335] *ibid*., pp.32,37
[336] Bull 1959, p.228
[337] Haygarth 1800, p.28
[338] Mem.med.Soc.Lond. 1;p.xiiii, 2nd ed., (1792)
[339] Adair 1787, pp.201-302
[340] Haygarth 1805, p.187
[341] Clark 1802, pp.208-216
[342] Edinb.med.surg.J. 1;504, (1805)
[343] *ibid*., 14; 534, (1818)
[344] reprinted *ibid*., 1;530, (1805)
[345] *ibid*., pp.530-531
[346] Collins 1929, p.17;Wright-Thompson 1963, pp.15-17; Stancliffe 1964, p.9
[347] See e.g. Sorsby 1936; Wyke 1975; Bynum 1978 B
[348] Smith 1821-1823
[349] Davis 1821, pp.388-389
[350] Med.-chir.J. 4; 265-283,450-459, (1817); 5; 281-282,379-380, (1818); Davis 1821
[351] Sprigge 1897, pp.103-110; Gibbon 1922
[352] Lancet 1832-1833, i; 16

CHAPTER FOUR: NAVAL MEDICINE

A. INTRODUCTION

The second officially appointed British professor of military surgery, Sir George Ballingall (1780-1855) compiled a ‘Bibliographical record of works and papers on the diseases and accidents of soldiers and seamen published by medical officers in Her Majesty’s and the Honourable East India Company’s service.” It was joined to the fourth edition of his *Outlines of military surgery*.[1] I counted 28 authors belonging to the Navy who contributed scientific works between 1750 and 1830, excluding authors writing solely on organisational questions of the medical service. There were also fourteen authors belonging to the HEIC some of whom wrote on subjects related to naval medicine. While thirty of these authors dealt with scurvy, fevers and dysenteries (later cholera), either separately or in general textbooks there was also a certain specialization: five authors wrote solely on scurvy, and four exclusively on surgical problems. Their distribution in time is shown in *Table 2*, in which the periods of eighty years is traditionally subdivided into three periods; the 18th century until the beginning of the French Revolutionary Wars constitutes Period I; these together with the Napoleonic Wars constitute Period II; and the subsequent years up to 1830 are Period III.

From the point of view of Ballingall’s bibliography, this subdivision appears somewhat arbitrary, for five out of 25 authors listed with several writings falling in column 1 published within two, Gilbert Blane even within all three of these periods. On the other hand, it shows that authors listed only with specific books on scurvy disappeared by the close of the 18th century, when those writing on yellow fever only just began to appear.

This chapter centres on the leading personalities listed by Ballingall, yet it includes also naval and civilian authors not mentioned by him. And it attempts to give a survey of the kinds of published evidence which eventually led to the eradication of scurvy in the British Navy, and of that used for the choice of treatment of ship and hospital fever. May it be noted that both issues also had a preventive aspect. Yellow fever and surgical questions are to be dealt with in chapters five and six respectively.

As hinted at in my remarks on the naval medical organization (Sir) Gilbert Blane, through the nature of his writings and his personal influence during the Napoleonic Wars, is especially relevant to our theme. Yet the analysis of the works of his forerunners, on whose shoulders he admittedly stood, and of that of his contemporaries reveal the same spirit of true social and preventive medicine, which James Lind for example, considered higher than the merely curative branch of physic.[2] And, as Lind stated also in 1757, the "prophylactic or preventive Branch of Medical Science does, in many Instances, admit of as much, or even more Certainty, than the Curative Part," for its rules were founded on "clear and often self-evident principles .... approved by Reason and established by Observation".[3]

The Navy provided opportunities not only for observation, but for mass-observation. The use of the latter as a scientific tool presupposes the will - or the obligation - to record them. Both were present in the Navy, where the special conditions of ships or fleets sailing into different climates had provided "closed populations" and motives for numerical comparisons for centuries:[4] "Whoever is in possession of those advantages", wrote Blane's contemporary Robert Robertson in 1783, "and will not profit by them, must obdurate indeed".[5]

The recording duties of the naval surgeons were fixed in the 1731 regulations (see above). Initially the importance of adequate records and the possibility of using them scientifically seem to have been appreciated also by the Barber Surgeons' Company, which was to receive the ship surgeons' journals. They resolved in 1731 that all extraordinary cases in surgery picked out by the Governor in the deposited journals should be published periodically at the expense of the Company.[6] But in addition some of the more willing naval surgeons organised themselves in order to promote and provide a "laudable plan for improving medical knowledge" by their labours. Soon after the dissolution of the ancient union between barbers and surgeons by an Act of Parliament of mid-1745, that encouraged also private teaching, i.e. no longer only the teaching organised by the (Barber) Surgeons' Company. They founded the Association of the Surgeons of the Royal Navy of Great Britain on January 8th 1747. It at once invited lecturers to provide instruction, among them the outstanding anatomy teacher William Hunter. Members were asked to report on remarkable cases or drugs at regular meetings and this new society - like the old Barber Surgeon's Company - endeavoured to collect articles for publication. But the latter was never done. This society, the history of which is difficult to unravel, presumably ceased to exist after 1762. Members had included William Hunter himself, the physician at Greenwich Hospital, Lind and some of the authors he quoted in his *Treatise on the scurvy*. One aspect of the Society's life can be gathered from the fact that this book was first planned as a paper to be published by the Society. George Cleghorn (see below) also dedicated his *Observations on the diseases ....in Minorca ...* (1751) to it.[7]

B. **THE CONQUEST OF SCURVY**

1. SCURVY AS A PRACTICAL ISSUE - JAMES LIND'S OBSERVATIONS 1753

James Lind (1716-1794),[8] born in Scotland, had only been an apprentice of a local surgeon, when he became a naval surgeon in the late 1730s. At the end of the Austrian War he studied in Edinburgh (M. D: 1748) and settled there until he was appointed, in 1758, physician to he Naval Hospital at Haslar, a position he held for 25 years.

Although Lind's *Treatise on the scurvy* (1753) was a specific work by an outstanding man, it is a good illustration of the mid-18th century basis of judgement and decision-making, because of the contributions of others quoted at length within it and because the therapeutic recommendations it proposed had such little actual impact. Significantly, Lind dedicated his book to Lord Anson (1679-1762), for it had been the account of the latter's voyage around the work in 1740-1744, published in 1748, with its extraordinary death rate from scurvy, [At least 380 out of a crew of 510 died on one ship.] which had excited his curiosity to inquire more deeply into the subject and to publish his earlier observations.[9] When he went into the literature, he realised that the only descriptions of the disease so far were by seamen and land-doctors "and that no physician conversant with this disease at sea had undertaken to throw light upon the subject."[10] This was one of the reasons why there reigned so much confusion about the diagnosis, prevention and cure of this disease. Lind wrote:

"Legions of distempers .... very different from the real and genuine scurvy, have been classed under its name: and because the most approved anti-scorbutics fail to remove such diseases, hence we are told by authors (Boerhaave and many others) that it is the masterpiece of art to cure it."

Or, as he put it more directly: "Indeed, before the subject could be set in clear and proper light, it was necessary to remove a great deal of rubbish". There is no direct evidence for Lind paraphrasing Locke, who represented himself as "an under-labourer in clearing the ground a little, and removing some of the rubbish that lies in the way to knowledge".[11] But it can fairly be said that he wrote in Locke's spirit for Locke's "master-builder" were his friends Boyle and Sydenham, and Huygens (1629-1693) and Newton, who all worked by way of observation and generalisation of facts, the very method Lind proposed for the advance in the interpretation of scurvy. Lind stressed in his *Treatise* that his work was to be founded "on attested facts and observations, without suffering the illusions of theory to influence and pervert the judgement. Such were "the surest and most necessary guides".[12]

The standard opinion of the time, as expressed by Lind's former chief, the physician to Greenwich hospital, William Cockburn (1669-1739), in his *Sea-Diseases*, attributed scurvy to bad air, congenital laziness and indigestible food. This view followed that of Boerhaave.[13] Lind also believed that indigestion and the seasons had something to do with the disease,[14] but he set out, in his own way, to minimize the theories and supposed cures, such as the earth bath, the elixir of vitriol, and salt water.

What were his facts? Lind's considerations were based upon his observations on board the "Guernsey" and the "Salisbury"; these were augmented by letters to him from members of the Society of Naval Surgeons, as well as by account of voyages from the past. On the "Salisbury" he had made his longest cruises in the Channel fleet during the war of the Austrian Succession in 1746-1747: Eighty out of 350 sailors were laid down by scurvy during this ten week's absence from shore.[15] A similar cruise of thirteen weeks with the Channel fleet in 1747 had led John Huxham in 1747 and 1750 to recommend fruit as a prophylactic and cure for scurvy. Lind quoted him and solicited his collaboration by letter.[16]

Lind's descriptions of aetiology, symptomatology and diagnosis were mostly qualitative although there was a quantitative element in some of his illustrations, e.g. when he illustrated that the current "bad-air" aetiology was untenable by calling into mind the fact that seventy sailors were cured of scurvy even though they had been hidden in the store-room of the "Guernsey", "where there is generally worse air than in any other part of the ship".[17]

This as well as any other passages in his book, especially the famous critical experiment on which he based his view on the prophylaxis and therapy of scurvy, show how Lind relied on his recorded observations both on the aetiology and the means of prevention and cure "beyond all doubt". The experiment began on 20 May 1747 when Lind gathered together twelve cases of scurvy, "as similar as I could have them".[18] They all received the same basic diet, as good a scurvy-producing diet as any biochemist today could devise.[19] Five groups of two patients each received either cider, elixir of vitriol, vinegar, salt water, or an electuary of garlic, mustard, horse radish, balsam and gum myrrh during a fortnight. The sixth group received two oranges and one lemon daily for six days only, because the quantity that could be spared was then consumed. But this short “treatment” had sufficed to render one of the two men again fit for duty. Next to the fruit, cider appeared to have the best effect - as also recommended by Huxham from a comparative trial by his friend Yves on a larger scale involving several ships. The elixir of vitriol and the other remedies were useless.[20]

This experiment is often considered as probably the first controlled clinical trial of its kind ever to be undertaken.[21] As pointed out by Hughes (1975) this is not true for internal medicine, and, as I shall show in my chapter on amputation, neither for surgery. Recent reassessments have mentioned the fact that Lind’s experiment constituted not a controlled study in the very strict sense, since the patients were not chosen at random and since there was no group without treatment.[22] I may add that Lind thought the salt solution - in opposition to current experimental practice - a possible remedy, and at the same time also a possible aetiological agent for scurvy. [According to one theory, scurvy was caused by salted meat, a major feature of the sailor’s diet.] However, the six experimental groups were compared at the end of the fortnight with a vaguely described group of “others who had taken nothing but a little lenitive electuary... in order to keep their belly open; or a gentle pectoral in the evening, for relief of their breast“, in other words “no medicine whatever”.[23] Possibly the course of scurvy without treatment was too well-known for anyone to feel a need for a group without treatment.

Lind judged this small number of observations, reported in detail, as convincing, because the results were sufficiently different from one another. In fact he “confirmed” them by selected observations of others, but these latter were not as reliable as his results, nor were they quantitative. In these, as well as in other experiments designed in advance it was the quality of *basic* observations rather than their quantity (or better both) that was important for Lind. (see above) One proper observation of one case could even be decisive; for instance, Lind said that that he had never had a great opinion of the elixir of vitriol because he had witnessed a patient contracting scurvy to whom he had prescribed it as a reconstituent, i.e.... “while [the patient was]under a course of medicine recommended for its preventiveness”. Similarly, the alleged aetiologies and cures of scurvy, once disclosed by Lind’s experiment, were then also “contradicted by the daily experience of seamen, [and] by the journals of our sea-hospitals...” These experiences and accounts he seemed to have in mind although he never quoted them explicitly.[24]

Lind’s therapeutic findings made little impact on medical opinion in Britain, and the supply of fruit-juice was actually rejected by the Sick and Hurt board the year after their publication (1753). But the *Treatise* of this “man of observation”, as his disciple trotter called him, proved to be a most influential guide for future work in naval medicine: in its three editions [1753, reprinted 1754, 1757, 1772) Lind taught and stressed the use of the experimental trial in clinical conditions; this message in itself “was just as important as his famous cure for scurvy”.[25] In fact, Lind made yet another “mistake”, equally of some consequence as the subsequent history of the fight against scurvy will show: he was, of course, aware of the storage problems for adequate amounts of fresh fruit of fruit-juice during long cruises. Thus he recommended a condensate, called “rob”, to be prepared by evaporating for several hours a dilution of fresh fruit juice in nearly boiling water. But unfortunately, heat destroys much of the ascorbic acid of fresh juice, - and the lack of effect of this rob was noticed by subsequent observers.[26]

The history of scurvy after the publication of the first two editions of Lind’s *Treatise* (1753 and 1757 respectively) affords a good example of the scientific status of British naval medicine in the second half of the 18th century. It has been summarised most recently by Lloyd and Coulter.[27] In hindsight the story of how Lind’s work was received, entailing a lag of 42 years between his clearly described and experimentally “proved” cure and its actual introduction by those responsible in the Navy Sick and Hurt Board, seemed “one of the most foolish episodes in the whole history of medical science and practice”.[28] However, the fact that Lind’s *Essay on the most effectual means of preserving the health of seamen* (1757) was republished in 1762 by the Admiralty as an honour because of his recommending a simple method of obtaining drinkable water by distillation of sea water,[29] would suggest that his literary work was not unknown to the authorities. Moreover the Sick and Hurt Board did not, during the first thirty years, act unreasonably when one considers that Lind’s was only one of a great number of treatises on the subject, [See Lind’s own ‘Bibliotheca Scorbutica’ in appendix to the first edition of his work already.] and that as a naval surgeon his status was lower than, say, that of an Oxonian cabinet writer and FRCP, or of a friend of the Hanoverian James Pringle [Meiklejohn (1954) suggests the possibility that Lind’s sympathies might have been on the Jacobite side.] whose views were in open contrast with Lind's.[30] Furthermore the Board was inundated with the suggestions concerning scurvy,[31] and, not least, lemon juice was by no means a new cure. (A fact which Lind was perfectly aware of). It is worthwhile noticing that Lind's was not rationally derived experimentation, but rather "controlled empiricism", and that, together with his rob, he also recommended a list of vegetable anti-scorbutics which are not effective according to modern experimentation. The latter recommendations were therefore in contrast to Lind's rejection of unwarranted speculation and his professed reliance on carefully observed facts only. Surely, this contrast exists even to a greater extent than mentioned by Hughes, for Lind developed at length a speculative theory of scurvy (see below). Yet more relevant, historically, seems to me the recognition that Lind was successful in promoting comparative clinical trials quickly, possibly even within the Sick and Hurt Board, whose lethargy has often been criticized. Besides the trials with anti-scorbutics related below, it later ordered others, too, e.g. trials of drugs against fevers (see below p.??). This was a new development and an application of the much-praised observational medicine. By contrast, Lind's theory of scurvy was traditionally speculative. It could well lead away from fruit-juice, too, as we shall see in the following section which considers the work of David Macbride.

2. Scurvy as a Scientific Problem: David Macbride's Experiments

David Macbride (1726-1778),[32] in whose views the Admiralty became chiefly interested, was actually an admirer, and perhaps, imitator of Lind. His *in vitro* experiments embodied the hypothesis of Cullen, which accorded with the prevailing vitalistic theory that scurvy was due to a lack of digestive fermentation in the absence of fresh vegetables. From his own experiments he derived the idea that malt, with its potential for fermentation, might be an ideal cure. This was not unscientific by 18th century standards. Rather it was completely rational. By these means Macbride, a hardly known Dublin physician looking out for a stronghold in London, impressed professional and administrative authorities far more than did the empirical observations of Lind, who, by the way, had initially explained the mechanism of the action of his fruit-juices on the basis of the same theory.[33]

Macbride was ten years Lind's junior. They had both been on active sea duty as mates and later as surgeons during the War of the Austrian Succession. They studied after 1748 for a time in Edinburgh, Macbride in London, too. Lind remained in Scotland until his nomination to Haslar in 1758; Macbride settled in his homeland of Ireland. In 1756 he was, with George Cleghorn, one of the founding members of the Dublin Medico-Philosophical Society, and he later became its secretary.

It was to this forum that he first presented, in the early 1760s, the results of his *in vitro* experiments, on fermentation and putrefaction which considered a sequel to those of Edinburgh men, and, above all of Pringle, who was at that time already highly considered in London (see below, p.??). Fermentation was seen as one specific expression of the cementing principle of life, its absence leading to disintegration or putrefaction. Since autopsies of scurvy corpses had shown a great deal of disintegration of putrefaction in many organs, this disease was considered also by Lind, yet without his own observations (1757), a “putrid” disease. A method for its prevention or cure had to counteract putrefaction and/or restore the cementing principle.[34]

Macbride showed the putrid substance to be of alkaline nature. This furnished him an easy explanation of the alleged effect of the elixir of vitriol and of fruit juices. They were acids and therefore counteracted putrefaction. He wondered whether a more easily obtainable vegetable capable of fermentation might not do for the restoration of the fermentative principle. Would not malt, for instance, in the form of semi-fermented wort thus prove an excellent prophylactic and remedy against scurvy?

Macbride had read “Dr. Lind’s excellent treatise on the scurvy”. He fully approved of his conclusions concerning its aetiology. He, too, distinguished between the general, predisposing causes, e.g. moist damp air, and an actual, inciting, cause, i.e. the lack of fresh vegetables and fruit, for the genuine putrid scurvy. It was the latter that they both proposed to discuss. Macbride also agreed with Lind’s evaluation of his therapeutic trial, which fitted perfectly his own theory. Firstly, fresh unfermented vegetables, cured and prevented the scurvy; secondly, semi-fermented liquors like cider or wine were useful for prevention only; whereas thirdly, acids (both organic and mineral), or ardent spirits, were dismissed practically in Lind’s own words.[35] Since, as admitted by Lind, so many fresh vegetables, be they acid, alkalescent, mild, acrid, sweet or bitter, cured the scurvy, this virtue must be owing to some property which they all possessed in common. His ranking showed therefore in Macbride’s eyes the importance of the fermentative quality: the effect of these substances was proportional to their potential fermentability, i.e. the quantity of fixed air they could eventually liberate. The strong acids actually stopped alimentary fermentation *in vitro* and were therefore useless or even dangerous in this disease.[36]

For Lind the anti-scorbutic action of fresh vegetables was also explained by their acescent quality (as opposed to the alkalescent or putrescent nature of animal substance), i.e. to their good digestibility. They worked through their saponaceous, attenuating and resolving virtue *and* their fermentative quality, which made them resist putrefaction (in opposition to flesh and animal substances which tended directly to it). This fermentative quality stimulated digestion, i.e. the setting free of a subtle imperceptible gas transforming food into chyle and blood. Such were the "chief and most essentially requisite" qualities, mixed together from several sources, for an anti-scorbutic mixture.[37]

For Macbride, and here they disagreed, the capacity of fermentation alone of vegetables, when mixed *in vitro* with animal substance and placed in the proper degree of heat, explained their effect. [In the course of fermentation the fresh vegetables "throw off an elastic vapour, or spirit [i.e. new air] of surprising activity endured with a power of *restoring sweetness* to *putrid* animal fluids.] Unfermented malt had this quality, too, and as a substitute for fresh fruit it had obvious practical and economical advantages: it was easily obtainable, storable and cheap; and it might operate also in other putrid diseases, e.g. fevers or ulcers.[38] Thus, despite the repeated anti-theoretical stances by both Lind and Macbride, speculation was an important feature of their work. Yet, their theories were now to be tested by others.

3. The First Trials of the Wort 1762-1773

In 1762, through the recommendation of George Cleghorn, William Hunter and one commissioner of the Sick and Hurt Board, the Admiralty became interested in Macbride. It ordered a trial of the malt in the naval hospitals of Portsmouth (Lind's hospital!) and Plymouth. The trial was envisaged methodically:

"It was absolutely necessary, in order to determine the genuine effects of the remedy proposed, that the patients should, during the time of trial, be entirely debarred from any sort of recent vegetable. [But this restriction] looked so like retarding men's cures for the sake of experiment, that it occasioned a murmur and disgust"

Thus the trial had to be stopped and the Admiralty ordered it to be taken up at sea “where it was expected that patient would cheerfully submit”.[39]

Yet until 1766, the Admiralty was unable to forward him any reports in consequence of its order. In 1764 however, unqualified bad effects were reported from both Plymouth and Portsmouth hospitals by Dr Huxham. And in 1767, in an appendix to the second edition of his *Experimental essays*, Macbride was able to quote a letter he had received from Sir John Pringle, in May 1764, in which the distinguished Army surgeon entirely confirmed Macbride’s theoretical view - actually his own - of the *modus operandi* for a cure of scurvy (from observations on Lord Anson’s voyage and the use of a malt-containing liquor against putrid scurvy and fevers in ships and prisons in Russia). This letter mentions however, that the inmates of the prisons free from such fevers had fresh vegetables supplied in abundance. In this appendix Macbride also printed several other letters testifying, albeit without numbers, to the successes of molasses and wort against scurvy. Thus, although direct evidence of the truth of his hypothesis was still lacking in June 1766, Macbride felt even surer than in March 1764, that “until it is disproved by actual experiment, I shall still continue to think that this liquor goes as far to cure the scurvy as the juice of any recent vegetable”.[40]

The Admiralty, strengthened in its views by Pringle, was keen on having the trial finally implemented according to its orders of 1762, and it therefore directed that it should be carried out on the ships of Samuel Wallis (1728-1795) and Philipp Carteret (†1796) during their circumnavigations, starting in August 1766.[41] This was not a consequence of some cases included in Macbride’s second edition of his *Experimental essays* and less so of his *Historical account of a new method of treating the scurvy...* as stated by Lloyd and Coulter,[42] for these writings only appeared in the spring and autumn of 1767 respectively.

It is a true that Macbride had finally received a first report (dated April 1767) from abroad while the second edition of his *Essays* was being printed and he was able to add excerpts from it as ‘Postscript’. It was by the surgeon of HMS “Jason” commanded by his own brother. He reported for cases in great detail which had been put on wort the same day and which had received the same vegetable-free diet. After nearly two months two of the patients returned to duty while still at sea, one had no specific complaints [It is mentioned that one of the four patients had a *lues venerea*.] and the fourth was but slowly mending. However, Macbride himself did not consider these cases as “altogether conclusive with respect to the anti-scorbutic virtue of the *Wort*" but at least he had shown that the preparation could be taken in large doses, in opposition to the claims expressed in Huxham's letter.[43]

It was the journal of Mr Badenach, a surgeon of the H.E.I.C. (transmitted to him by Dr Hunter even after that of his brother's surgeon so that it could be included only in his *Historical account*), which "put the Matter beyond all Dispute" in Macbride's eyes.[and not, as stated by Lloyd and Coulter, the "prejudiced testimony" of his brother's surgeon. Indeed Macbride reprint Badenach's cases only in his important textbook of 1772.[44] (see below)] There had been enough malt only for six cases, related in detail, in which the disease was more or less checked (in three cases there were relapses!) until they landed. Then all were cured by the use of fresh fruits and vegetable soups landed. Then all were cured by the use of fresh fruits and vegetable soups in five days. This surgeon contradicted himself, however, saying that one of the three relapsing cases and "the rest of the [forty] scorbutics were but very little better when they were landed....[45]

In order to enable the reader to form a judgement of the virtues of the wort as a substitute for fresh fruit, Macbride reprinted the account of Lind's trial of 1747 in his *Historical account*. He thought that the comparison of his cases with Lind's showed that the wort was as powerfully and quickly active as fresh juice of acid fruits, but being so cheap and so easily storable there could be no hesitation in preferring it. He recommended that it could be rendered even more efficacious when mixed with currants and raisins.[46][Modern analysis shows only traces of ascorbic acid in malt, but a very high concentration in blackcurrant (in terms of wet weight).[47]]

Indeed, the Admiralty seems to have been pleased by these results. This was defendable on three grounds at least: 1) On superficial reading, the evidence brought forward so far by Lind and Macbride was similar when it came to the cases actually observed and described. 2) Their patho-physiological theories of scurvy and their explications of the *modi operandi* of the acknowledged anti-scorbutics were not too divergent. 3) wort was much easier to handle and it was cheaper than rob of fruit juices.

Thus, in July 1768, Wallis and Carteret not having returned yet, the Admiralty sent Lieutenant Cook precise orders for another trial during his first voyage, with explicit reference to Macbride's *Account*, a copy of which was sent with the orders.[48] Cook was also expected to try other anti-scorbutics such as fresh fruit and to keep exact accounts. The prevalence of a favourable view of malt on this evidence may be illustrated by the reaction to it of such a prestigious figure as Sir John Pringle, and by that of a simple surgeon to the East India Company, John Clark. In December 1764, very shortly after the first publication of Macbride's *Essays*, Pringle sent a copy of "this very ingenious piece" to Haller in Berne, "for the book deserves to be known." Of course it corresponded exactly to his own view, based on his highly rated experiments of the early 1750s, on fermentation (which he regarded as beneficial to man) and putrefaction (which he regarded as harmful).[49]

In 1768 John Clark, (later in Newcastle), used wort during a voyage to the East Indies. He was aware that the proposals of the "ingenious Dr Lind", advancing the preparation of rob of fruits and berries, had "been found to answer best upon experiment". However, he said, of late "no proposal has inspired greater hopes of success than the malt infusion recommended by Dr Macbride. His theory ... is founded upon the most plausible principles, and is supported by seemingly conclusive experiments made out of the body", (i.e. *in vitro*).[50]

What were the facts reported from the three voyages by Wallis and Carteret, Cook and Clark?

As mentioned above the Admiralty expected experiments with a variety of anti-scorbutics at the same time. On Wallis's "Dolphin" three among the scorbutics selected for the experiment with the infusion of malt were showing severe symptoms: they "either boiled their bread or fruit [!] in it, and after fifteen days fell in some island with plenty of fresh vegetables and cocoa nuts..." Carteret's "Swallow" was less fortunate. Eighteen people were affected; details of five were given, two of whom had died, "notwithstanding they were fully supplied not only with the wort, but also with a variety of fresh vegetables...., which were procured from time to time at different islands". This surgeon believed that the wort did not actually cure, but retarded the process of scurvy [He wondered about the quality of his malt, it being somewhat damaged by insects.), a view also held by Carteret himself in his report after his return in 1769.[51]

It is highly to Macbride's personal credit that he did not conceal the reports of Wallis's and Carteret's surgeon in the Appendix to his *Methodological introduction* (1772) [Translated into Latin, French, Spanish, and German.] in which he gathered all the evidence known by then on the value of wort in scurvy.

In favour of the wort Macbride reprinted Badenach’s six cases, and mentioned “many” others from another East India man which had been cured if the symptoms were moderate, or the progress stopped if they attained a certain pitch. These cases “may be deemed sufficient to establish the credit of this new anti-scorbutic, but none of them come up to” a case of land-scurvy communicated by the prestigious Dr John Fothergill (1712-1780), (we would probably diagnose it today as chronic alcoholic malnutrition), which was cured after a very few days by the wort alone.[52] This case, as well as two reported from America by Benjamin Rush in a London medical society and published in their *Observations and Inquiries* “where the wort alone had cured ulcers”, showed him the correctness of the underlying principle. Upon comparison with the reprinted trial of Lind and considering the practical advantages of wort, there could be

“no hesitation in giving it the preference for general use... to the common anti-scorbutic juices, which from their offensive taste can seldom be taken in such quantities, or continued for such a length of time, as is required to work a permanent change in the state of animal fluids.”[53]

At his last, but as it showed later, most important evidence, he was able to add extracts from the journal of Cook’s surgeon, Perry; Cook’s “Endeavour” had returned to England in July 1771 without one loss from scurvy.

Perry’s was a confused and contradictory piece of work , but it contained a firm conclusion, out of the blue, which was quoted fully by Macbride: after listing the anti-scorbutics used - sauerkraut, mustard, vinegar, inspissated orange - and lemon-juice, sugar, molasses vegetables, etc. - Perry said that “these were of such infinite service to the people in preserving them from a scorbutic taint that the use of the Malt was, with respect to necessity, almost entirely precluded”. It was used in four cases of scurvy in March and April 1769, apparently with all the good effects that could be wished, and from them

“while at Sea the Wort became part of our diet, so that, excepting five cases, ... not a Man suffer’d any inconvenience from this distemper. In the cases I have mentioned, a trial was made of the Robs (of orange and lemon) and attended with success. It is impossible for me to say what was the most conclusive to our preservation from Scurvy, so many being the preventives used: but from what I have seen the wort perform, from it's mode of operation, from Mr. Mcbride's reasoning I shall not hesitate a moment to declare my opinion, viz. That the Malt is the best medicine I know, the inspissated Orange and Lemon juices not even excepted."[54]

As shown most recently by Watt (1978) this statement by a decent young man was meant not to contradict the views of his superiors in the Navy Board and in professional circles. Perry's conclusion, despite its weak basis, seems to have been adopted even internationally, as is illustrated by Vicq d'Azyr's obituary for the Société Royale de Médecine in Paris. Vicq d'Azyr repeated in 1779 that a great deal of Cook's success had to be attributed to the use of wort.[55]

Let us turn now to the facts presented by John Clark after his journey to the East Indies. His *Observations* were first published in 1773. He reported on four cases in great detail. They had received the wort for a maximum of three weeks, but as the scorbutic symptoms constantly increased, they were all given fruit and vegetables. In two other cases they had aggravated so quickly, that the medication had to be discontinued earlier. From another of the Company's ships which had been at sea in 1770, two similar cases were described in detail and four were mentioned briefly. All were totally cured only after they came on shore. Clark thought himself "wanting in duty to the public", if he had concealed these cases. However, as the malt infusion had been supported by very strong instances, namely the cases related in Macbride's *History*, he suggested cautiously that "it may still seem to merit some further trial". For this purpose he recommended strongly a comparative trial - as Lind had made: could not the beneficial effect reported by Macbride on the "Jason" have been due to the nourishing diet seasoned with garlic, currants, rice and sago given to all four patients in addition to the wort? Had two of them been put on this regimen without, and two with the wort, the latter's effects might have been more precisely ascertained.[56]

Thus the first clinical trials of the wort set the balance officially in favour of this remedy on the basis of well intentioned trials, but which, *in praxi*, could not be called scientific, even by contemporary standards. This fact was clearly recognised by an independent observer such as John Clark. It is of particular interest, therefore, to analyze the attitude James Lind took to them, nearly twenty years after his first publication on scurvy.

4. THE ATTITUDE OF JAMES LIND 1772

In 1772 Lind published the third edition of his *Treatise*. In a 'Postscript' he inserted the substance of four volumes of observations, daily and carefully made in the chambers of the sick at Haslar Hospital. Just as during his service afloat, Lind had kept records of all his patients: during the first two years of his activity there he saw 1146 cases of scurvy out of 5743 patients.[57] During the Seven Years' War (1756-63) he said he had frequently visited three or four hundred scorbutic patients a day.[58] Let us see the use he made of this unique opportunity. Again Lind would not publish lists of his cases, nor numerical *results* of his therapeutic trials nor of his autopsies. But there was a change in his theory on scurvy according to his own autopsy finding. Lind became reluctant to assert that scurvy was a "putrid" disease - anyhow a badly defined designation as he now thought.[59] This was important for it shook the rationale for the malt therapy. Besides this, Lind's view of the disease had not changed very much during the fifteen years spent at Haslar.[60] Justifying his change of view concerning putrefaction, he said:

"some doubtful theoretical doctrines remained unaltered, as resting on the faith and dissections of other authors, and as being agreeable to the present theories of physic; .... but the theory of ...[scurvy] as well as of many other diseases, is in general merely conjectural, and is always the most exceptional part of a medical performance.... it is indeed not probable, that a remedy for the scurvy will ever be discovered, from a preconceived *hypothesis*; or by speculative men in the closet, who have never seen the disease, or.... at most, only a few cases of it."[61]

These clear remarks were probably aimed less at Macbride than at some other contemporary British authors writing from their closets (see above, p.??). [This would also appear upon reading Lind's review of Macbride's *Essay* which he calls "useful and ingenious."[62]]

Lind's new clinical experiences were summarised in the 'Postscript' to the third edition of his *Treatise*. Several experiences of e.g. "some thousand", "several thousand" "above two thousand" "some hundreds", and of ten or twelve "out of the number of 100 scorbutic patients" were hinted at.[63] As to the cure of scurvy, he inserted letter from four naval surgeons relating a total of 232 scorbutic patients cured with fruit juices during the Seven

Years' War. He said that the wort had not produced "any considerable effect" in the trials on Wallis's and Carteret's circumnavigations; he quoted however the testimony of one of Carteret's soldiers who had assured him personally that he had been restored to health by it.[64] In fact, when reporting his own trials, Lind also had a good word for the wort; whether he knew already of Perry's report, approved by Cook and the Admiralty, is uncertain: The Admiralty had received this report, dated 12 July 1771, before the 2nd August 1771, and Lind's manuscript was finished on the 30th August.[65] Macbride's infusion of malt was the only omission from Lind's list of "all the medicines and methods of cure that have been recommended for this disease" of which he had made it his "study for some years, with unvaried diligence, to observe the effects by putting them to the fairest trials". [i.e. Scorbutic juices, scurvy-grass juice, Peruvian bark in large quantities, infusions of guianac...] Nor was the wort dealt with in the main text, but only mentioned in a footnote:

"I put 130 scorbutic patients under a course of it for fourteen days,... it has the advantages, when newly made, to be extremely palatable, the patients were very fond of it, and there was not one instance of its occasioning sickness, gripes, or purging. On the whole, it is a very nourishing liquor, well adapted for scorbutic patients."[66]

It was a cautious, non-committal statement, but considering Lind's popular position among his naval colleagues by 1772, (Meiklejohn 1954) it might well have been interpreted as evidence in favour of the wort.

Lind's had been controlled studies, the patients were confined in special wards, strictly watched and "debarred from eating any green vegetable, fruits or roots whatever, though many of them had not tasted anything of that sort for several months; they were not even permitted to taste the hospital broth"; and their state was followed daily.[67] Comparative effects of different fruits and vegetables had been assessed likewise. These experimental conditions had not failed to impress colleagues working on scurvy like John Clark. Thus Lind had changed his theory of scurvy according to his own observations, a fact which is well worth stressing in view of recently expressed doubts of his practicing experimental science. Yet in terms of therapeutic recommendations, he at best remained stagnant, at worst paid tribute to his time. As his counterpart in the Army, John Pringle, Lind was both an experimental scientist and a theoretician working in the speculative framework of his time. But Lind at leas undertook the verification of one of his hypotheses. And even with respect to the therapy of scurvy he may have been more objective an observer than previously thought of , for many of his scurvy patients probably suffered from a mixed deficiency of both vitamins B and C,[68] and wort *was* rich in vitamin B complex. One of his conclusions in 1772 was therefore not astonishing:

"A work, indeed, more perfect, and remedies more absolutely certain might perhaps have been expected from an inspection of several thousand scorbutic patients, from a perusal of every book published on the subject, and from an extensive correspondence with most parts of the world...; but, though a few partial facts and observations may for a little, flatter with hopes of greater success, yet more enlarged experience must ever evince the fallacy of all positive assertions in the healing art. *Est modus in rebus, sunt certi denique fines*."[69]

5. The Second Voyage of James Cook 1772-1775

But by the time Lind's third edition appeared, Cook had departed already for his second voyage (1772-1775), which in terms of survival from disease proved to be an even more spectacular success than the first [During a voyage of 70,000 miles lasting over three years in every variety of climate he lost only three deaths from accidents and one from consumption out of a total complement of 118.] On the basis of the same kind of superficial evidence as on the first voyage, the value of wort was professed in a paper that Cook himself read on March 7th 1776 at the royal Society:

"This is without doubt one of the best anti-scorbutic sea medicines yet found; and if given in time will, with proper attention to other things, I am persuaded, prevent the scurvy from making any great progress...; but I am not altogether the opinion, that it will cure it in an advanced state at sea. We have been a long time without any [wort] without feeling the want of it, which might be owing to other articles."[70]

The last sentence, ambiguous and contradicting the first part of cook's statement, was however dropped in the printed version of Cook's paper and therefore escaped public attention. Cook allowed to having been provided with rob of lemons, "which the surgeon found useful in several cases". But on July 7th he wrote to Sir John Pringle, then President of the Royal Society, again confirming his low opinion as opposed to the high price of the rob in which he was perhaps not entirely agreed with Cook; and later that year, when awarding Cook the Copley Medals of the Royal Society (for the best paper of the year), he publicly praised both Macbride's fermentation hypothesis (see above) and Cook's description of the experiment with malt.[71] [Perry's, Cooke's and Pringle's documents were published by Pringle in the same year 1776.]

From hindsight it results that Cook's and Pringle's inability to discriminate between essential and contributory factors in scurvy delayed the general introduction of lemon juice. The unusually small incidence of scurvy on Cook's ships is rightly to be put down to his superior officership and the singular advantages he had of obtaining fresh vegetables. It must be stated, however, that the reason why Cook and his surgeon had thought so little of the robs was that Cook had been recommended a purely conjectural dose; the experiment had been done with this dose, but with so little advantage, that judging it inadvisable to lose time, he turned to the wort only. Pringle himself thought it probable that, because of their evaporation to rob, the fresh juices had been weakened, "having lost their aqueous parts [and] not a little of their aerial, on which so much of their antiseptic virtue depended". He proposed further trials with entirely purified juice, because there were "some numerous and some strong" testimonies in favour of its healthy qualities that a few failures - as in Cook's case - were not sufficient for striking it off the list of probable preservatives against scurvy.[72]

For a while, the wort had won the race. There had been very little evidence against it, for even that by Clark, given in mild terms as it was [Lloyd and Coulter quoted from the revised 2nd edition of Clark's Observations, which appeared in 1792, and which indeed contained strong evidence against the wort.], was outweighed by Lind's statement of 1772 that scurvy could appear despite a vegetable-rich and easily digestible diet, which on the other hand "proves a certain (i.e. a sure) means of relief to others from this disease", according to experience at his Hospital in 1759.[73] Cook's and Pringle's statements were overwhelming,[74] and internationally recognized. Macbride quickly and proudly quoted them in 1777 as his principal testimonies for the use of wort in the Navy and in garrisons. Fairly enough, he credited Lind entirely for the prescription of fruit juices and fresh vegetables. Their drawback, however, was that they presupposed favourable circumstances for obtaining them.[75] A change of general opinion was only brought about by the extensive numerical data from the American War.

6. The American War: Robert Robertson's and Gilbert Blane's Returns

Meanwhile the War of American Independence had begun in 1774 and was to become general in 1778. The official policy for the prevention of scurvy derived from Cook's recommendations: the Sick and Hurt Board allowed wort, sauerkraut and potable soup, whereas rob of fruit juice was considered ineffective and too expensive. Some naval surgeons, however, looked on it as a medicament which they might occasionally provide from their own purse.[76]

During this war nosological tables, which included for the first time the results of different treatments for scurvy, were compiled and published: It is the great merit of Robert Robertson F.R.S. (1742-1829) to have initiated this practice in the Navy during active duty afloat. Before entering the Navy in September 1760 he had kept a journal while serving as a surgeon in the Greenland whale fishery, and he continued keeping daily records during his whole practice afloat until 1783. But more than that, he continually wrote them up and summarised them in "pathological and comparative tables to show the efficacy of different modes of practice".[77] As he served in Africa, the West Indies, North America and the Channel Fleet good accounts on the naval medicine during the American War became available from these theatres (*Journal* 1777, *Jail fever* 1783). Gilbert Blane, who became physician to the West Indies fleet in 1780, wrote *Observations on the diseases incident to seamen*, which appeared soon after the end of the war in 1785 (and was re-edited in 1789 and 1799). These three books illustrated not only their authors' passion for statistics, but also their ability for drawing succinct conclusions from the elaborate nosological tables which they loved to compile.

Robertson listed thirty cases of scurvy in his *Journal* (1777) none of whom had died on board. He had used Lind's method of cure, to which in his opinion nothing needed to be added.[78] Blane, having read Lind and Cook, wisely limited himself in his instructions printed upon his appointment in 1780, to merely stating both their opinions, and he recommended both malt and lemon. His application in 1781 for a stock of lemon juice was refused by the Board, who referred to the testimony of Cook and his surgeon: This "rob" was "of no service", being inferior to fresh fruit and far too expensive.[79] Promptly, scurvy broke out once more. Blane, as physician to the fleet, did not hesitate to present his findings in the form of 'Memorials' to the Sick and Hurt Board as early as during his first leave to Europe in October 1781 and again in 1782.[80]

It is from this time onwards that the Board may be blamed for its hesitating attitude, for from the statistical evidence it became clear, that scurvy was not being kept under control. The letters of surgeons asking for a more liberal supply of preventives increased.[81] Furthermore, Robertson's and Blane's tables gave for the first time regular statistical information on the nosology of both scurvy and fevers with a view to their better understanding. Scurvy appeared as a worse scourge than typhus in the West Indies; both were associated with cold latitudes; but whereas typhus disappeared with the sun, scurvy continued if the ships remained long at sea. For instance, Blane reported that by May 1781 no fresh vegetables had been taken on board since January of the same year and that there were 1077 cases of scurvy compared with 678 in April and 543 in June, when supplies became available again. The superior efficacy of fresh lemons as compared with the rob, was also noticed. Blane called the native juices of lemons and oranges "real specifics in that disease, if anything deserves that name". [Although he attributed a certain value to sauerkraut, molasses and malt.] It could also be stated precisely that scurvy was not infectious, that it was not due to a defect of digestion, but to a defect of diet. And it was realized that it was not the land *air* which was curative, but the *diet* ashore.[82]

Thus the deliberate use of quantitative methods during the American War yielded a clearer description of the aetiology of scurvy, and a more precise assessment of the remedies employed was gained by the mid-1780s by Robertson (1777), Blane (1785), and again John Clark. The latter contributed not at least by his blunt numerical evidence that the currently used treatments had been largely ineffectual (1783). Frederick Thomson for instance, another naval surgeon, relied in his *Essay on the scurvy* (1790) on Lind and especially on Blane, some of whose tables he reprinted as clear instances of the still possible ravages by scurvy. Unfortunately, Thomson had lost his own papers by accident but he was nevertheless determined to confine himself to "practical observations and facts" as he did not think one could find any good information as to prevention and relief "from a regular scientific work like that of Dr Cullen".[83] [As to the classical preventions of scurvy they might help, he thought "when swallowed by pounds and quarts" but this was not the point for which such a medicament was searched for, with oranges and lemons a small quantity, used daily sufficed - however not in dosage of droplets either.]

On the other hand such observational books, especially if they contained many figures, were sometimes considered, a new kind of dull literature, and were despised by many.[84] However, they must have proved more convincing in the long run, even to the authorities, than the conjectural arguments of men of the opposition, who, like William Renwick (1740?-1814) in 1792, claimed without such proofs that scurvy was “not occasioned by diet to which it is so generally attributed, but.. [by] mephitic exhalation, by which the animal fluids are very differently affected”. Renwick therefore pretended that the distribution of lemons and oranges had been less favourable than would have been as many pinches of snuff to sustain the vital powers - and this in a fleet for which precise returns existed by then.[85] It was perhaps luck that the “right” side fought with those better arms, for with statistics, too, one could prove many things, as I shall outline especially in chapter six.

7. The Conquest of Scurvy 1795

By the 1780s there remained still the task to impress the utility of the fruit juices upon the hierarchically responsible directors of the Navy. The final approval by the Sick and Hurt board was apparently accidental. Part of the story says that in 1793, upon Blane’s advice to a friend appointed to East India, a fleet well supplied with lemon juice (kept with alcohol) reached madras scurvy-free after nineteen weeks without touching at any port. This remarkable demonstration of the effect of lemon juice enabled Blane, now himself a commissioner of the Board, to persuade the Admiralty in 1795 to sanction the issue of lemon juice on a far more generous scale than ever before. Yet a number of naval surgeons claimed equally credit for its introduction in the fleets under their responsibility without there being a general order.[86] This is perhaps of less concern to us than the fact that the consequences again were easily expressed numerically. For instance, it is said that, when in 1797, the First Lord of the Admiralty asked to see a case of scurvy when visiting Haslar Hospital, no such patient could be found. Although such cases continued to be frequently reported, nearly all could now be cured on board. Thomas Trotter, (1760/1-1832)[87] the physician to the Channel fleet from 1794 who had asked for and received lemons and oranges before sailing in April 1795, was able to state that “upon comparing notes and remarks from the reports of the surgeons .... not less than three thousand cases, unfit for duty had been cured, on board”.[88] In 1815 Gilbert Blane showed that the scurvy had almost disappeared from the fleet: according to the figures sent to him by Dr John Lind, the son and successor of James at Haslar, only two cases had been sent to this Hospital in the last four years of the Napoleonic Wars.[89]

In summary the history of scurvy in the British Navy during the second half of the 18th century shows how comparative clinical trials were well described by Lind and Clark, yet imperfectly set into practice on a very small scale. Simultaneously the patho-physiological explanation of scurvy was still speculative, at least in its earlier decades, not precluding thus the episode of Macbride's wort. Yet, due to an interplay of accurate observation and simple numerical records from individual ships and whole fleets in wartime kept by Robertson and Blane both the clinical features of scurvy and the effects of means of prevention and therapy became better assessed. This led ultimately to a change of opinion in favour of lemon juice within the authorities, both professional and political, directing the naval service, and thus to the conquest of scurvy.

C. FEVERS

1. INTRODUCTION

The main issues concerning fevers were, as mentioned in my introductory remarks, their identification, classification, contagiousness and therapy. They were all dealt with by naval authors. In this section I shall discuss those aspects in the work of Lind and Robertson whom I have mentioned quite extensively in the foregoing section. They became the Navy's expert writers on tropical, and ship and hospital fevers respectively in the later 18th century. This will allow me to give a further, yet not exhaustive, picture of the work these important naval surgeons published.

2. JAMES LIND

Lind was not only an authority on scurvy, but he used his position at Haslar Hospital also for an extensive study of fevers. The first outcome was *Two papers on fever* [Translated twice into French.], prepared in 1761, which dealt mainly with jail, hospital and ship fever. The introduction reads like that to Lind's previous *Treatise on the scurvy*:

"A very extensive practice in fevers during three years in one of the first hospitals in Europe, qualifies me, in some measure, for making researches into he dark and abstruse subject of infection, [to elucidate] a chaos of contradicting precepts."[90]

Lind was a practical man, with a duty to perform and he had little patience with the then innumerable attempts to classify or to account for fevers theoretically.[91] He classified fevers according to their location rather than their symptoms. He realized that jail, hospital and ship fever were the same, that they had the same aetiology, namely crowds, dirt, close contact and semi-starvation. A number of statistical returns from ships and from his hospital illustrated his opinion that fever was "of all diseases... the most destructive of mankind".[92] They also supported his view that the common ship fever was infectious, as all his epidemiologic conclusions and hygienic recommendations were based on such numerically stated facts.[93] To check contagion he adopted strict measures of isolation and burned infected clothing. He fumigated the wards with brimstone, tobacco or gunpowder. In fact he introduced separate fever wards at Haslar twenty years before Haygarth did so in Chester. As the latter, Lind was keen on accounting the result of this innovation, especially the non-spreading of contagion. He wrote:

"As the best proof of the efficacy of any method, is the success with which it is attended, I here gave you an account of the mortality amongst the nurses, servants and all other persons in the hospital (exclusive of the patients), from June 1758 to January 1760."[94]

This mortality was in fact below 5%, but Lind did not give the number of those who had caught the disease.

As to therapy, Lind made some judicious statements, too: "I have", he wrote, "often thought, that publishing only one or two singular or particular cases, does more harm than good". If the medicament or mode of treatment was said to be salutary on such a basis, this was not convincing enough to discern whether the constitution alone would not have performed the cures. If it was said to be ineffective, this did not preclude its efficacy in many other cases, for because of personal habit and idiosyncrasy "all the maxims of physic are limited, and there is in it no universal infallible method or remedy". It was true that a general plan could be gained from a large series of observations, but even this did not mean that such a plan would apply to each individual case.[95]

In practical terms Lind recommended bleeding only occasionally, in cases of light fever. He thought it dangerous in the malignant pestilential fever ("typhus") where he recommended certain antimonial medicines as febrifuges, local blistering and clysters, still hoping that a specific might be found for these fevers (as was the bark for the intermittents).[96] In accordance with his general outlook, Lind claimed the more attention to his indications as they were not founded on private observations, "or on any one particular case, which might prove an exception to a general established principle in practice. They are the result of some thousand patients, whose cases are still preserved in the hospital".[97] In fact, during the visit of the Russian fleet in 1769, (4,200 men), 1521 cases of typhus were landed at Haslar, and only 86 of whom would die.[98]

In 1768 Lind published the first edition of an *Essay on diseases.... in hot climates*, which became a standard work [It was still edited in England forty years later and in America even in the 1810s.] written for the benefit of seamen, soldiers, and emigrants alike - a subject of fundamental importance for the British enterprise abroad. A long 'Appendix' dealt with intermittent fevers, which were also prevalent in some English counties by the way. The approach to theory, classification, prevention and therapy of these fevers in the *Essay* equalled that to the continuous fevers in the *Two Papers*. Practically his plan consisted in bringing about a remission of the first hot fit by tartar, blistering and opium (but not with bleeding). In the interval he started with Peruvian bark. As with the scurvy he listed over fifty other possible cures some of which might be occasionally helpful. But usually his simple plan would do for all types of intermittents,[99] as proved by his overall results stated numerically, but not in precise a manner.[100] [E.g. "Of between four and five hundred patients, afflicted with remitting or intermitting fevers, under my care in the year 1765, I lost but two; neither of whom had taken the bark."]

The choice of bark for agues (Malaria) needed a probably no special justification in Lind's opinion, since Sydenham had already recommended it. His use of opium to abbreviate the first hot fit, however, was based on the following trial: in one fever ward, Lind had given it to all 25 patients, nineteen of whom had felt immediate relief; with three there had been no change, and the three remaining had not taken it (no results stated). Upon this success, Lind administered opium to another dozen patients the next day, in eleven of whom it removed the headache and abated the fever, so that bark could be started earlier than usual. Since that time he had (at the moment of publication of his *Essay*) given opiates "to upwards of three hundred patients labouring under this disease" and noticed its effect in the hot fit.[101]

As in the case of scurvy, the Admiralty had ordered trials of medicaments against fever, especially of Dr James's powder. Lind was probably prejudiced against this panacea (see above). He thought that, considering the different nature of fevers one powder would hardly be universally effective. And - in about the same years as Millar and Lettsom - Lind campaigned against the secrecy of medicines: the powder was not likely to be of general benefit to mankind unless made public. Yet Lind had to obey orders and give it at Haslar, in various cases “to above a thousand patients”. He found it to have about the same effect as tartar emetic in similar cases. But he admittedly had continued his usual treatment “as if no such powder had been given”. Decidedly Lind did not mean to make a case for the powder right from the start of his “trial”.

Lind used his hospital facilities consciously for a number of trials of febrifuges - for instance a comparison between *vinum antimoniale* and tartar emetic.[102] However, as in those with various anti-scorbutics, he did not present the results with precise numbers, the only exception being the trial of opium.

Yet, as shown above, Lind realized, and repeatedly wrote upon, several valuable points concerning the evaluation of therapy: A given treatment could only be effective in a certain proportion of patients, requiring in turn large series of observations before it could be adopted or rejected. Success, expressed in terms of comparative mortality, was the measure of all plans of treatment. Accordingly, Lind stated some results numerically, which he had compiled early in his hospital career directly from the records. He kept them unaltered in the later editions of his works on fevers.[103] After Monro, Lind was perhaps the first to have published success-rates obtained in one group of diseases, with one known method, in one hospital during a given time. He could do this directly for his own practice at Haslar. Because it was insufficient for the fevers of the West and east Indies, Lind relied for them on the descriptions of Robert Robertson’s papers - even before the latter had published some of them.[104] Robertson on the other hand, became the leading expert on “typhus” in naval circles in the 1780s... Thus, even the imperfect publication of results by Lind as from 1763 was a departure from the vaguely supported claims of a Boerhaave, Pringle or James, who fought with isolated cases or indirectly with figures selected from the Bills of Mortality. It was a departure towards a more objective evaluation of therapy. Lind had shown a way at Haslar, which first Robertson and John Clarke and then Blane were going to walk with perseverance during their active service afloat in several parts of the globe, and later in their respective hospitals in England.

3. Robert Robertson

Robert Robertson (1742-1829) has been unduly neglected and misrepresented by historians. Lloyd and Coulter do not list his many writings in their bibliography, and in their text quote only from his books published in the early 19th century, although they mention that he had published a *Journal* by 1779.[105] Some of his sick-lists are (partly) reprinted (with faults), and erroneously ascribed to Blane.[106] Robertson was a Scot who entered the Naval medical service with the low rank of surgeon's mate in 1760. He was thus on active duty during the Seven Years' war and remained so until the end of the American War in 1783. Meanwhile he graduated M.D. of Aberdeen in 1779. Early in his career he started keeping accurate registers, not to be "cursory remarks made for amusement in the idle... hours" but clearly meant as a "specimen of a plan for obtaining a further knowledge concerning diseases, by recommending to gentlemen of greater abilities and experience, especially surgeons of the Royal Navy, the keeping of accurate registers of diseases, their symptoms and cure, in the course of other voyages". He referred to Lind who had indeed stressed as we have seen that "knowledge in physic can only be obtained by a series of observation", and who had thus shown how to separate "experienced truth from hypotheses". Robertson, too, declared that once could judge the true pathognomic symptoms and the effect of a cure not from one but from a great many cases; that all cases, the successful and especially the unsuccessful ones had to be related minutely to permit a positive judgement; and that merely general assertions could contribute little towards promoting the real knowledge of medicine. This programme, concerned with obtaining greater certainty of diseases and the effect of cures, accompanied the publication in 1777 of Robertson's *Journal* for 1772-74 (1777).[107] [A part of it had already been included by Lind in his *Diseases incident of Seamen in Hot Climates*]

As to the arrangement of his *Journal* he admittedly imitated Huxham's *Observations de aere et morbis epidemicis*.[108] Indeed he detailed not only the sick but also the weather: readings of the thermometer, barometer and exact position of the ship in degrees longitude and latitude were listed daily. But in his monthly review of the prevailing diseases (a development by Huxham of Sydenham's annual review) he went a step further than Huxham: his incidences were numerically stated, and he compiled a summary of the number of sick and dead according to a list of diagnoses.[109]

In opposition to what Lloyd and Coulter say[110] Robertson did draw practical conclusions from his statistics. He reduced the number of fevers to five {remitting, intermitting (malarial), slow nervous (typhus?), catharrous, and slight fevers (flu?)} which were distinctively defined. In his opinion, further subdivision of the remittents, e.g. - according to Hoffman, or Celsus, was irrelevant. From his own experiences in England, Africa and the West Indies he could also ascertain that these fevers were the same the globe over. As for curing them, he opposed indiscriminate bleeding and advocated instead liberal use of the Peruvian bark, the effect of which he presented numerically. With this practice he had not lost one case of remittent fever out of 62.[111]

Robertson's honesty is noteworthy. For the cure of dysentery, for example, he admitted imitating Huxham's and Pringle's prescriptions, albeit with some alterations. But since he lost nine out of 96 cases, even though some were mild or simple relapses, he could not "help thinking that I was unsuccessful in my method of treating this disease".[112]

Robertson's *Journal* was followed in 1778 by John Hume's *Account* of West Indian fevers. Hume (1706-1772)[113] was by then one of the commissioners of the Naval Medical Board. He recommended the same therapy supported by hospital returns which he had made out as early as 1741-1742.[114]

Robertson's next book, the *Observations...* (1783) appeared still before Blane's *Observations* (1785). It was dedicated to William Hunter, whose lectures to the Society of Naval Surgeons had excited the "attentive spirit of enquiry, which now prevails among the navy surgeons". As in all his subsequent writings Robertson repeated his programme defending it against those who think it "dry and insipid reading or altogether useless in the practice of physic".[115]

In this work, whilst tabulating the monthly incidence of all disease, he concentrated on continuous fever. The data (number of patients, deaths, and evacuations to hospital) were presented for April 1776 - May 1782.[116] Having shown numerically, in an unilateral *post hoc - propter hoc* manner, that typhus could successfully be treated with bark in his *Journal*, he promptly made his case even stronger, with a comparative statement, when exterior circumstances forced him temporarily to abandon this method. [Bark was expensive, and not liberally provided by the Sick and Hurt Board.[117] Thus inadequate supply of the bark as well as sticking to the traditional theory of fever could be reasons for different therapies.] When he was a surgeon of the "Juno" his stock of bark lasted from April to December 1776. Afterwards he had to rely on various other methods until July 1778. In a table he compared the results of both treatments (obtained thus on the same ship), such tables being to him the last argument and positive evidence on the subject.[118]

Continuous fevers

Under the bark Method		*Under all other Methods*	
(April 4 - December 31, 1776)		(Jan. 1, 1777- July 30, 1778)	
Were treated on Board	Died on Board	Were treated on Board	Died on Board
216	1	296	6

69 patients (19 under bark treatment) were sent to two hospitals on shore, nine of those under either antimonial or camphor treatment died. No result was stated for the bark patients. The proportional mortality of the latter was therefore 1 in 216, whilst that of all other tables Robertson compared these results with those obtained with bark and other therapies at the New York and Rhode Islands military hospitals, again during a fixed period.[119]

When he was transferred to the "Edgar" in 1779, Robertson wanted further to ascertain the efficacy of bark in typhus "regardless of the expense which I knew must attend the experiments, tho' I could ill afford it".[120] This time he compared his results aboard ship with those of the Gibraltar Garrison Hospital with which he had been in touch in early 1780 - on an occasion on which he had shortly met Blane (see below). Robertson had again used bark, whereas the practice at the Hospital had consisted in antimonials, camphor and bloodletting.[121] The figures were as follows:

On Board the "Edgar"		*In Gibraltar Hospital*			
(1 July 1779 - 1 Aug. 1780)		(19 Jan 1780 - 20 April 1780)			
		Pat. of the *British Fleet*		Spanish prisoners of War	
Fever	Died	Fever	Died	Fever	Died

patients		patients		patients	
475	6 or 1 in 79	570	57 or 1 in 10	437	33 or 1 in 13

Even if 28 patients had been sent from the ship to hospital for "convalescence", this numerical attempt at comparative evaluation on therapeutic "experiment" [I have simplified these tables, but all the figures, inclusive of the proportionals, are original.]. He was right in stating that "few practitioners in physic will have so many cases come under their care, as the preceding observations are founded on; and consequently as few readers will ever have experimentally, authority to deny the validity of them".[122]

With his comparisons Robertson did not aim to criticise the hospitals as such in the way of certain contemporaries, such as Pringle and Howard. His aim was the comparison of treatments. Thus he had given "irrefragable proof" of the superiority of the bark in continuous fever and would only submit to objections based on similar comparative trials on a large scale. Even if the advocates of the traditional Dr James's powder claimed that many of their patients had died because of too low a dosage, one could say that deaths under bark might be equally due to the same defect; besides, he stressed, it was an unalterable fact that for one fatality under bark management, fifteen died under other methods of treatment. Robertson conceded however, that the nature of the disease or the properties of the bark might change with time and that a new evaluation would be necessary in order to demonstrate this.[123]

In the second edition of his *Observations* (1789) Robertson could acknowledge Millar's approval of his kind of evidence. He was by then able to differentiate further that not only the liberal use of bark, but also its early prescription, was important for the cure of all varieties of fevers: from 1783 till 1789 he had been engaged in private practice in Hampshire, but he had also undertaken three voyages to America on HMS "Salisbury". In his country-practice he admitted to have been unsuccessful with the treatment of fever. His annual mortality there varied between one in thirty-three and one in six yielding an average of almost one in fourteen out of a total of 228 patients, whereas he had lost none out of 159 on his trips on board the "Salisbury". As he emphasized himself, "The contrast between the success stated... [in these two tables] is so striking as to attract every reader's attention". He attributed it to the fact that in the Navy the doctor was consulted early in cases of fever, that he saw the patients often and had authority over them. In private practice on the other hand, he was often called too late and encountered "the obstinate disposition of the lower class of my patients. A very melancholy and important lesson to the community".[124]

In a further *Essay on fevers* (1790) [printed at his own expense] Robertson advocated the extension of his plan "beyond the limits of navies and camps to civil service at large", in peace and war. He had in mind especially the great metropolitan hospitals whence only overall mortality figures were available which were not broken down to diagnosis and which made no indications of the methods of treatment. He concluded his plea: "For God's sake, let mere theory or hypothesis no longer regulate the profession of a science upon the success of which the interest and lives of mankind depend".[125]

From 1790 till 1807 Robertson was physician at Greenwich Hospital and thus senior physician of the Navy. His *Diseases incident to seamen* (in four volumes) which he had printed from 1804 to 1807 contained his previous works and the results of his practice at Greenwich Hospital, again with accurate statistical information. An abridged version in two volumes was published in 1810-11. Robertson resumed active service from 1814 to 1819.[126] He became a L.R.C.P. in 1783 and Fellow of the Royal Society in 1804. A more detailed study of this humane naval physician would be well worth while. From the point of view of my thesis he deserves credit for introducing the numerical method outspokenly as a basis for evaluating a therapy into the Navy. For him clinical medicine was a "science", regulated (as all sciences) by "experiments". In this respect it is also noteworthy that he "was introduced to an acquaintance with Dr Millar" in 1779. Afterwards he became engaged strongly in proving the superiority of bark treatment in typhus with the aid of comparative tables of results. He also acknowledged John Clark's work along this line.[127] What the ship-surgeon Robertson did out of personal initiative, with a special interest in fever, was with Blane the physician to a fleet, to become official and applied to diseases in general.

D. **THE SPREAD OF THE SCIENTIFIC USE OF RETURNS**

1. GILBERT BLANE AND OFFICIAL RETURNS

a. In Active Naval Service

Gilbert Blane (1749-1834)[128] [He was also made the subject of a novel entitled “Physicians extraordinary”.[129]] was doubtlessly the most distinguished and influential British naval doctor of our period. His long career extends from his days as a pioneer of clinical statistics during the American War of Independence in the early 1780s, right into the mid-1830s. a most industrious and conscientious Scotsman, as were his seniors Lind and Robertson, Blane studied in Edinburgh under Cullen and graduated M.D. in Glasgow in 1778. Meanwhile in 1776 he migrated to London. Through William Hunter’s offices he gained an introduction to society where he met George Rodney (1719-1792), the future admiral in the West Indies campaign between 1780 and 1783. His professional career may be divided into 3 parts: physician to the fleet (1780-1783), physician to St. Thomas’s Hospital, London (1783-95) and town practitioner and Government consultant (1795-1834). In all three phases he clearly saw his statistical approach as a most valuable way for propagating advances in preventive and curative medicine.

Blane entered the naval service in an unusual way in 1779. He accompanied George Rodney as a private physician without any official position, on the flagship of the fleet which was to raise the besieged stronghold of Gibraltar. Having shown courage at the first engagement, Rodney appointed him, in a piece of jobbery, physician to the fleet when he was himself appointed commander of the West Indies stations in 1780. A gifted organiser and administrator, but uninformed about specific naval medical problems, Blane distributed a *Short account of the most effectual means of preserving the health of the seamen* (1780) to all captains, at his expense, the subject-matter of which was mainly drawn from the advice of Lind and Cook. In his new capacity of physician to the fleet he received the monthly returns from all surgeons on the health on their ships; this scheme having been initiated by Rodney for administrative and logistic reasons. These returns allowed Blane, through his close relationship with his commanding officer, to make suggestions for curing and checking the progress of predominant diseases; moreover, “they served also ... as a method of collecting a multitude of well-established facts, tending to ascertain the causes and course of disease”. The same holds for the returns from the hospitals. When expanding his first little treatise he soon realised that even more could be done for the preservation of health and life of the seamen and that it was “a matter not only of humanity and duty, but of interest and policy”.[130]

Not without being aware of several practical obstacles, Blane's programme was explicit: compare averages of great numbers of observations, arrange them in tables for convenience - and to avoid tediousness for the reader.[131] He wrote:

"I conceive to be the only true method of cultivating a practical art ... to collect and compare a great number of facts. A few individual cases are not to be relied on as a foundation of general reasoning, the deductions from them being inconclusive and fallacious, and they are liable to be turned and glossed.... It has been my study to exhibit a rigid transcript of truth and nature, upon a large scale, and to take the average of numberless particular facts..., and I have endeavoured to analyse and collate these facts, by throwing the returns... into the form of Tables, as the most certain and compendious way for finding their general result."[132]

He envisaged it as a preliminary attempt, which might serve as a "ground work". Also Blane indicated that he had continually tried to improve the kind of accounts he wished to receive.[133] A specimen of the forms used is reproduced in *Table 3* of this thesis. He explained that

"If the materials are not sufficiently ample, or if the method should be found faulty and imperfect, led it be remembered, that I had no example to go by in this field of observations. It is to be regretted that the ages have passed without any attempts being made to transmit regular records of this kind to posterity."[134]

Such records, weighed and compared, would enable doctors to describe diseases and to form a proper estimate of the real efficacy of different remedies and modes of treatment".[135]

The first summary of his findings was presented in tow memorials to the Admiralty in October 1781 and in 1782, while Rodney and Blane were on leave in England. Although approved by the Sick and Hurt Board, Blane's recommendations had no practical consequences in general (except in the fleet under his car), until he himself was able to enforce them some fifteen years later when he had become a commissioner of that Board.[136] Indeed, "the peace...of...1783, put an end to all my enquiries, and particularly prevented me from following out some practical researches. Consequently, only part of his original plan was executed and the results compiled in time for his *Observations* (1785). He stressed the incompleteness of this treatise, saying that his obligation to travel, incumbent of his post had deprived him “in a great degree... of the fruits” of his measures by not allowing him to see personally their results, which he published however numerically in terms of mortality, in this book.[137]

b. In a Civilian Hospital

Upon his return to London in 1783, Blane was elected physician to St Thomas’s Hospital, largely through the influences of Rodney and of two fellow Scotsmen. Together with William Black, who had already a name as a vital statistician in 1781, he also became consultant to a short-lived Dispensary for Poor Married Women, founded in 1785.[*][138] He now applied his Navy methods in his civil practice: He was

“impressed with a high opinion of the advantages desirable to the art .... from comparative views ... [and] the little value of single facts for the induction of useful inferences unless related to others presenting themselves in uniform combination.”

Therefore he kept notes of all his cases during the greater part of his time as physician to one of the largest hospitals of the Metropolis, (1783-1785) “and also in my private practice at all times”. He explicitly described this approach as Baconian.[139] In the year of his election to St Thomas’s, some improvements were carried out there in respect to cleanliness and ventilation, and the number of beds was reduced in that hitherto overcrowded hospital. In order to show their effect, Blane compared overall hospital mortalities over the ten years before and after the introduction of the hygienic reforms of 1783; he also recorded the variation of diseases with time, and the different incidences and mortalities of certain diseases (e.g. gout) according to the social class of the patient.[140]

From Blane’s testimony it is quite clear that the undertaking of a nosological listing of the hospital’s cases was his own venture and was by no means a professional duty imposed by the hospital.[141] Before him, there had only existed general mortality tables for the hospital, which stated the number of admissions, discharges and deaths. Until 1764 the printed reports had not even distinguished between the in-patients and the out-patients in the account of the two former. Since the number of deaths applied to the in-patients only, no judgement could be formed on the rate of mortality. Blane knew that the comparative mortality at different

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[*] The charity is not listed in Highmore (1814).

hospitals was "a most fallacious test of the success of practice, unless the nature and intensity of the several diseases are taken into account." But in his opinion, such an objection did not apply in the case of one and the same hospital, administered on the same principles, when observations from different periods were compared.[142]

The practical application of Blane's comparative views in what he called "prophylactic medicine" was "too obvious to require comment". But it was "very desirable that such views should be made available to the purpose of curative....medicine" as well[143] and since continued fever was the principal cause of death, it was the most important to look at it from that point of view. In his opinion, the first point to be ascertained with regard to treatment was to calculate, with some degree of precision, the extent of the powers of nature and to determine what was due to these and what to the agency of medicament. For unless this discrimination was made "we must frequently run the risk of congratulating ourselves on a great *cure*, where there may have only been a happy *escape*."[144]

The conception of fever as a manifestation of the healing endeavour of nature, further elaborated in Blane's *Medical logic* (see below) was typical of the pertinent English literature of the eighteenth century, e.g. in the writings of Huxham and Pringle.[145] Through Boerhaave's prestige, Sydenham's neohippocratic concept of the *vis medicatrix naturae* became the therapeutic basis for many British physicians throughout much of the 18th and early 19th centuries. There were however, certain limitations. As Blane said: "This does not preclude the interposition of art as and auxiliary to the efforts of nature, which are frequently inadequate".[146] This attitude can be also observed among the 18th century British surgeons over the question of amputation (see below).

With a view to resolve the important problem whether recoveries from fever were effected by *virtue* of therapy Blane saw the necessity of "controlled" study: "it would be desirable sometimes to leave nature to her own struggles, as a standard for observation in comparing the result with that which occurs under the use of artificial means". However, there were ethical difficulties: "In the present circumstances of society, practitioners would hardly find it either prudent or warrantable to institute such experiments". But he circumvened them, as John Millar had done, with the help of the 42 cases reported in the Hippocratic corpus. From the 31 fever cases without local affection, fifteen had died under pure expectative treatment. In Blane's opinion, "This record of remote antiquity, while it proves that nearly one half of those who are attacked with some of the most dangerous diseases incident to humanity may recover by the unassisted efforts of nature, furnishes us certainly, at the same time, with a powerful argument in favour of artificial means of relief."[147]

Indeed this ancient mortality far exceeded the one Blane had found in his own hospital as well as his private practices (i.e. 15%) and that presented in other contemporary statements.[148] As mentioned in my introduction, Bisset Hawkins used this example as an illustration for statistics affording the most convincing proofs of the efficacy of medicine in the first English monograph on medical statistics in 1829 (see above, p.??).

However, fairly enough, Blane also mentioned contemporary cases militating against this very assertion: the case of 85 children in an orphanage hospital in Edinburgh who recovered from a fever under expectative treatment indicated that medical intervention was not always requisite. Blane admitted that fevers were so varied in degree and circumstance, and that the powers of nature could operate so diversely that these powers alone could sometimes be sufficient, in which case the use of active remedies might be not only superfluous, but injurious. Furthermore, if an author disproved the efficacy of a medicine in interrupting the course of fever and shortening its duration, he begged the central question,

"... for the point is, not what interrupts the course or shortens the duration, but what mitigates the symptoms, and prevents a fatal termination of fever. If medicine performs this last, it effects all that is required of medicine."[149]

This last crucial passage was dropped in later reprints of Blane's article in the several editions of *Select dissertations* from which Bisset Hawkins probably quoted to prove the curative value of therapy.

Blane by no means claimed any priority for his methodological views when he discussed them, as president, at the Medico-Chirurgical Society of London in 1813; rather he referred to a number of British physicians who had published vital statistics around 1800. He considered the historico-comparative statistics [Note his use of the term "statistics", new in the English language at that time (Cullen 1975).] as his "main position, ... highly useful, and ever indispensable, in eliciting truths applicable to the prevention and cure of diseases", and his hope was that the Society would become a channel through which this programme might be pursued and the results collected, diffused and perpetuated.[150]

It is the more remarkable therefore that I was unable to find any direct reference to Robertson in Blane's publications although he quoted Lind and other naval surgeons freely. They had met at the beginning of Blane's career in Gibraltar, and Robertson, who by then had been twenty years in the Navy, had given him some advice on the management of the military hospital there, which Blane followed.[151] There may have been some personal antagonism between the two men - or just the fact that Blane was reportedly a cool character.[152]

c. General Interest in Statistics

In the third stage of his professional career Blane became the best fulfiller of his own wish. He used "his" method wherever mass observation were available and might be useful to pressure for reforms. The *Medico-chirurgical Transactions* had already published his Facts and observations respecting intermittent fever which had decimated the British expedition to the Continent in 1809 (1812). Later they included his hospital statistics (1813), his magisterial survey of the advances in naval medicine from 1779 to 1814 (1815), his papers in support of vaccination (1819), and on cholera (1820), all abased on statistical comparisons of mortality at different times. These articles were reprinted in the two volumes of Blane's *Select dissertations* which ran through several editions from 1822 until 1833 and were also translated.

Blane's "Statement of the comparative health of the British Navy from the year 1779 to the year 1814 with proposals for its further improvements" was read to the Society on that same 20th June 1815 on which James McGrigor presented his "Medical history of the Peninsular War" to the same audience. It followed the latter in the same volume of the *Transactions*. As in McGrigor's report, information for the last few years was drawn from the official returns, in this case the returns of the Navy. As Blane had been a commissioner to the Navy Sick and Hurt Board from 1795 to 1802 it was not difficult for him to get the very latest records for his purpose. In addition to McGrigor's account, Blane gained a supplementary dimension of time, as proper to this method, by comparing the latest data with those which he had acquired when he had been physician to the fleet in the West Indies and North America from 1780 to 1783.

He also found earlier records on the Seven Years' War and the American War in the House of Commons' Journal Office.[153] Blane's conclusion from his survey was optimistic, even complacent, which was rare in naval medical literature. This was partly warranted by the fall in the rates of sickness and mortality, which Blane demonstrated to have decreased from 1 in 2.4 to 1 in 0.7 and from 1 in 42 to 1 in 143 respectively, from the beginning of the American War to the end of the Napoleonic Wars.[154] In the opinion of one historian,

"Without the reforms in hygiene, ventilation and victualling for which he was partly responsible, [General supply of lemon juice in 1795, free supply of drugs to the naval surgeon in 1796 and 1804, supply of soap (by deducting the cost from the seamen'' pay) in 1796.[155]] the naval record of the Age of Nelson would not have been as impressive as it was. Indeed, according to Blane, if the mortality rate prevailing in 1779 had continued, the whole stock of seamen in this country would have been exhausted long before the defeat of Napoleon."[156]

However, it must be stated that prior to 1810 there were no means of collecting data on deaths occurring on board ship, nor were there precise indications of the actual strength of men.[157] [The regulations and instructions regarding this period were officially printed in 1808 (see below, p.??)] For reasons of comparability, Blane's calculations were therefore based on the precise numbers of seamen who were sent sick to, and died, in hospital in Britain and abroad, and the gross number of seamen voted for each year by Parliament. Modern examination of these morbidity and mortality statistics by Lewis (1960) and Greenwood (1942) show that Blane was justified by and large in his claim that two ships of war would now do what three were required to do before.[158]

Blane himself was interested in many aspects of statistics. I have mentioned medical and vital statistics. But there were, for instance Patrick Colquhoun's (1745-1820) *Treatise on the wealth, power, and resources of the British Empire...*, actuarial compilations, Malthus's (1766-1834) writings and the first British census.[159] He also used, for comparison between military and civil navigation, the medical records of the East India Company. This gave him the possibility of making enquiries on a cholera-epidemic in India in 1819. He used their returns for an epidemiological account that he delivered to the Medico-Chirurgical Society in 1820, giving the authors full credit for their contributions, as he would always do.[160]

d. A Brilliant Career

As Lettsom's, Blane's originality lies not on the medico-scientific side of his work (as he recognised himself[161]), but rather in his analytico-administrative innovations - and in "his power of using cajolery and flattery to get his own way with the powers that be". Through his excellent private practice he had the right connections. He was popular with one of Britain's most humane, influential and intelligent admirals, Lord Rodney, and with other rulers of the King's Navy.[162] This together with his renunciation of an important hospital position to join the Sick and Hurt Board of the Navy in 1795, and his well documented *Observations*, gave him an early prestige and influence in high places. Such prestige was important, in those days when everything was a question of individual patronage, for the enforcement of reforms which had been suggested in vain by more humble seniors or contemporaries like Lind, Robertson, Gillespie or Trotter.[163] All these naval doctors had used statistical presentations to some extent, in their books and pamphlets, yet because of his position and his skill Blane's were more complete.

Through his career he also showed the utility of arithmetic observation outside the purely naval context. He was one of those who used and propagated this method in nosography, therapeutics and in social and preventive medicine. In 1829 Blane institutionalized his programme by creating two Prize Medals to be awarded every two years to the author of the best naval medical journal kept when afloat.[164] Two naval surgeons based their plea for immediate amputations on their statistics of the naval battle before Algiers in 1817 (see below), and Blane made it clear, on the occasion of the first award of his prize, that those were the kind of useful reports he had thought of.[165] Some tabular reports of the occurrence and outcome of all diseases on board ships during a given period, arranged according to diagnoses, had indeed been published during the Napoleonic Wars and after peace.[166]

Statistics served Blane also in politics, anxious as he was to improve the professional attainments and positions of naval surgeons. [An initial reform in this sense was in fact accepted in 1805.[167]] After his retirement from the Board in 1802 he was still consulted by the British Admiralty, the Russian Fleet, and the Turkish Company, later even by the British Army on behalf of public health questions. He became something of a recognised authority on such matters for his later statistical writings came before a more general public. In consequence he was the first naval doctor to receive a baronetcy in 1812.[168]

In summary, Blane's was a fine career, for the success of which statistics were not irrelevant, and which, in turn was not irrelevant for the success of statistics in naval medicine, (and perhaps also in other professional sections). I shall attempt to illustrate this in the conclusion of this chapter.

2. Carmichael Smyth

When Blane was commissioner of the Sick and Hurt Board a new method was introduced to disinfect the ships by nitrous fumigation. By this method vitriol was poured over powdered nitre, and warmed.This obviated the dangerous practice of lighting fires below deck. Used in other current methods, such as burning gunpowder, tar, or tobacco.[169] Surely the way in which this method was presented by its inventor, Dr Carmichael Smyth, must also have appealed to Blane, for it was entirely based on comparative statistics from ships, hospitals and prisons. In 1780 already Smyth, and Edinburgh trained Scot, had written on fever among Spanish prisoners of war at Winchester, showing that the proportion of sick to those in custody, and the proportion of fatalities among the sick, had decreased after the introduction of his nitrous fumigation. In 1795 the Board instigated a trial by its own appointed expert, who wrote a very favourable official report. Smyth reprinted it in his *Account* (1796). His comments culminated in the statement that, to bring the success

"home to the understanding and conviction of all mankind, it is only necessary to look with attention on the annexed Hospital return; for by comparing the state of health of the ship's company, with the progress and effects of the contagion, before and after the experiment was begun, a clear and decided judgement ma be formed of its effects, even by the most ignorant."[170]

In 1799 Smyth issued a book on the subject, about which he was also involved in a priority quarrel.[171] In this treatise, the Board's report was reprinted, together with Smyth's original report on Winchester and a number of extracts from surgeon's journal. The best were three statistically based letters by McGrigor (1799) and some tabular accounts similar to Smyth's own by two naval surgeons. Smyth put all the tables he possessed together in one great unfolding table annexed at the end of his volume. They afforded "perhaps, the most complete Evidence, of a Medical Fact, that was ever presented to the Public".[172]

*During the preceding two months thirty patients had been seized (22 recovered, 8 died) - during the two following months two had been seized (1 recovered, 1 died).
Finally, Smyth received £5000 from Parliament.

Clearly statistical returns had become a "must" for the advancement in the world. This was even recognised by a naval surgeon like Thomas Trotter, who otherwise did not like Blane, so strongly associated with statistics.

3. Thomas Trotter and Leonard Gillespie

Thomas Trotter (1760-1832),[173] yet another Scotsman, a younger, perhaps more original and surely more empirical and less theory-involved reformer of British naval medicine than Blane, hardly gained any public recognition for his highly meritorious work; for instance, he too deservers much credit for the practical introduction of lemon juice against scurvy in 1795.[174] He had entered the Navy in 1779, the same year as Blane, but in the lowest rank of surgeon's mate. His first publication, the *Observations on the scurvy*, appeared in 1786, when he was temporarily retired from service afloat and was studying at Edinburgh under Cullen.[175] Neither this nor a second edition (1792) included numerical statements, yet it set out to refute one by one, on a basis of general observations, the three substances on which the Navy Sick and Hurt Board still relied for the prevention of scurvy, i.e. elixir of vitriol, sauerkraut, and essence of malt.[176] In the second edition, that included many references to Blane's observations,Trotter said that "the necessity of recording facts must be obvious to everyone" and conceded that the first edition was written at a period of life "when few people think of giving their opinion on practical subjects of medicine". His major experience with the scurvy came from a voyage on board a slave ship to America, when he had seen "about three hundred" slaves tainted with scurvy while on a diet of unfermented farinacea. This fact was hardly reconcilable with Cullen's widely accepted vitalistic patho-physiology of scurvy, and had incited him to publish his tract.[177]

Trotter resumed active service in 1789. He was appointed second physician to John Lind (son) at Haslar Hospital in 1793, and in 1794 physician to Lord Howe's (1726-1799) Channel fleet, a position Blane had held in the West Indies fleet fifteen years earlier. As an old sea-dog Trotter did not much like the courtly Blane. Nevertheless he confessed that, upon this new appointment, he had "endeavoured to imitate Dr Blane, in calling upon the surgeons for occasional remarks; and much valuable information has been received from that quarter, which I have thrown into the form of notes".[178] Indeed his subsequent history of the health of this fleet was resplendent with such accounts and tables made of the monthly returns of the ship-surgeons.[179] He did for Howe's Channel fleet what Blane did for Rodney.

A naval surgeon directly patronized by Blane was Leonard Gillespie (1758-1842),[180] an Irishman of Scottish stock who had served a few months under him during the West Indian campaign in 1780. Gillespie compared his own official records from there with careful notes he continued to take when he became later associated with the New York Naval Hospital. He developed a very successful cure for ulcers instead of amputation, published in the *London Medical Journal* in 1785 and 1787, and which Blane recommended in turn.[181] After 1783 Gillespie studied at length in Paris and briefly in Edinburgh. He received (with Blane's testimony) further naval appointments, (for a time under trotter) and was then in charge of a Navy hospital at Martinique from 1798 till 1803. He finished his career with the highest rank of physician to Nelson's Mediterranean fleet from 1804 to August 1805. It was to Blane he sent the manuscript of his *Observations on the diseases... on the Leeward Island Station...* from Martinique in 1799, asking him to arrange for publication, which Blane did in 1800. Its material was drawn from Gillespie's very accurate diaries "and included some impressive statistics" - although not given in tabular form --, and illustrative case histories.[182]

A body of knowledge was thus elaborated by the turn of the century on a statistical basis which became also assimilated into new textbooks; in turn the practical surgeons started to rely on them.

4. A New Textbook and New Regulations for Naval Surgeons

The most widely read new and specialised textbook was William Turnbull's *Naval Surgeon* (1806),[183] which appeared in 1806 just after the reorganisation of the Sick and Hurt Board in 1804/1805 and after a reform to upgrade the status of the naval surgeons had taken place.[184] It was a compilation "particularly indebted to the works of Lind, Blane and Trotter". Other authors referred to were Millar, Robertson, Clark and Gillespie (whom I have mentioned as early record-keepers and arithmetic observationists". The *Naval Surgeon* also recommended the arrangements o fall cases in the journals at sea as "of the highest importance" not least to the surgeon himself. Indeed, it itself included tables by Lind, Trotter, Robertson, Blane and McGrigor as illustrations to various chapters.[185]

Turnbull was up-to-date since he printed a specimen of the new form for the medical and surgical journal as it had just been decided in January 1806 in the new *Regulations and instructions relating to His Majesty's Service at sea.*[186] They basically replaced those of 1731, but were officially printed in 1808 only. They were adapted to the experiences of the men who had lately directed medical services in the fleet: Blane, Trotter, Robertson and Gillespie. The reporting system was accordingly completely changed: the surgeon had to keep a daily journal, which was to be sent to the Sick and Hurt Board once a year, and also to be abstracted. All cases without exception had to be recorded and the forms required the following information:

Men's names, age quality, time when and where taken ill	history, symptoms, treatment, and daily progress of the disease or injury	when discharged to duty, died or sent to hospital

The abstract, being a summary, arranged the diseases in nine categories, the incidence and outcome of which were to be listed numerically and the added up according to six columns "Discharged to Duty; to Hospital; Died on Board; Invalided; Harbour duty; unserviceable" (Form 38, Appendix). The form for the weekly (when in Britain) or monthly returns (when abroad) was similar to the abstract form, but had two more disease categories. [Continuous fevers, fluxes, scurvy, ulcers, wounds and accidents, rheumatism, pulmonary inflammation, intermittent fevers, other complaints (phthisis and venereal diseases).] This design had been used in a more elaborate form by Robertson since the 1770s and subsequently by Blane and trotter. John Clark had recommended it in 1780. Of particular note was the regulation that each man sent to a hospital or a hospital ship had to be accompanied by a written account, stating the whole history of his disease including its treatment (and also specifying any reason "for suspecting any... complaints to be feigned").[187] Compared with the spirit of those of 1731 (see p.??) the new regulations marked an important shift from an interest in individual cases to the arithmetic average of cases for application both to nosography and to therapy; the quantitative element became as important as the qualitative.

E. CONCLUSION

Judging from the constant opposition to it by most authors I have discussed, the preference for the single, extraordinary case, characteristic of one type of medical publication since antiquity, was still widespread in the 18th century. But there was also a slow rise of epidemiological and preventive thinking in naval medicine which set the sum of the individuals above the single case. This can be seen in the development of the work of Lind himself, who pioneered this as well as clinical aspects of practical medicine. In 1753 his preference for lemon juice over other anti-scorbutics was still based on a trial with two patients whom he had compared with five other groups of two patients each. In 1763 and 1768 (and in the following editions of the respective books) he explained the necessity of large series of observations before a therapy could be approved of or rejected and reported himself, though inconsistently, hundreds of cases from his hospital in confirmation of his methods. His quantitative bent was also obvious in his statement, that one therapy could only be effective in a certain proportion of cases of the same disease. In accordance with this view he also designed the comparative, clinical trial with numerical expression of the results, which was taken up by Robertson, Blane and John Clark.

Robertson admittedly adopted Lind's precepts applying them with the use of arithmetics, be he a surgeon afloat, a country practitioner or the head of a naval hospital, as had Blane, who elicited and then used statistical returns throughout his long career. Both wrote programmatically thereon and they may be termed "arithmetic observationisst" with the same right as Millar, Black or John Clark. Other naval surgeons more or less wholeheartedly started emulating their example of the rather troublesome record-taking and analyzing. Blane's and Robertson's insistence on numerical analysis was doubtlessly reflected in the equally insistent new official regulations of 1806 (1808), with Blane having just left the Navy Medical Board, and Robertson being senior physician of the Navy. Comparison of the ensuing returns with his own records enabled Blane to attain his view of the health of the Navy from 1779 to 1814.

In fact I have hardly, in this chapter, pushed my analysis further than 1815, for I shall refer to alter publications of naval doctors in chapters five and seven of this thesis. If one can trust Blane's figures (and it seems that one can) I tend to believe that the introduction of a simple arithmetical analysis was not irrelevant for the improved health of the seamen in 1814 as compared in 1780. This is surely true for the prevention of fevers, but appears also to be so for scurvy, where numerically stated facts finally convinced the authorities of the superior value of fresh fruit juice. And, it may also hold true for the therapy of fever. With our hindsight, Lind's, Robertson's and Blane's recommendations appear doubtlessly saner than inconsiderate bleeding, against the practice of which in the Army Millar and Rowley drew rebelled. But it is difficult to evaluate to what extent and for how long their precepts were actually adopted by the mass of naval surgeons outside their immediate sphere of influence.

In conclusion, the leading and influential British naval doctors of the latter 18th and early 19th century recognised the value of, and used, note-taking and comparative arithmetical analysis of large series of observations, as the only means for advancing both the knowledge of diseases and the evaluation of therapy. And, as I shall show in the following chapters, doctors in their orbit used the method - if only to get on in their careers.

F. REFERENCES TO CHAPTER FOUR

[1] Ballingall 1852, pp.589-601
[2] Lind 1757, p.x; Dudley 1953, pp.370-372
[3] Dudley 1762, p.x-xi
[4] Allison 1943, pp.25-28
[5] Robertson 1783, p.ix
[6] Quoted by Allison 1943, pp.119-120
[7] Lind 1757, pp.vii-ix; Cleghorn 1751, p.vi; L.&C., Vol.3, pp.11-12
[8] Roddis 1950
[9] L.&C., Vol.3,pp.293-296
[10] Lind 1757, p.vii
[11] Locke 1894, Vol.1, p.14; Lind 1757, pp.viii, 159
[12] Lind 1757, pp.xii, 147
[13] L.&C., Vol.3, p.299
[14] Lind 1757, pp.65-68
[15] ibid., pp.ix, 60-61,65-68,94-103,110-127,151-152; Rolleston 1915, p.14
[16] Lind 1757, pp.55,131-135,444; L.&C., Vol.3, pp.297-298
[17] Lind 1757, pp.60-61,145,154
[18] ibid., pp.50,149
[19] A.J. Lorenz, quoted by L.C., Vol.3, p.300
[20] Lind 1757, pp.149-153
[21] Schadewaldt 1955, pp.2518-2519; L.&C., Vol.3, p.300;Ackerknecht 1973, p.91; Seydel 1976, p.67
[22] Hughes 1975; Wyatt 1976
[23] Lind 1757, pp.50-51,152
[24] ibid., pp.152-159
[25] L.&C., Vol.3, pp.42,218,303
[26] Hughes 1975; Watt 1978
[27] L.&C., Vol.3, pp.298-327
[28] Hirsch quoted by :&C., Vol.3, p.298; see also Herbert Spencer quoted Roddis 1950, pp.72-73; Dudley 1953, p.378 Ackerknecht ???
[29] Rolleston 1915, p.9
[30] Meiklejohn 1954
[31] L.&C., Vol.3, p.303
[32] Smith 1847

[33] Lind 1757, pp.249-260
[34] Macbride 1764, pp.vii-xi, 171
[35] Compare Macbride, ibid., pp.183,189-190 with Lind 1757, p.253
[36] Macbride 1764, pp.84,183,190
[37] Lind 1757, pp.184-185,187-188,192-199
[38] Macbride 1764, pp.184-185,187-188,192-199
[39] ibid., pp.174-175
[40] Macbride 1767 A, pp.134-134,163-165
[41] Hawkesworth 1785, Vol.1, p.171, Vol.2, p.3
[42] L.&C., Vol.3, p.308
[43] Macbride 1767 B, p.30
[44] Macbride 1772, pp.641-651; L.&C., Vol.3, p.308
[45] Macbride 1767 B, pp.38,40,45,49-50
[46] ibid., pp.54-60
[47] Meiklejohn et al. Quoted in L.&C., Vol.3, p.302; Hughes 1975
[48] L.&C., Vol.3, p.309
[49] Singer 1949-1950, pp.152,240-242
[50] Clark 1773, pp.181-182
[51] Macbride 1772, pp.653-654; L.&C., Vol.3, p.308
[52] ibid., pp.651-652
[53] ibid., p.658
[54] Quoted from Beaglehole 1967-1969, Vol.1, p.632; also in Macbride 1772, pp.659-660
[55] Included in Macbride 1787, p.xiv
[56] Clark 1773, pp.283-300
[57] Lind 1762, p.141
[58] ibid. Pp.i-ii
[59] ibid., pp.512-515
[60] Stewart 1953, p.361
[61] Lind 1772, pp.iv-v
[62] Lind 1772, pp.iv-v ibid., pp.471-472; Meiklejohn 1954
[63] Lind, ibid., pp.504,506,531-532,536
[64] ibid., p.180
[65] ibid., p.vi; Beaglehole 1967-1969, Vol.1, pp.632,635
[66] Lind 1772, pp.537-540
[67] Hughes 1975
[68] Lind 1772, pp.538
[69] ibid., pp.v-vi
[70] Quoted from L.&C., Vol.3, p.314
[71] Pringle 1776, pp.13-16; Cook reprinted ibid., pp.41,44
[72] ibid., 20-22; L.&C., Vol.3, p.309
[73] Lind 1772, p.540
[74] Manning 1780, Vol.1, pp.405-406
[75] Macbride 1787, pp.504-509
[76] L.&C., Vol.3, pp.123, 317; Robertson quoted ibid., p.127
[77] Robertson 1804, Vol.1, pp.xxviii, 20
[78] Robertson 1777, pp.120,130-131
[79] L.&C., Vol.3, pp.129,317
[80] Blane 1785, pp.329-344
[81] L.&., Vol.3, pp. 129,317
[82] Blane 1785, pp.54-56,56-57,285,287,295,463-464; L.&C:, Vol.3, pp.131-132
[83] Thomson 1790, p.xviii-xx, xxii, 8-9,128,154,192
[84] Robertson 1783, p.vii
[85] Renwick 1792, p.14-15
[86] L.&C., Vol.3., pp. 320-326
[87] Rolleston 1919; Porter 1963
[88] Trotter 1797, Vol.1, pp.133-134
[89] L.&C., Vol. 3, p.326
[90] Lind 1763, pp.1-2
[91] Lind 1768, p.322
[92] Quoted by L.&C., Vol.3, p.335

[93] Lind 1763, pp.1-2, 4-5,7-10,26,34,40-41,58, Postscript
[94] ibid., p.73
[95] ibid., pp.71-72,79
[96] ibid., pp.70,82
[97] ibid., pp.71-72
[98] L.&C., Vol.3, p.336
[99] Lind 1777, pp.291,310,316-317,329-336,347
[100]Lind 1768, pp. 291, 313-315
[101] ibid., pp.316-317
[102] ibid., pp.275-280
[103] See Lind 1777, pp.262-265,291,313-315,342-343; 1808, pp.271-272,333-334,353-354
[104] Robertson 1777, p.xi
[105] L.&C., Vol.3, pp.20,44-50,89,199-200,334,339-340,356
[106] ibid., p.126-130
[107] Robertson 1777, pp.vii-viii, 22,120
[108] ibid., p.ix; McConaghey 1969
[109] Robertson 1777, pp.130-131; McConaghey 1969, p.283
[110] L.&C., Vol.3, p.46
[111] Robertson 1777, pp.130,140-144,196
[112] ibid., p.131,189
[113] L.&C., Vol.3, p.101-103
[114] Hume 1778, p.p.242-243; tables partly reprinted in L.&C., Vol.3, p.107
[115] Robertson 1783, pp.vi-vii
[116] ibid., pp.139-146,230-236,302-309
[117] Robertson 1777, pp.194-195
[118] Robertson 1783, pp.178,238,145 (Table VI), 227 (Table I)
[119] ibid., pp.228-229 (Tables II and III)
[120] ibid., p.237
[121] ibid., pp.300-301 (Tables I and II)
[122] ibid., p.312
[123] ibid., pp.312-316
[124] Robertson 1789, pp.xviii, 471,473,485
[125] Robertson 1790, p.1-3
[126] L.&C., Vol.3, p.261
[127] Robertson 1790, p.5
[128] Rolleston 1916
[129] Murrell 1949
[130] Blane 1785, pp.vi-viii, 85,206
[131] ibid., pp.ix-xi, 85
[132] ibid., p.xii
[133] ibid., pp.viii, xii, 20,21,73
[134] ibid., pp.ix, xii-xiiii
[135] ibid., pp.329-344; Allison 1943, pp.xv-xvi
[136] Rolleston 1916; L.&C., Vol.3, p.46
[137] Blane 1785, p.xi
[138] Spencer 1927, p.181; Glass 1973
[139] Blane 1813 A, pp.90-91
[140] ibid., pp.117-119
[141] Blane 1833, Vol.1, pp.149,154
[142] ibid., pp.117-119
[143] Blane 1813 A, p.126
[144] ibid., p.127
[145] Neuburger 1943, pp.82-83
[146] Blane 1813 A, p.127
[147] ibid.,pp.127-129
[148] ibid., pp. 133-137(Tables)
[149] ibid., p.130
[150] ibid., pp.131-132
[151] Robertson 1789, p.266
[152] Garrison and Morton, p.433, No. 3715

[153] Blane 1815, pp.571-573
[154] ibid., pp.539-540 (Table I)
[155] Rolleston 1916, pp.4-5; Schadewaldt 1955, p.2519(??
[156] Lloyd 1965, p.133
[157] Blane 1815,p.491
[158] ibid., pp.537;Lloyd 1965, p.134
[159]Blane 1815 B; 1815, pp.492,519
[160] ibid., p.534-536;1820
[161] Blane 1785, p.viii
[162] Rolleston 1916, p.3; Dudley 1953, pp.378-379
[163] Lloyd 1965, p.132; L.&C., Vol.3, p.38-48,129
[164] For the regulations, see Blane 1833, Vol.2, pp.257-262; see also L.&C., Vol.4, p.49-60336
[165] Blane 1833, Vol.2, pp.270-271
[166] See e.g. Med.-Chir.J.Rev. 5; 93-106 (1808(; Edinb.med.surg.J. 4; 450-456, (1808); 12, 2nd ed., 123-125, (1817); 14; 168-190, (1818)
[167] L.&C., Vol.3, p.33
[168] Rolleston 1816, pp.6,8
[169] L.&C., Vol.3, p.76
[170] Smyth 1796, pp.52-53
[171] D.N.B. Vol. 18, p.592-593
[172] Smyth 1799, pp.123,152-165,177, Annexe
[173] Rolleston 1919 (??); Porter 1963
[174] L.&C., Vol.3, p.321-326; Porter 1963
[175] Trotter 1792, O.xix-xxii
[176] ibid., pp.181-188;1786, pp.90-93
[177] Trotter 1792, pp.xx,xxiv, 69,79,105,114
[178] Trotter 1797, Vol.1, p.2
[179] ibid., pp.65,70,72,86-93,110,133,141
[180] Keevil 1954
[181] ibid., p.304; Gillespie 1800, p.237
[182] See e.g. Gillespie 1800, pp.100,161,223; Keevil 1954, pp.304,320
[183] L.&C., Vol.3, p.34-35
[184] ibid., pp.5-7, 32-35
[185] Turnbull 1806, pp.xxxviii-xlv, 149-150,193-194,232
[186] ibid., xlii-xliii; Admiralty of Great Britain 1808, p.iii, Appendix No.38
[187] Admiralty of Great Britain 1808, pp.279,281,282, Appendices No.38 and No. 39

CHAPTER FIVE: ARMY MEDICINE

A. INTRODUCTION

Ballingall's bibliography (see above and *Table 2*) lists 62 medical officers of the British Army and fourteen of the Honourable East India Company's Service who wrote papers and books on diseases and accidents of soldiers between 1750 and 1830 [The following are not considered in this list: purely organisational and programmatic writings, and literary, descriptive accounts of campaigns.]. As in the chapter on naval medicine, it was found convenient to subdivide this long time span into three periods, viz. Period I from 1750 to around 1790, (the era of the pioneers), period II from 1793-1815 (the time of broader application and further development of their examples during the French Revolutionary and Napoleonic Wars) and period III, from 1816 to 1830, (the period of the continuance of those applications). This division appears historically defensible for there was only one author out of 46 who had to be listed in both periods I and II, and only four out of 48 in periods II and III. *Table 2* also illustrates that the bulk of the work concerned epidemic diseases (i.e. fevers) and hygiene (36 authors). Ten authors wrote exclusively on wounds and surgery, eight published only on the ophthalmia which ravaged the Army after the Egyptian campaign of 1801 and two dealt only with venereal disease (not listed).

As in the foregoing chapter I shall first analyze the works of the pioneer authors on military medicine up to 1815, concentrating on the questions of fever and hygiene. Then I shall discuss the treatment of fever from 1750 to 1830 more broadly. There will be a summary on ophthalmia, which has already been analyzed to some extent,[1] and remarks on venereal disease. The authors studied include all the leading figures, but my analysis does not rely uniquely on Ballingall's list. John Hume, John Millar, John Marshall and Theodore Gordon for instance are not mentioned in this bibliography.

B. THE SETTING OF THE STAGE

1. GEORGE CLEGHORN AND WILLIAM HILLARY

The first book in our period was George Cleghorn's *Observations on the epidemical diseases in Minorca* (1751). Cleghorn (1716-1789), a Scot, was trained in Edinburgh. He was a favourite pupil of Alexander Monro *primus*, even living in his house.[2] He entered the Army (by buying a commission of a regimental surgeon) in 1733 and stayed in Minorca from 1736 to 1749. There he „determined to observe and record with the utmost *Care* and *Impartiality* whatever should appear conclusive to a thorough knowledge of [the island's] Diseases and their Cure...". With this view, in 1743 he began to keep a diary of the weather, to note the course of the seasons and to describe the diseases they produced; he continued it „with no small labour and assiduity" until 1749. Back in Britain he analysed his observations in order to make some general conclusions „from a vast multitude of cases".[3]

That Cleghorn's book was considered important by his contemporaries is indicated by its four English editions (the fourth appearing in 1797), an American edition (1809) and a German translation (1776). It is also important for our present concern, for it applied one method of the Leyden-Edinburgh schools - i.e. the neo-Hippocratic observations of clinical, pathological and climatic appearances and their recording - to a completely new climatic environment. And, since Cleghorn's observations *were* careful and impartial, the result was a new and original outlook, not relying on mere authority.

Tertian fevers [which he defined with all its subspecies in a traditional way[4]] were most frequently encountered disease in Minorca. Cleghorn found it still badly understood by modern writers; he remarked that

„although the *Greeks* and *Arabians* have treated them at great length.... we do not find them described as they really are, but as they would be if Galen's Theory of the Four Humours was well grounded.... They ascribe symptoms to the Fever, from a preconceived Hypothesis, which seldom or never accompany it in reality."[5]

His opinion of contemporary treatment of this fever was similar. Prejudices which he had „imbibed early from some of the most approved authors" made him use Peruvian bark during his first six years in Minorca too diffidently.

For instance when he had observed that these fevers frequently relapsed, he had suspected that this might be due to too early use of the bark. And

„as I observed, that the greatest Number of Tertians went away of their own accord in a Fortnight's time, I thought it would be more advantageous to the patient to suffer a few Paroxysms... than to hazard a return by having it prematurely suppressed; But afterwards [when he had started recording the cases], by comparing a number of cases which had terminated of their own Accord, with others wherein the bark had been given, I evidently saw that those were most liable to a Relapse whose Strength had been most impaired by the primary fever, whether they had been cured by Art or Nature; so that a Delay in giving the Bark, seems frequently to have produced the Effects ascribed to its having been used too early."[6]

Thus he resolved to give it, during these last years, if the fever did not terminate spontaneously after a maximum of ten days defying the plausible theories of some authors, and the positive assertions of others, which were delivered „in so dogmatical a Manner, as if they were wholly the Results of Careful Observation".[7]

Cleghorn's approach corresponded to a retrospective „controlled"study. Truly, it lacked the numerical presentation of the results but the way to that appears not difficult if one considers how Cleghorn presented his meteorological data in the same book: he gave monthly readings, recording the ranges of temperature (coldest and hottest day), and also the medium temperatures, calculated and expressed in fractions. Finally he made up these three sets of figures of 61 months in a „thermometrical table", distributing them according to the years 1744-1749.[8] [There were, of course also figures on the rainfall.] Clearly this kind of analytical work implied an appreciation of the usefulness of simple arithmetics for extracting maximum meaning from crude data.

What Cleghorn did in Minorca William Hillary (1699?-1763) undertook in England and especially in Barbados. His career was somewhat similar to that of Francis Clifton (see above).[9] He had been a pupil of Boerhaave at Leyden (where he had graduated in 1722), and he, too went to the West Indies during his career (1752-1758). He published two series of systematic observations and measurements of the weather as related to prevalent diseases, namely one on Britain in 1735 and the other on Barbados in 1759. [It reached a second English edition in 1766 and an American on in 1811.]

Concerning the therapy of fever, Hillary was conservative, recommending bleeding both for slow nervous fever (typhus) and for yellow fever; this was to be replaced by purging only if the doctor intervened at a later stage in the disease. He claimed he had been so successful in treating yellow fever as to have lost only two patients in eight years. (And even these fatalities were explained away).[10]

In terms of setting the stage for a numerical method in nosography and therapy, the systematic recordings of weather measurements were probably not irrelevant. They were an early for of quantification in medicine, of tabular presentation, of calculation of averages, and of a type of organisational discipline such as was later imposed by the military regulations. I found such recordings with many of the writers mentioned in this thesis, whether they were in the Navy (Robertson, Clark), the Army (Cleghorn, Pringle, Rollo, McGrigor) or in private practice (Alexander Monro, Hillary). But the association is not necessarily a strong one, for other authors, too, followed this track.[11]

For Hillary this approach fitted in with his plea for rational empiricism, which he made in his separate methodological *Inquiry* (1761) that I have already mentioned in chapter two (p.??). An historical survey showed him that the method of advance in medicine could only consist of accurate clinical observations and judicious experiments *assisted* by „just“inductive reasoning; taken alone neither would lead to lasting improvements. The evidence of history led him to discard Aristotelian philosophy and Galenic rational theories. Instead he went from Hippocrates via „the great Lord Verulan“ and Sydenham to his master Boerhaave.[12] He recommended the study of geometry and arithmetic because even Hippocrates had „reasoned *geometrically* himself, or in a geometrical manner from observations and certain *data*, tho' he might neither make use of geometrical lined or numerical figures“. Would not such a use of figures be necessary in order to avoid basing a therapy on only „two or three extraordinary cases“and creating „mischievous fashions“? Similarly the physician might realize (as Lind had done) that a medicine would be effective in a proportion of cases only.[13]

Unfortunately Hillary died in 1763 so that we cannot find a development of these topics in any later works of his pen. But it is clear that both he and Cleghorn were important stage builders for a numerical approach to medical problems. They overcame traditional prejudices (even those of their own masters) both in theory and partially in practice. Cleghorn's civil career was highly successful. Back in Ireland he opened the country's first anatomical school and became the first professor of anatomy at the University of Dublin [He was also a Fellow of the Société Royale de Médecine in Paris. At his death Lettsom, who had corresponded with him, wrote a highly appreciative memoir on Cleghorn.[14]]

There he was one of the protectors of David Macbride, whom we have met in the foregoing chapter in relation to the scurvy and who, with his chemical experiments, represented another feature of the Leyden-Edinburgh school.

2. John Pringle

In vitro experiments were also one important feature of the scientific work of another Scot, (Sir) John Pringle (1707-1782), who had studied at Leyden.[15] He, too, actively used the opportunities provided by being an Army doctor for research, but in a slightly different way from his contemporary Cleghorn. Pringle's *Observations on the disease of the army in camp and garrison* (1752) constituted the first specific treatise embracing the whole of military medicine.[There were two earlier Italian works by Orazio Monti (1627) and Antonio Porzio (1635).] Its success shows its relevance: seven English editions appeared during Pringle's lifetime and the last was reprinted as late as 1810 and 1812 in England and America respectively. The work was quickly translated, into German (1754), French (1755), Italian (1757) and Dutch (1763). These translations were also reedited.[One further German edition (1772), three French (1771,1793,1795), two Italian ones (1762,1781), and one Dutch (1785-8).]

Because of this book Pringle is commonly called the „father" or the „founder" of modern military medicine and hygiene.[16] For his earlier *Observations on the.... hospital and jail fevers* (1750) he must also be considered one of the originators of a vast movement of hospital and prison reform, to which men like John Huxham (see p.??) and John Howard (1726-1790)[17] contributed in Britain, which gradually spread to the continent of Europe and to America.[18] Pringle, the youngest son of a Scottish baronet, also was a Boerhaave man. He graduated at Leyden in 1730.[19] I shall now analyse his method of research [As for his motivation, it was profoundly humanitarian. During his time as Army physician in the War of the Austrian Succession (1742-1748) he was the first to urge that military hospitals be considered as sanctuaries during combat operations, an „agreement... strictly observed on both sides all that campaign; and tho' it had been broke[n] through since, yet we may hope that in a future war, the contending parties will make it a precedent."[20]] which became exemplary and characteristic for Army doctors throughout the 18th century.

Pringle's *Observations* of 1752, as did Cleghorn's (1751) and Lind's *Treatise* (1753), featured personal observation, the taking of detailed notes and numerical comparisons. He used as the basis for his study the material he had collected when he had been a physician in the British military hospital in Flanders, from 1742 to 1748. In the preface to the first edition we read:

„Upon my first being employed in the army, I soon perceived what little assistance I could expect from books; and therefore I began to note down such observations as occurred, in hopes of finding them afterwards useful in practice. And having continued this method to the end of the war, I have since put those materials into order, and with as much clearness and conciseness as I could, have endeavoured ... to supply, in some measure, what I thought so much wanting in this branch of medicine.[21] [Similarly John Ranby (1703-1773) had used a much smaller and more scattered material collected in Flanders for his *Method of treating gun-shot wounds* (1744,1760). One of his chief motives for writing this booklet had been „that of inciting others, of more considerable abilities to give a detail of their.... successful practice... as there are amongst us [many] who have made several campaigns, and kept, no doubt, exact Journals...'tis a Justice, in my opinion, which the owe to the world".[22]]

Pringle's „clearness and conciseness"included excerpts from his medical (epidemiological) journal, and a classification of diseases according to seasons and to causes. The most surprising among the causes were actually „those very means which are intended for... health and preservation: I would say the hospitals, on account of the bad air and other inconveniences attending them".[23] [This led Pringle to recommend small regimental infirmaries instead of crowded general hospital.] Both the first part of Pringle's book (journals from 1742 to 1748) and the second part (classification and prevention of diseases) abounded with numerical statements taken from sick-lists. Pringle gave absolute figures and also the establishments of the regiments and of the military formations, so that proportions could be calculated, - albeit with some difficulty. Pringle actually calculated some himself.[24] He also gave a numerical nosological list for a military hospital for a given time, comprising seven categories of diagnoses. [1) pleurisies and peri-pneumonias; 2) rheumatic pains with various degrees of fever; 3) inflammatory fevers; 4) intermittent fevers; 5) hard coughs without fever; 6) 'old' coughs and consumption; 7) others[25]]

His recommendations on the importance of the season and the weather for the prevention of disease were entirely founded on a numerical evaluation of the proportional incidence of diseases during the eight campaigns in which he had assisted.

„I conclude... with comparing the numbers of the sick at different seasons, in order that the Commander may know, nearly, what force he can, at any time, rely upon for service; the effects of short or long campaigns, upon the health; the difference between taking the field early, and going late into winter quarters; with other calculations, founded upon such materials as were furnished by the late war.“[26]

Pringle did not compile any concise tables, his figures instead being scattered throughout the text. But their comparison allowed him to state that an early return to winter-quarters was more important than a late beginning of a campaign in the spring; thus, if a long campaign was foreseen, it was better to start it in April than in May. Since in winter chronic diseases were most common, winter campaigns were not as dangerous as commonly believed, so long as troops were well provided for by the victuallers. Moreover, those troops would be best seasoned for a second campaign, whose constitutions were least weakened by the first. However, one had always to remember and consider in these calculations the errors due to the filling up of regiments with new recruits.[27] Pringle was also aware that

„the *data* are, perhaps, too few to deduce certain [i.e. sure] consequences from; but as I have not found any other I could depend on, I was obliged to use these only; which at least will serve for a specimen of what may be done in this way, upon farther experience.“[28]

Those parts of Pringle's work which dealt with preventive medicine exhibited thus a modest but important step towards the quantification of „experience“; but this cannot be said of that part where he described his curative practice. Whereas the former parts had been written for officers as well as physicians, the latter was written for physicians only, and he relied on rather general statement such as „after repeated trials, I found...“ and „many have recovered....“; he also relied on single cases to draw general conclusions and on recommendations without any relation to the success of therapy.[29] Yet I found one comparative semi-quantitative attempt: a bilious fever which „yielded to medicine.... for, a great number of the country people perished for want of it, whilst most of our men recovered by timely care of the surgeons of their regiments“. Indeed, one regiment (commonly 813 men) lost twenty, another with the next highest returns, only eleven. And those men who were kept with their regiments did better than those who were sent to the general hospital.[30]

This apparent dichotomy in his work was explained by Pringle himself in an interesting discussion of the function of general theory in determining clinical treatment, and which was a justification of rational empiricism:

„To my account of the malignant fever.... as well as to the account of the bilious fevers and dysentery, I have subjoined my conjectures about their more subtle and immediate causes; tho’ I am aware than an attempt of this kind may rather tend to weaken than to confirm my observations: as we but too frequently see the judgement influenced and perverted by such kind of theories. But not only the descriptions but the cure of all those diseases were long established, before I thought of assigning these causes; and which, indeed, have been sometimes first suggested by the effects of the remedies. Yet, the use of a theory is evident from the necessity of varying the medicines oftener than can be taught either by mere empiricism or even by analogy from other fevers.“[31] (The last sentence was another relic of Boerhaavian teaching: „Quam vanum, fallax & damnosum sit, ad has commentare unum, qualecunque demum sit, medicamentum proprium? Aut unam universalem medendi methodum“?[32]).

Boerhaave had revived the Hippocratic corruption of the humours as remote cause of fever. He assigned it to an imbalance of the acids and alkalis having led to „septicity“or „putridity“. „But“, Pringle added, „as my celebrated Master had not time to ascertain every part of his doctrine from experiments of his own; it was no wonder some mistakes were made, and... the extent of these principles were not fully understood“.[33]

Pringle in 1750-1752 undertook the necessary laboratory experiments, by which he ascertained the „septic“and „antiseptic“qualities inherent in various substances and parts of the body. „Two things“, he said,

„induced me to prosecute this subject; the great number of putrid cases, that were under my care in the hospitals abroad; and the authority of Lord Bacon, who offers many reasons for considering the knowledge of what brings on and retards putrefaction, as most likely to account for many of the more abstruse operations of nature.“[34]

His experiments were highly rated by his contemporaries and brought him the 1752 Copley Medal for the best communication to the Royal Society of the year.[35] They were so important to his wider conclusions that he reprinted them as an appendix to his *Observations* (1752).

According to his theory, which was now supported by experimental evidence, Pringle proposed for putrid fevers and diseases a treatment aimed at the purification of the air without, and of the bloodstream within the patient. The former he would effect by cleanliness and ventilation, utilising the skill of an engineer, the latter, by correct diet, and above all by bleeding and antiseptic medicaments.[36] But these recommendations lacked Lind’s confirmation by statements of actual success in terms of mortality. Unlike Lind, Pringle did not have a hospital at his disposal after the Austrian War. Although he left ten folio volumes of case notes of private patients[37] he never analysed them numerically, a fact for which many reasons can be found.

The difficulties which the private physician had at that time in collecting sufficient evidence of the effects of a medicament are shown in the trouble Pringle took to ascertain the „specificity“ of antimony against dysentery in 1738, before he became an Army physician. He wrote to five colleagues; four responded with one or two successful cases, whereas the other, who was the only Army surgeon, described having „190 patients all treated after the same method...., of which I lost but one“.[38] We encounter the same situation again in the mid-1750s when Pringle, now a London physician, tried to collect evidence (both clinical and experimental) concerning therapy of syphilis (see above p.??), and the use of soap, lime-water and Carlsbad water against „the stone“. Besides some laboratory experiments, he collected only successful cases of dissolution of stones.[39] Therapeutic trial was definitely more Lind’s than Pringle’s field.

On the other hand Pringle surely propagated scientific thinking. His readiness to adapt a hypothesis to new observations shows his recognition of the primacy of the latter over the former. I have mentioned one example in the history of scurvy, when in 1776 he even questioned the value of his famous experiments of 1752, (see above p.??). This attitude becomes also apparent when one considers how he continually expanded and re-adjusted the above quoted passage concerning the use of a theory: in the third edition of his *Observations* (1760) he wrote:

„In reasoning upon the nature of the bilious fevers, the hospital-fever, and the dysentery, I have so much recourse to the *septic principle*, that the Reader may imagine I have considered it as a more universal cause than I really think it; for except in these distempers, and in a few more, which I have alluded to in this work, I have hitherto referred no other disorder to that origin. But as to the reality of such a principle... I think I have sufficiently ascertained [it] in the observations.“[40]

In the fourth edition (1746) he was even ready to change his opinion and to replace this septic principle by a system of contagion by animacula. He wrote, after perusal of the recent literature: „It seems reasonable to suspend all *hypotheses*, till that matter is further inquired into“.[41] Thus Pringle, aware of the imperfect state of the medical sciences of his day, was cautious in his deductions, which were often accompanied by a plea for further research.[42] His influence was as great as it was Janus-headed. Relying on the past as well as being progressive, it can perfectly be traced, for instance, in the 18th century history of scurvy. It can be seen also in Pringle's support for a new literary *genre* among travelling doctors, based on an old assumption: one of his papers to the Royal Society was an account which a military surgeon had sent him from the East Indies and which tried to relate precise meteorological data to the incidence of disease in general, and to certain specific diseases (26 species).[43]

Personally Pringle was highly recognised. He was an F.R.S. as early as 1745, and became a F.R.C.P. without being a graduate from Oxford or Cambridge. [The election was carried through, as in the case of Donald Monro, through the rare process of *speciali gratia*.[44]] He was knighted in 1766. His presidency of the Royal Society (1772-1778) was very successful. Honours came to him from scientific and medical bodies in the Netherlands, Spain, France, Russia and Naples. He frequently entertained eminent and learned foreigners in his house in London where his personal medical circle included the leading London men of his time.

In conclusion, Pringle seems less relevant for the quantitative evaluation of therapeutic efforts than for that of preventive efforts and for the propagation of the inductive method. However, it must be remembered that he and Cleghorn were important for the propagation of the emphasis on observation and recording into a field where it could become mass observation, and where it was also soon to be utilised for the numerical assessment of therapeutic success.

3. Contemporaries and Followers

The subsequent 18th century books of military medicine and surgery not only quoted Pringle (together with Cleghorn and Lind) but bore the stamp of their early treatises. Those meant to be practical guides for the lower ranks of surgeons, like Ranby's *Method* (see above p.??), van Swieten's *Kurze Beschreibung* (1758) or Theden's *Unterricht* (1778), cannot be expected to contain any accounts of research. More revealing are books containing medical accounts of wars or single campaigns, like that by Francis Home on some aspects of the Austrian War, those by British authors such as Donald Monro and Richard Brocklesby, and by the Prussian physician Ernst Baldinger on their experiences during the Seven Years' War.

Francis Home (1719-1813),[45] who had served as surgeon in Flanders from 1742 to 1748 and had studied in the winter-breaks at Leyden, described his experience with camp diseases in his *Medical facts and experiments* (1759) as a complement to Pringle's account of the Flanders campaign. This book proved his careful recording of his observations in Pringle's style. [It was translated into French and German] Home stressed the value of unsuccessful cases and the fallacy of trusting a few observations only for the assessment of the value of a cure. Himself he reported all his thirteen experiments of inoculation of measles.[46] His methodological recommendations were later referred to by John Ferriar (see above p.??).

Richard Brocklesby's (1724-1797) *Economical and medical observations* (1764) was meant to be a less genuine Verulamian but more popular „Pringle" type account.[47] It featured, in fact, all the characteristics of the latter: some numerical accounts of incidence of diseases, iatro-chemical experiments as the bases of treatment, and, in an appendix, a medical topography of Senegal.[48] The same held for Ernst Gottfried Baldinger's *Krankheiten einer Armee*, (1765) except that there were no accounts of chemical experiments. Both works lacked numerical statements of the success attending their therapeutic recommendations. Both however tried to extend Pringle's spirit amongst military surgeons.

One way for propagating Pringle's approach consisted in proper regulations for field hospitals (which both drew up) and their enforcement by the application of military discipline. For

Baldinger this was the only means of overcoming the major drawbacks inherent in military medicine, i.e. the vulgarity of the soldiery, and the constant changes of doctors and of medical institutions. A perfected system of returns such as that existing in the Prussian army was thus seen as „necessary control“, and also as a useful scheme for the doctor who might be aware of the advantage of great numbers of observations, which were possible in military practice. Admittedly, these returns were initially designed more for logistic and economical than for medico-scientific purposes, but Johann Ulrich Bilguer’s internationally known example of their use with this latter aim (see below) was proudly and repeatedly acknowledged.[49]

By the mid-1760s therefore the development of aspects of Boerhaave’s teaching by his own men had helped seting the stage for the introduction into the Army of those methods which Lind and Robertson had already been using in the Navy for several years. This introduction was to occur on the next occasion of war, some ten years later, not without a dramatic opening on a text concerning the treatment of fevers.

C. The Treatment of Fever 1750-1790

1. The Seven Years’ War

Neither Cleghorn, Pringle, nor Home had published results of theirs method of care during the Austrian War. A position similar to Pringle’s in Flanders from 1742 to 1748 was held by Donald Monro during the Seven Years’ War, when he was physician to the British military hospital in Germany from December 1760 to March 1763.[50]

His *Account* thereon (1764) (see above p.??) still showed all the features of Pringle’s work. Monro tells us that returns of every patient, including diagnosis, treatment, dates of admission and of discharge, and success or lack of success had had to be entered into registers - again for principally administrative use.[51]

But in a second edition (1780) with the „Lindish“ title of *Observations on the means of preserving the health of soldiers*, Monro added some data on morbidity *and* mortality from an encampment at Coxheath in 1778-79, where he had again directed a hospital.[52] This edition is also interesting for the account of the fate of the hospital returns from the Seven Years’ War. Monro pretended that they had been „lately“destroyed by an official in the War Office.

Therefore the returns included in John Millar's *Observations* (1777), for arithmetical comparison of mortalities of various hospitals, had to be a fake. In a lengthy Appendix Monro made this accusation as a defence against Millar's open attack on the military doctors, and especially against Millar's criticism of the old-fashioned anti-phlogistic therapy in camp fevers, as recommended in the standard works of Pringle and Monro himself.[53] Millar's pleas and Monro's regrets that he could not present accurate hospital returns thus found entry (through the „back door") into a current work of military medicine.

I shall now discuss how the practice of the active Army surgeons with respect to the presentation of results was to evolve during the subsequent West Indian Campaigns.

2. THE WEST INDIAN CAMPAIGNS

a. John Hume

One of the first specific reports on fevers from the new theatre of war was John Hume's (1706-1772).[54] *Account of the true bilious or yellow fever...* (1778). By then Hume was a commissioner of the naval Sick and Hurt board. Previously he had served as a naval surgeon and organizer of a hospital in Jamaica from 1739-1748. Besides a clinical description of a type of yellow fever which he showed to be distinct from other species, [It was later adopted by Lind.] Hume also reported on the treatment he had used in the 1740s. Having seen only bad effects with traditional bleeding and vomiting, he changed to clysters followed by bark and abundant drinking to induce transpiration.[55] Nevertheless his mortality was „nearly one in seven, of the whole number sent to the hospital". This statement was illustrated by quarterly returns of the sick and dead from his hospital in Jamaica for 1741 and 1742.

Hume described his account in a letter to Donald Monro as follows:

„The foregoing account is an exact one, because it is taken from the hospital books, which are checked by the books of each ship to which the dead and sick men belonged."

He also aptly weighed his statement of the results for he admitted to being unable to ascertain what proportion of the 11,800 men had suffered from yellow fever. But from the comparison of the respective returns Hume could at least conclude that the overall mortality in the Army had been still greater than that in the Navy.[56]

With the next four authors I shall examine therefore how the fever problem was dealt with by the Army surgeons at that time.

b. John Rollo

From the Army point of view the first report was the *Observations* from Barbados (1780) by John Rollo (t1809), a Scot trained in Edinburgh. Rollo's *Observations* contained a table of all cases treated in his hospital on St. Lucia from December 1778 to May 1779 with name, diagnosis, age, dates of admission and discharge (or death). Writing in the typical climatologic fashion of the period, in another table Rollo related the numbers of healthy, sick and relapsing men, and the numbers of deaths, describing also several localities on the Island and giving an account of their climate. His tables were designed to distinguish, „with some degree of certainty", the healthy from the unhealthy situations on the Island.[57] Rollo mentioned no therapeutic trials, but he referred the reader to a future book which would describe his new therapy of dysentery, the most common disease in his practice. This booklet came out in 1786. It contained many speculations on the cause of dysentery taken from Pringle, Cleghorn, bland and Donald Monro, but included no numerical statements on the efficacy of any treatment.

Later, as chief of the Medical Department of the Ordnance and of its hospital at Woolwich, Rollo again kept records of his practice, and published a complete numerical hospital report in 1801 (see below) It included the 1798 instructions of the Army Medical board for his subordinates. Rollo especially stressed how the medical books were to be kept. He printed specimens of forms developed by him which I have reproduced in *Table 4*.[58]

Rollo stressed that the principles on which the management of Woolwich Hospital and the whole medical service of the Ordnance were conducted related „to the benefit of the patient, and the improvement of the Surgeon. Objects invariably in view. They are reciprocal. Unless the surgeon improves, the patient cannot obtain all the advantages he otherwise would derive". As had Millar, Rollo thought that human life was too short for a conscientious doctor

„to acquire, even with the most suitable education, unremitting observation, accurate investigation, and unwearied reading, (he recommended Lind, Pringle, Donald Monro, Blane and Chisholm.[59]) ....satisfactory confidence in the unreserved treatment of the sick committed to his charge... . Therefore, in all establishments the improvement of the Profession should be held systematically in view.“[60]

Statistical accounts were the basis for this plan. Thus each surgeon detached from Woolwich had precise recording duties. He had to keep a register and a case-book, and he had to send regular abstracts to the commanding officer as well as to Rollo, who fully agreed with the sentence of the 1798 regulations [Contained already in John Clark (see above) and John Hunter (see below).] that „the journals to be kept by the surgeon will be undeniable proofs and the best evidence of his professional abilities“. Indeed, the returns preserved had allowed him to make a tabular abstract of the occurrences, the cures, deaths and proportional mortality of certain diseases for the period from January 1796 till December 1800, intended as a basic reference document.[61]

Rollo, who later also published statistics in defence of vaccination (1804) is otherwise especially known for his dietary treatment of diabetes.[62]

c. John Hunter (of Jamaica)

John Hunter (†1809), a distinguished military surgeon in Jamaica, wrote perhaps the most influential book on the diseases of the West Indies.[63] He was Edinburgh trained as was John Rollo, became an L.R.C.P. of London in 1777, and physician to the forces in Jamaica in 1780. He was elected F.R.S. in 1786. His *Observations* first appeared in 1788.[There were four English editions and a German translation.] Hunter insisted on one’s own observations being more conducive to the improvement of knowledge than the collection of the opinion of others; he thus analysed returns from hospital books from 1779 until 1783 with reference to the earlier works of two naval colleagues Robertson (1777) and John Clark (1773,1780).[64] He declared that: „The number of men lost annually by the several regiments...will point out the principle and aggravating causes of mortality, and what is of more consequence, it will show how in a great degree they may be avoided“. Just as Pringle had, he said that these calculations would give the commanding officers the number of sick men at the most healthy and the most unhealthy season of the year - as well as an estimate of the rate of diminution of a new contingent. Thus he compared the mortality and morbidity figures in each regiment before and after certain preventive measures had been taken, and also added them up to overall figures.[65] Hunter repeated the plea for the necessity of hospital books or registers, (practically using John Clark's own wording) for they would effect

„the best proof of the diligence and abilities of the surgeons... A plan of this kind might greatly contribute to improving our knowledge of diseases, in all the various climates to which the possessions of the British empire extend; and, by enabling us to take better care of the health of our seamen and soldiers, prove a national benefit."[66]

However, like Rollo's, Hunter's book contained no numerical statements in its therapeutic section.

d. Benjamin Moseley

Numerical statements of therapeutic outcome were also meagre in Benjamin Moseley's Treatise on tropical diseases (1789). (Four English editions up to 1804, one German translation in 1780). This Englishman (1742-1819) had studied in London, Paris and Leyden. He claimed to have invented the cure of dysentery by inducing massive perspiration. By this treatment he said he had not lost a single patient out of the 267 suffering mainly of the dysentery and of bilious and remittent fevers in the autumn of 1780.[67]

Despite the deficiencies of his own work, Moseley's diatribe on the theory of presentation of therapeutical results ranks amongst the most incisive ones of all the 18th century military writers. He criticised that „custom of very ancient prescription", that medical authors select their successful and remarkable cases only to support any new doctrine, as profitable to them, but less to mankind.[68] He felt that the entire history of the literature on dysentery, with the exception of Sydenham, led to one conclusion: that „different practitioners having seen the disease under different circumstances, conclude that every person beside themselves has been mistaken respecting the true method of treating it".[69] This is just what Moseley did himself for he wrote: „I never could cure the diseases to which [my work] extends, by the books that have been already written on the same subject by others". And in support of his method he deemed to above stated results sufficient, instead of drawing out his materials into long dissertations.[70]

Another contemporary book full of valuable theoretical assertions concerning the evaluation of therapy, but without their application, was Robert Jackson's *Treatise* (1791) which I shall discuss more appropriately in the context of his complete works (see p.??).

The West Indian Campaigns therefore yielded at first no statistical results of the therapeutic practice in fevers although hospital returns were analysed with respect to the effect of preventive measures. It is understandable that one John Bell, an Army surgeon in the West Indies (and namesake of the more famous Edinburgh anatomist and surgeon), in 1791 called for an official Army handbook summarizing the useful lessons from these campaigns. John Hunter of Jamaica had already made the same observation in 1788. In fact, the more famous surgeon - physiologist and surgeon general of the Army John Hunter, recommended his namesake's book when a new attempt for domination of the Caribbean islands was decided on by Pitt's Ministry in 1793.[71] Yet in this very year there was one exceptional book to appear, by a pupil of John Millar, which also included material from the West Indies.

e. Two Pupils of John Millar: Thomas Dickson Reide and John Marshall

Thomas Dickson Reide's *View of the diseases of the Army...* (1793) appeared just before the reopening of a campaign in the West Indies in the same year. Reide had been a *protégé* of John Millar at the Westminster Dispensary. He had made out the early statistics of Millar's practice there in the mid-1770s, and, on becoming an Army surgeon in 1776, adopted his system of noting every case and of analysing them at given periods.[72] His work was full of tabular compilations, such as monthly and annual returns of the sick and dead according to diagnosis, and the proportionality of deaths to the whole numbers of the sick.[73] The number of enlisted men was also regularly tabulated, so that other proportionalities could be made out.

Reide had served from 1776 till 1791 in Britain, North America and the West Indies, so that his book attempted to give an objective view of the mortality in these different climatic zones; he independently arrived at Robertson's and Clark's view of the unnecessary subdivision of fevers into more than three species, all of which seemed to him to yield to one single plan of treatment: the early and liberal use of the bark. This was the chief design of his book, for Reide had adopted Millar's method of treatment, having been rather unsuccessful with others' before. He listed not only his own results, but he compared them with those of Blane, Rollo and Robertson, which showed that he had been very successful indeed.[74]

The intrigue around the attempted prevention of the publication of this work by governmental authorities was narrated by John Millar in 1798. It was partly ascribable to the tendency to save money on the medical preparation for campaigns, since the occurring diseases were anyhow regarded as „incurable". Apparently a promotion which was to be granted to Reide directly by the Duke of York, as recognition of his clear success in the West Indies, was withheld. Reide, thus expelled from the Medical Service, became a captain and regimental adjutant in Ireland, „a station to which thousand were competent", as Millar commented angrily.[75] (According to Millar one John Marshall, another of his pupils who became an Army surgeon and adopted his plan of publishing returns, seems also to have been prevented from re-entering the service at the outbreak of the Revolutionary Wars, being considered a Jacobin, leveller, republican and democrat..."[76]). Reide, however, was to be rehabilitated as the first true Army statistician by Henry Marshall in 1833.[77]

There is no better summary of the situation concerning the therapy of fevers by 1793 than that by Reide himself, which is, in addition, a masterly example of British common sense. He said that many treatments of tropical diseases had been recommended in the past few years. All authors spoke of successful cures, although none agreed with each other. How was the young practitioner to decide since many were mere assertions, „unsupported by [the]least shadow of proof"? [Compare this statement with Rosen's description of the same situation in the late 1820s (see above p.??).] The answer lay in the application of „shop arithmetic". A physician without the assistance of regular registers could form no idea of the result of his won practice, and much less of its comparative success, and yet, partial registers were worse than none, as they were not only defective but fallacious.

„How ridiculous would it appear [for a merchant] to judge of the advantages or disadvantages of particular branches of commerce from reasoning and conjecture, whilst the result can be reduced to certainty by keeping regular accounts, and balancing them at stated periods."[78]

This was exactly what Millar's two pupils had been the first to do persistently within the Army so far as therapy was concerned, [Pringle and John Hunter of Jamaica had used the method earlier in relation with preventive, hygienic measures] with the sole exception of the abortive attempt by Donald Monro , (which had probably been induced by Millar's *Report* of 1777). One may now wonder whether these examples would be emulated during the forthcoming wars.

3. JOHN MILLAR

I have already hinted *en passant* to the literary quarrel between Monro and Millar. Here I subjoin some detailed figures which Millar used to argue his case, i.e. the replacement of bleeding and antimonials by Peruvian bark for the treatment of both tropical and continuous fevers. First, he compared general mortality rates of big cities (e.g. London 1 in 20, Vienna 1 in 19.5, Berlin 1 in 45), with those of the Army in the Seven Years' War in Germany (1 in 16, 1 in 8 and 1 in 6, for 1759, 1760 and 1761 respectively). This difference, he thought, might partly be accounted for by a difference in air and manner of life. Yet he declared that the doctors ought to reconsider their practice, too; for from some hospital returns which an anonymous friend had procured for him, and from the death lists of the British regiments, it transpired that 1300 out of 20,000 soldiers had died from wounds obtained in action, but 6500 from diseases, a mortality among the sick of over 50%.[79] This was about double the mortality in the British military hospitals in Flanders during the earlier War of the Austrian Succession, for which hospitals Millar compiled a table from the returns delivered to the commander in chief of the Army. For Pringle's hospitals, for instances, the figures were:

Physician	**Hospital**	**Year**	**Period**	**Admitted**	**Dead**	**Proportion**
J. Pringle	Brussels	1744	28/4- 24/12	1259	89	1 : 15
J. Pringle	Maestricht	1746/7	26/7- 28/2-	1165	119	nearly 1 : 9

The average mortality he calculated to have been at least 1 in 9.[80] But the mortalities in the hospitals in the Seven Years' War (i.e. 50%) also looked dreary if compared with those in civil hospitals in Britain (where they varied between 1 in 13 to 1 in 20), in Amsterdam (1 in 7) and in Paris (1 in 7 in the Hôtel-Dieu). The situation seemed even worse if compared with that in the dispensaries. Lettsom's Aldersgate Dispensary had a mortality of 1 in 33, Millar's own Westminster Dispensary 1 in 110: Even if the deaths omitted due to Millar's peculiar listing (see p.??) were considered, one arrived at 1 in 30 at the worst. The information on British hospitals was available from vital statisticians such as the Reverend Price and Thomas Percival. That about Paris and Amsterdam Millar obtained from a medical friend who had travelled on the continent.[81] Millar used these figures to prove that the mortality of fevers treated without antimony and bleeding could be dept very low. It must be added that he made the assumption that camp fever (typhus) had been the most prevalent disease in both camps and civil institutions. He was obliged to do this, though he and Lettsom had published specific mortalities of fever itself, but such figures were not available for the Austrian and Seven Years' Wars. In 1779 and 1783, however, he would be able to make a specific comparison.

Indeed, after this 1777 report, Millar continued his assiduous campaign for the abolition of „vampyrism"in the Army wherein statistical returns were crucial. He made this clear in his subsequent *Observations on the management of the prevailing diseases in Great Britain, particularly in the Army and Navy*, privately printed and distributed gratuitously in early 1779 to the official departments concerned. It was followed in 1783 by a *Reply* to Donald Monro's justification of 1780 and in 1798 by an *Appeal to the people of Great Britain*, together with a reprint of the former books.

From a historical review of the various methods of cure proposed since Hippocrates for putrid inflammatory fevers, Millar had concluded in 1770 already that Peruvian bark appeared to be the only remedy on which one might depend. For recent evidence he had referred to Lind's trials at Haslar Hospital and to Cleghorn.[82] Meanwhile he had confirmed this conclusion at the Westminster Dispensary, and an „abstract of the returns of the military hospitals in Flanders and Germany renders it easy to reduce to numbers, and compare the effect of a contrary practice".[83] In 1779 he thus reprinted all the tables already included in the 1777 report with appropriate references. But there were some important additions.

First Millar thought that

„as a standard for comparing the various success of different methods of cure, it will be proper to ascertain the ordinary termination of disease when left to the unassisted efforts of the constitution.... Hippocrates has given an account of the progress of fevers under careful domestic management and this may be taken as a standard..."

Millar then subjoined the numerical details of 42 cases which were also to be used later for the same purpose by Blane and Bisset Hawkins (see above). He asserted that the antiphlogistic treatment lately continued and confirmed through Boerhaave's authority and that of some of his pupils like Pringle, went back to Galen. The latter had narrated:

„in his manner, a miraculous story of the effects of bleeding, which were so palpably evident, that the spectators exclaimed, O! wonderful Doctor! Since then, the practice has always been general but never successful.“[84]

Second, Millar looked at fevers from a new point of view: there was no disease more fatal than this putrid fever; its mortality in the last war had been even greater than that of the plagues of London in the 17th century, as calculated by Graunt.[Millar's calculations were only partially correct: from the West Indies there were reports of mortalities of 55 out of 70 patients; the Army alone had lost 1 in 6; but the plague in London in 1665 97,000 out of 460,000-500,000.[85]] This indicated, he felt, a „degeneration of medicine“, which brought the practice of physic into discredit, and increased that of quacks.[86] Such high mortalities were considered normal by the established physicians such as Donald Monro, Pringle and their friends. They even wondered how in Haen's practice in Vienna only twelve out of 500 ill with malignant fever could have died; bluntly pretending that the diagnosis must have been mistaken, that the fevers in Vienna were probably less acute than in London, and that Haen's petechiae were only flea bites. Millar replied with a statistical argument against these assertions: „It may be fairly presumed that in an equal number of fevers at Vienna and London, or elsewhere, an equal proportion of slight and dangerous cases would originally occur“. He claimed that mortality was lower in Vienna because Haen had treated his patients properly, i.e. only with unspecific cordials in light cases and the bark in difficult ones. And he apologised to Pringle for having been obliged to quote him so often in a bad light. But he added: „Amicus Socrates, Amicus Plato, *sed magis Amica vertitas*“(*sic*!)[87]

In fact Pringle himself had already yielded to evidence, for in his *laudatio* on Cook in 1776 he had admitted that bleeding was not indispensable.[88] This was noted with satisfaction by Millar in 1779. By then he was also able to give additional evidence for his case in the form of specific mortalities of fevers from different sources. First, he paid tribute to Heberden and Fothergill, two eminent London physicians, by including two of their isolated cases, treated favourably with bark. Then, he relied chiefly on statistical reports, such as those from

Lettsom's Dispensary (decrease of fever mortality after the introduction of bark in 1773), from John Clark's *Observations*, and from Robertson. Together with his own mortality figures and those of Hippocrates, Haen and Pringle, Millar presented all this evidence in a succinct comparative table.[89] As collateral „proof" he also adduced the London Bills of Mortality, but not in the indiscriminate way which he had criticised before. They indeed showed a decrease in mortality from *fever* since 1770 when compared with the fifty years before 1770. [1770 was the year when his *Observations on the diseases of Great Britain* had first appeared. Millar arranged the figures from the Bill as follows:

	1720-1770	1770-1776
Average annual mortality from fever	3577	2638
highest	7528	3608
lowest	2070	1893

Finally, Millar advocated his Dispensary plan concerning the use of recorded mass observation for the evaluation of therapy (see above) for adoption in the Army.[90]

This work, stormy in style and farsighted in presentation of the data and in its outlook provoked mixed reception. Soon after its appearance William Hunter wrote to Millar:

„I have already read the whole; and, admitting the facts (and no doubt you will take care to be accurate) anybody but myself would be surprised. The publication would be of great use at this time, especially in America. The Lord in his mercy keep us out of the hands of (Pringle?) in this world, and the Devil in the next."[91]

In December 1778, Hunter arranged that the book be given by Millar to Lord Amherst, the commander in chief. But his plan was allegedly obstructed by ministers and by officers until the 1790s: the traditional antiphlogistic therapy for fevers and dysentery continued!

The North American Campaign under Cornwallis (1738-1805) turned out to be a disaster, both militarily and medically.[92] Millar implied that there was a relationship between the military and medical failures, accusing the military administration of mismanagement and failure to recognise what had already been proved clearly. In 1783, in his *Reply* to Donald Monro, Millar again illustrated the advantage of „mild“ treatment of fever with a striking series of numerically stated results; these were drawn together in a Table of the comparative success of different methods of treating fevers, chiefly taken from the recent writings by Robertson, whom he had met in 1779. He also included data from Rollo and Monro himself. (Rollo had not himself analysed his tables with that respect, but Millar could do this easily from the precise indications contained therein). In terms of mortality this table again showed the clear advantage of the bark treatment over the bleeding and evacuation therapy (BE), over other medicaments, and over the practice of allowing the disease to take its natural course. Millar did not refrain from including non-numerical statements, such as „almost all (or none) died“into a table, too. The figures were:[93]

Place	**Therapy**	**Patients ill***	**Death Rate**	**Reference**
San Juan	BE	400	„almost all“	T. Dancer
Senegal	BE	?	„almost all“	Schotte
New York	camphor	36	1 in 7	R.Robertson
Rhode Island	antimonials	24	1 in6	--
Gibraltar	antimonials	570	1 in 10	--
	& camphor	437	1 in 13	--
Coxheath	BE			
	antimonials	?	1 in 17	D.Monro
	bark later			
St.Lucia	antimonials,	105	1 in 15	J.Rollo
	cooling medicines			
	bark			
Senegal	E, bark	?	„almost nil“	Schotte
HMS Edgar				
1779/80	bark,early	475	1 in 80	Robertson
1781/82	-	177	nil	--

HMS „Juno“	-	216	1 in 216	--
HMS „Juno“	unspecific cordials (i.e.) natural course of the disease)	296	1 in 49	--

* The figures in this column, except those underlined, are added by me from the original references quoted by Millar. The underlined figures are in Millar’s original table or in the descriptive text of his 1783 monograph.

After the American War had ended Millar continued his crusade for improving medicine by rendering the results of medical practice more transparent, „with the same zeal and ardour ...[as] the indispensable duty of a citizen of the world“. His own account of his trip to France in 1788, of how the publication of his plan in Paris was withheld by the intervention of the English Government with the aid of Mirabeau is almost unbelievable. It would deserve separate study.[94] However, Millar was finally received, at the onset of the Revolutionary War in January 1792, at the Secretary of State’s office in Whitehall, where he presented a Memorial to the president of the Board of control. He underlined the advantages of the precise control of medical practice by including in his Memorial the reports of his former pupils Reide and Marshall, and Clark’s analysis of the returns instituted by the East India Company in 1770. On considering the Memorial, the President of the Board of Control ordered a plan to be immediately made out along the lines of the HEIC. It was delivered by Millar three days later and transmitted to the Secretary of War and the Chairman of the HEIC. His own estimation of the plan can be seen in Millar’s statement that „It would be improper to publish that plan at the present, lest the advantages to be derived from it.... should be improved against us by our enemies.“[95]

Nevertheless, in 1798, Millar published „An appeal to the people of Great Britain on the state of medicine in England...“as a prefix to the second edition of his *Observations on the prevailing diseases in Great Britain*. This „Appeal“ intended to show the reader that medicine was no longer the concern of individual physicians, but was extremely relevant to political economy and military affairs, and was necessary for the successful direction of the business of the state. As such, medicine was in turn influenced by them ought to adopt their modern techniques, such as arithmetics: „Where Mathematical Reasoning can be had, it is a great folly to make use of any other, as to grope for a thing in the dark, when you have a candle standing by you“. Millar also enjoined the need, and the opportunity, for mass observation in the armed forces:

„For though in the detail of private practice, it must be subject to that mystery which imposes on the frailty of mankind, yet, by the chaste application of Arithmetical Calculation, it my be accurately determined, what can or cannot be done in the cure of diseases, among bodies of men assembled in fleets and armies.“

Just as the example of the East India Company had shown.[96]

Like Robertson, John Millar has been a neglected figure, although their contemporary William Black stressed their pioneering role with special mention of their „indefatigable“study „of comparative success by different febrile remedies“. Lloyd and Coulter list and quote only briefly from Millar’s collected works published in 1802 or 1803, and he is mentioned by Abraham and Hunt only in relation to his troublesome presidency of the Medical Society of London. The D.N.B. speaks of him as an „excellent physician...., but ... eccentric and irritable“.[97] Millar would merit to be dealt with more extensively, especially as for him medical reform through arithmetics was only part of the reform of society in general.

D. More General Use of Returns during the Revolutionary and Napoleonic Wars

1. Thomas Clark and the New Regulations for Regimental Surgeons

After more than thirty years of arguing for the medical utility of good case-books by a number of authors on military medicine their aim was achieved. In 1798 the Medical Board of the Army issued new „Regulations for regimental surgeons and for the better management of the sick in regimental hospitals.[98] Accurate daily registers of the sick, including their treatment, were prescribed and forms for these returns were distributed. However, the fulfilment of this new duty by the regimental surgeons depended entirely on their medical superiors enforcing it and on the military commander’s readiness to support them.

Thomas Clark (born 1770), a Scottish military surgeon stationed in Ceylon, received the new orders through his physician and inspector general in 1798. They were addressed to all Army surgeons and also to those attached to the East India Company. However, Clark asserted that because of the „enervating climates“ and constant overwork „not one of the Company's surgeons at Ceylon and none of the King's ... in all India, obeyed the above orders, excepting the Author, [i.e. Clark] for a time, and Mr Christie of the 80th Regiment“. And after a month, his own assistants, having

„all along considered the obligation to write case-books as an intolerable piece of drudgery, when added to their other duties...., they now resolved to cease to obey an order, which was disregarded with impunity by almost every surgeon in India.[99]

Upon an inspection by his medical superior Clark was jailed for disobedience of orders, and accused of malpractice. That is why he later appeared „before the public, chiefly for the purpose of vindicating his own reputation“, not „merely with a view to extend the limits of science .....“ He was able to plead his innocence. For even it was not his practice to keep regular case-books, he nevertheless had recorded his patients' names, times of admission, diagnoses, discharges or cures and their prescriptions in a daily register which he kept from his arrival in India in November 1796 until his departure in November 1798.[100]

He compiled a tabular abstract of these records and in the text explained the treatments used. During the first year he also subjoined corresponding tables of his own barometer and thermometer readings taken in the morning and at noon. A summary abstract concluded this long series. His much criticised treatment of inflammatory (remitting) fever consisted of nothing else than early bleeding, followed by mercury, and eventually bark at a later stage only. In low nervous fevers (typhus) he gave stimulants (opium and alcohol) internally and applied heat externally. His success was excellent if one can trust his statistics: in 23 months he had admitted 711 fever cases of whom he had lost only seven; of 470 cases of „fluxes“ he had lost only sixteen.[101]

There was only a small step from the completion of the 1798 forms to the idea of linking this technique with more elaborate enquiries concerning scientific goals. One surgeon general, James McGrigor, did it.

2. James McGrigor

(Sir) James McGrigor (1774-1858)[102] was in many ways typical of the late 18th century spirit of emulation in British medicine and surgery. Like Blane in Edinburgh he founded a students' Medical Society in Aberdeen, modelled admittedly on the former.[103] He kept the new Society's case-book of medical histories. Like other Scots who were to make their careers in London, McGrigor started his professional practice as a military surgeon. He was an admirer of the writings of Pringle and Donald Monro who thought the keeping of a daily register of the sick and of their treatment advisable.

Beginning with his first case-book compiled in Halifax in 1797, and then throughout his services in the East Indies and in Europe, McGrigor showed a fascination with statistics, experimenting with techniques of maintaining records of cases, symptoms and treatments of disease. He was also much interested in climatology keeping a register of weather and temperature. He published his statistical findings because „the confirmation or refutation of an opinion in medical science ... [was] no less useful than a new theory, a new medicine or a new mode of curing disease".[104] His initial demonstration of the effectiveness of medical care was contained in his „Account of diseases of the 88th regiment... from December 1798 till June 1800" (1802) wherein the discussion of several diseases was centred on a table of results. It was followed by the description of the successful crossing of the desert from the Red Sea to the Nile Valley by General Baird's Army with only three soldiers lost. McGrigor also published a study on the Egyptian ophthalmia based on hundreds of cases. After this campaign, encouraged by Scottish friends (among them William Wright (1735-1819) and Blane), McGrigor published his notes in *Medical sketches of the expedition to Egypt* (1804), a work closely modelled on Pringle's *Observations*. Besides traditional miasmatic and climatic pathology, and emphasis on Lind's and Blane's preventive reform, it included a major point: its author's emphasis on the necessity for surgeons to keep detailed case-histories of their patients.[105]

After a decade of overseas campaigns, McGrigor hoped for a quieter position at home. He held several minor posts before being appointed in 1806 to the important one of inspector of hospitals of the Portsmouth region. Yet he did not abandon his interest in medical inquiries. In 1810 he published a statistical analysis, arranged in four tables, of the returns he had received from November 1808 to July 1809.[106] He was concerned with the results of fever therapy; but was unable to draw any conclusions, for the treatment had varied with the practitioners whose figures he had drawn up in one table [The overall mortality of continuous fevers was 107 out of 824 cases, i.e. about 1 in 8.5. None of eleven patients with remittents had died.]. Significantly for the time, since this was the middle of the bloodletting revolution (see chapter 4), McGrigor noted that the cold water affusion had been seldom used.

Soon afterwards, Wellington, handicapped in his peninsular campaign by poor medical services, called for his old East India acquaintance, making him surgeon general and head of his medical department. McGrigor sailed in December 1811 for Portugal. Wellington wanted an improvement in the health of his army less for humanitarian than for pragmatic reasons. The sick and wounded in his army, sometimes numbering from one tenth to a third of those under his command, inhibited his campaign, in which he was anyhow heavily outnumbered. For instance, although there were general hospitals, these were badly administered and were far from the theatres of action, and the British had not adopted Larrey's system of ambulances. Moreover, McGrigor's predecessor, who had been appointed without military experience, was apparently unable to provide Wellington and the Medical board in London with regular and accurate data about sickness and mortality which were necessary to plan any renewal of troops.[107]

„Reports of sickness having never been regularly established", McGrigor claimed, „I immediately set about establishing certain returns and records". He closely supervised the execution of his orders, warning his negligent subordinates that „such incompetence, if continues would be reported to the military commander".[108] The excellent terms on which he was with Wellington have him the necessary power to enforce his views. He was a stern taskmaster. Letter after letter to his staff indicated his compulsion for precise and regular statistical information, not only for operational reasons but, as he pointed out, also as valuable material for research and as a basis for medical practice: „In the hands of an Officer of observation and research.... [these returns]ought to be turned to the purpose of science, and ought not to be considered as mere official documents or tasks".[109]

McGrigor himself used statistics for at least three goals. Firstly, he based his suggestions for organisational changes in the medical services [Especially the creation of the regimental hospitals, i.e. middle sized, eventually transportable installations, situated between the front line and the more permanent general hospitals in the rear.] on statistical evidence. He then used similar evidence of his surgeons' successes to raise their social prestige; for instance, during the winter quarters of 1812/13 4,500 men were returned to active service due to medical measures, a proof of the efficacy of McGrigor's system which did not fail to impress Wellington. Indeed, Wellington was on such good terms with his medical chief that, for the first time, he publicly recognised the merits of medical officers in his official report of his victory at Badajoz in 1812. Thirdly, the quality of the returns he received also served McGrigor as one means of estimating the merits of individuals in his medical corps and of promoting keenness and friendly competitiveness among them. Wellington, in turn, supported him in promoting such meritorious men as James Guthrie (see below p.??), instead of inexperienced London appointees.[110]

By such measures McGrigor installed a sense of professionalism and pride in the Army medical services that had not existed previously. He wanted a set of surgeons who understood what they were doing! His own *Sketch of the medical history of the British armies in the peninsula of Spain and Portugal....* (1815) was naturally full of excellent tabular views in which he analysed and summarised the returns he had received. When he became director general of the Army Medical Board in 1815 (a post he held until 1851), he was able to introduce a compulsory reporting system for the whole empire.

At first sight there was great similarity in intention, means, actual achievements and their presentation, and even in public recognition between Blane's work for the Navy and McGrigor's work for the Army. Blane was McGrigor's senior by 22 years and was already an accomplished naval sanitarian and successful hospital physician when McGrigor saw his first active service in 1794. In general, curative and preventive medical care were then superior in the Navy, and McGrigor and his regimental surgeons had the task of elevating military medicine to the level of naval medicine. It is worthwhile noticing how Blane supported his fellow Scotsman. He encouraged him to publish his manuscript of the *Medical sketches*. He also quoted McGrigor's later papers wherever he could, usually with a favourable comment. Blane met McGrigor on his inspection tour to Walcheren in 1809 and supported his organisation of regimental hospitals in his report thereon.[111] However it took nearly twenty years before McGrigor's dream of a comprehensive statistical study of disease, climatic conditions and related measurements was first realized and began to be published (see below p.??). This was despite the fact that he had repeatedly „called for a digest to be made of the folio-volumes of returns"which had been accumulating since 1816.[112] McGrigor's work in the

Army together with Robertson's and Blane's efforts in the Navy established detailed reporting on broader grounds in public life had spread it among the generation of younger medical men who had served under them. McGrigor, as from 1811, was in an influential position to organise and promote the use of the statistical method. He himself analysed the returns of his subordinates, for his keen interest in improving his medical service in particular and for medical science in general. And in parallel a number of senior Army doctors pursued similar „arithmetic observationist" aims with specific reference to certain diseases, one of the main topics still being fevers.

E. THE TREATMENT OF FEVER 1791-1815

1. TYPHUS

I have already discussed the re-introduction of bloodletting for fever in Britain, especially for typhus (see pp.??). In his recent analysis Niebyl points out that in Britain this change of practice (initiated by Benjamin Rush) was entirely supported by statistics[113] notably from the military hospital at Deal and the Fever Hospital in Dublin. In fact, as we have seen, the tradition of bloodletting for continuous fever (typhus) had never died out in Army and Navy hospital practice. It could be found among medical writers from the War of Austrian Succession (1742-48), such as Cleghorn (1716-1789) and Pringle in the 1750s, who had, however, given no specific numerical accounts of their practice. [This may explain partly the aggressive tone in the publications by John Millar.]. But in 1806 the military setting gave Thomas Sutton (1767-1835) (trained in London, Edinburgh and Leyden), the opportunity to submit bloodletting to a thorough clinical trial. He examined what he believed to be a single disease in the one military hospital (Deal), comparing the mortalities of four groups: 1) the usual treatment for typhus (i.e. Peruvian bark, wine, etc.) at the onset of the disease in 37 cases; 2) the same treatment, though administered later in the course of the disease (92 patients); 3) as for group (2) above, but with moderate bleeding at the onset of the disease; 4) bloodletting as the principal remedy.

The results were highly in favour of the last regimen. These patients exhibited an average mortality of one in twenty, compared with one in seven (3/20), one in five (18/92) and one in three (11/37) in groups 3), 2) and 1) respectively.[114]

Several naval surgeons soon published similar numerical „proofs“, for instance in 1812 from the Royal Naval Hospital at Plymouth (remarkably for the time, this report also listed pulse rates, i.e., beats per minute), and in 1816 from the hospital ships of the Russian fleet, directed for some time by (Sir) David James Hamilton Dickson (1780?-1850) the future chief medical inspector of naval hospitals and fleets.[115] Dickson had recommended the antiphlogistic method earlier to his subordinate naval officers, yet without his own trials.[116]

Indeed, there seems to have been little resistance to bloodletting,[117] a practice which was in fact an attack by the retrograde inflammatory theory against the more modern approach of contagion in the question of continuous fevers. The latter consisted in hygienic measures, attempts to lower the body temperature (measured by a thermometer!) by internal and external cooling, and a proved febrifuge, the Peruvian bark. However, the question of fevers in Britain thus being settled in favour of the lancet, I may quit it here and concentrate in the next section on the evaluation of therapy of tropical fevers, and particularly yellow fever.

2. Yellow Fever

Yellow fever was becoming an important issue. Ballingall in his naval and military bibliography (*Table 2*) quotes eleven authors in the time-span from 1791 to 1830 (out of 64 scientific authors) writing exclusively on this topic. First, I shall mention McGrigor's friend[118] Robert Jackson, then Colin Chisholm, William Lemprière and William Wright who served predominantly in the West Indies, and (Sir) William Pym who started his military career there. All were Edinburgh trained Scots who entered the Army or Navy as uncommissioned hospital mates and slowly climbed the ladder. By contrast Edward Nathaniel Bancroft, another author on yellow fever, had begun his career with the high ranking post as a physician to the forces after his M.B. at Cambridge.

a. Robert Jackson and Colin Chisholm

Robert Jackson (1750-1827)[119] was trained in Edinburgh 1768-71. He had his first contact with Army medicine as an assistant to a contractor doctor of the British Army in Jamaica in 1774. He became a regimental surgeon until the end of the North American War. Back in Europe he married, travelled, studied (M.D. Leyden 1785) and finally set up in practice until the onset of the Revolutionary Wars. He then served in Flanders and from 1795-98 he was again in the West Indies (St. Domingo). In 1800 he was appointed physician and head of the Army Hospital at Chatham by the commander in chief. This was done in disregard of the Army Medical Board with, whom he took repeated issue and whose management he continuously criticised after 1803 (when he retired because of the peace of Amiens) until its reorganisation in 1810, when he was rehabilitated with the high rank of inspector of hospitals and medical director of the West Indies (until 1815).

Naturally, Jackson was concerned with the most ravaging disease he encountered, i.e. „fevers". His first *Treatise on the fevers of Jamaica....* (1791) recalled his earlier experiences in the Caribbean and North America. If his writings „failed to exemplify scientific standards", as stated by Blanco,[120] he was at least candid and frank and saw the difficulties in discerning the real effects of treatment. On arriving from Europe he had first tried Peruvian bark for Jamaica fever, „the power of which appeared soon very precarious". Then, he had tried the other drugs *en vogue* (antimony, James's powder, warm water); but was sorry to say that he had not had the opportunity to make proper trials of them in America. Nevertheless he concluded that none of them could be safely trusted.

Jackson honestly admitted that at first he had flattered himself „... in many instances, that I had actually saved life: - I now find, on maturer reflection, that I had in reality done no material good". Indeed, he admitted that he had sometimes also left fevers entirely to nature, „and I cannot say that the difference of the event gives me much cause to be vain."[121]

This was not, he felt, a reason to exaggerate the inefficacy of medicine or even to accuse doctors of killing their patients, but to start anew to evaluate the effects of treatment properly. The first step he envisaged in dealing with the lamentably conjectural state of medicine was for the physician to write down his observations on the spot, day by day:

„If we defer making remarks till the patient recovers or dies, difficulties will be easily go over, and such facts as contradict opinions, in which we have long believed, will be more easily reconciled, as being less perfectly remembered."[122]

Jackson said nothing about numerical results in 1791, yet there were soon to appear when war resumed in the Caribbean in 1793. For as Nodes Dickinson (1776-1755), another surgeon stationed in the West Indies at that time wrote, quoting Charles McLean (see above):

„A most illiberal controversy was carried on by the Practitioners of Jamaica, relative to the best mode of practice in the Yellow Fever. [It bore a great many names and its nature and its symptomatology was avidly discussed throughout our period.] The Object of this dispute did not seem to be the discovery of truth. They universally ranged themselves under two banners; the one maintained the particular efficacy of mercury in all cases: the other, with equal ardour,... the superior efficacy of bloodletting and other antiphlogistic remedies.“[123]

The „best proof“[124] in favour of mercury was considered to be the trial published by Colin Chisholm (†1825) in his *Essay* as early as 1795 and re-edited in 1801. Chisholm advanced the theory that yellow fever was carried from Africa (where it had first been described) to America by slaves.He aptly quoted Lind, saying that „these observations claim the more attention, as not being *only* a few remarks made in private.... [but] they are the result of an attention to some hundred patients, whose cases are still preserved.“[125] He first gave tables of the wind, temperature and weather measurements, then of mortalities (relative to the sick and relative to the whole number of troops) of various units. Finally he distributed 82 cases of yellow fever, which had received four different treatments, in a table showing the number of deaths and the number of recoveries (both of these being subdivided into groups according to the time of death or the beginning of recovery relative to the onset of the disease). He had started the mercury treatment not because of authority, but because upon dissection he had found the lives most affected, and mercury was known as a liver-specific drug. (In fact he gave it together with antimony, nitre and camphor.) In this group sixteen patients had died and forty-two had recovered. This result was better than that with Peruvian bark (eight deaths, seven recoveries) or with an unspecified „Russian treatment“ (two deaths, two recoveries). Angustura bark, however, was encouraging (five cures, no deaths), and further trials of it in twelve cases yielded a mortality of one in three. Thus mercury was more certain in its effects, but this latter new bark could be useful as an accessory medicament.[126] In a later *Manual* on tropical diseases (1822), dedicated to the Duke of York and to McGrigor, Chisholm included his trials again, together with returns showing the seasonal mortalities in regiments and, in an appendix, statistical tables of the mortality from phthisis in Geneva.[127]

In 1796 James Bryce (of the H.E.I.C.) quickly supported Chisholm by reporting only three deaths out of 250 patients under calomel.[128] In 1798 Robert Jackson criticised Chisholm’s calomel cure by means of a report of trials made by one of his subordinates in Jamaica in

1796: out of fifteen patients admitted into hospital on the first day of disease five died, and ten recovered. Salivation had been established in three of the dead and in five of the cured. Four patients, admitted on their second day, recovered but salivation had been established only in one. All three patients admitted later had died, though high doses had been given. This „fairly made“experiment was not encouraging, especially when one considered that „the operation of mercury involve[d] a mystery, and the management of it does not require the exercise of thought and reflection“. Jackson recommended instead copious bleeding at the onset of the disease, yet without being able to „speak positively of the difference of mortality“, which he considered „much less than might be expected from the directly opposite methods of treatment„. Within one year two thirds of the European soldiers on the sea coast would perish anyhow.[129]

Eventually in 1808 Jackson published some figures concerning the mortalities in the West Indies in the 1780s and 1790s and when he had been chief of the military hospital at Chatham (1800-1803). Concerning the former he explained:

„I do not possess detailed returns of what I now state: indeed regular returns of hospital casualty were not then made by me; but I have been able to collect from notes and memorandums that are still in my papers, that one in fifteen was nearly the proportion.“[130]

This low mortality was admittedly only of persons he had seen on their first day. For sailors, arriving later, he gave a proportion of one in five. Usually, at S. Domingo in 1796-1797, one in three was considered a favourable return, but during the time he was regimental surgeon at Cape St. Nicholas for six weeks in 1796, the mortality did not exceed one in twenty. „This is official,“ he added, „and I can venture to say, though no official return was made of the fact, that the result was equally favourable... in subsequent periods“ during his continuance on that island. As head of the Chatham Hospital Jackson indicated a mortality of fever patients of one in 32, for the whole year 1801 (and one in 23 for that of the Isle of Wight). This was low if one compared it with James Currie’s average from four years at the Fever Hospital in Liverpool, i.e. one in 10.5, with his cold water affusion. For Jackson cold water was „not always the first, and not always the more important,“ part of the treatment. Copious bleeding frequently preceded it and was essential to success in his opinion. But as he left „the reader to think it over himself“, we are entitled to say that, despite the numbers involved, this was a mere assertion.[131]

The most evident proof of the superiority of his mode of treatment, Jackson thought, lay in comparing returns from the various West Indian hospitals. Since he became physician-in-chief there in 1811, residing at Barbados, he was able to enforce his cure and at the same time all the returns became accessible to him. He also availed himself of those of previous years for comparison. He presented this statistical report to the Medical Society of London on Christmas Day 1815, (six months after Blane and McGrigor had reported in the Medico-Chirurgical Society) and published in the *Transactions* (1817). The editor's comments displayed a good approach to the use of statistics for the time:

„The principal object of the Society in recording the... returns was to ascertain the advantage from the different modes of treating fever in tropical climates. The comparative mortality of blacks can, in this respect, give but little information; nor indeed, can a fair estimate be formed without a more accurate knowledge of the age and condition for the subjects. The white troops, on the contrary, are chiefly affected with tropical diseases, the consequence of change of climate; are for the most part adolescent; and, generally speaking, in a state fit for active service and... long voyages. In taking the average of any three previous successive years, in the mortality of the white troops, and comparing it with that of 1812,1813 and 1814, the advantage of Dr Jackson's plan will be sufficiently apparent.“[132]

Jackson himself reported that prior to his return to the West Indies (late in 1811), the treatment of febrile diseases had undergone several fashions: such as the Brownian stimulation with alcohol and opium, the cold affusion according to Currie, succeeded by purgatives according to Hamilton (see above) and finally the exhibition of mercury according to Chisholm. For each year from 1803 to 1814 he drew out a separate table giving the numbers of patients admitted, discharged and dead, and the proportion of deaths to the discharged and to the effective establishment of the troops. He listed blacks and whites separately under each heading, and summed up the annual data from about ten stations.[133]

It was, of course, a crude attempt to use overall mortality figures from hospitals for the evaluation of a therapy of a specific disease, a back-fall even as compared to certain workers in the 1770s and 1780s and to contemporaries like Theodore Gordon[134] and McGrigor who published hospital returns broken down according to diagnoses. Yet if one does a summary table for the whites of Barbados, as suggested by the editor, the difference between the pre-

Jackson era (1803-1811) and his time (1812-1814) appears at first sight impressive. The proportional mortalities were one in 19.33; 10; 9.33; 31.05; approx. 15; 11.75; approx. 15; 15.5 and approx. 11 as compared to one in 31.75; 38.5 and 37 with hospital turnovers in comparable ranges.

On closer inspection, one sees that the data nearly overlap, which, even in the absence of any „test of significance“, ought to have called for caution in their interpretation. Yet the appeal to the authority of numerical comparison as ultimate proof was basically accepted by the somewhat biased Jackson *and* the unbiased (as it seems) editor. [Jackson was biased in favour of his mixed cure consisting of copious bleeding followed by stimulation with cold water, emetics, purgatives etc., for which he was involved in a priority quarrel with James Currie.[135]] But further studies hardly would have changed anything in his conclusions. Jackson used the fashionable numerical method for confirmation of his preconceived idea of the inflammatory nature of fevers at home and in the tropics: fevers had therefore to be treated with bleeding. This was perhaps less the case with Chisholm who, in a later edition of his book (1801), admitted to having lost 21 out of 26 freshly arrived recruits treated with mercury.[136]

b. William Lemprière

Jackson’s contemporary William Lemprière (t1834), John Hunter’s successor in Jamaica, continued the latter’s use of official returns in an impressive two volume work on *Practical observations on the diseases of the Army in Jamaica* ...(1799). He could draw on his experience as a superintendent of the military hospitals there. The books gave a survey of the country, climate and diseases of Jamaica, pointing out, that different locations on the island created different influences on one’s health. The effects of discipline on health, as opposed to intemperance, were shown numerically with data for a dozen regiments. The description of the most prevalent diseases, and the mode and success of their treatment, formed another part of the work which was concluded with a description of the duties of the regimental surgeons; this also included a reproduction of the forms for medical and administrative returns.[137]

Lemprière founded all his statements on returns, impressive both for their number and their sheer size (they were given on large folio sheets). He compared the death lists of two towns (from their parish registers thus not including Jews and Blacks) with the hospital returns or the sick list of a regiment stationed at the same place for the same time. Both these lists he broke down according to diagnoses. He tabulated the proportional mortalities and discharges from regimental hospitals according to their post on the island, as he also did with the quarterly returns from a regiment in order to illustrate the salutary effects of discipline, with reference to Theodore Gordon. He also included two detailed tables of quarterly sick returns of all regiments for a period of one and a half years. There was no apparent special therapeutic issue at stake, yet Lemprière made a plea for the scientific utility of these numerical returns.[138]

Lemprière, who obtained a Scottish M.D. in 1799, afterwards became a well-known physician, so that he was sent by the Medical Board with Gilbert Blane as a consultant on the inquiry into the disastrous conditions in Walcheren in 1809. He then became physician to the Army hospital on the Isle of Wight, and as such he tried, with the help of simple statistical methods, to elucidate which of eight diseases he found there (various types of fevers, rheumatism, dysentery etc.) were amenable to the waters of a local spring.[139]

c. William Wright

At the same time as Chisholm and Lemprière, William Wright (1735-1819)[140] was also in the West Indies. After Jackson had refused an appointment as (senior) physician to the expedition by Abercrombie in 1795, Wright had been appointed. He was already sixty years old by then and had a long experience in those latitudes. Indeed he had been a naval surgeon during the Seven Years' War and later set up practice there. On his coming back to Europe in 1777 he was personally invited to Pringle's house, attended meetings of the Medical Society of London, and met several members at their houses. By 1797, the new duty in Jamaica became too cumbersome for him and he returned to Edinburgh. His *Report* on his latest West Indian campaign was published there in 1792 and afterwards translated into several languages.[141]

Wright, too, classified the occurring fevers into only four groups, i.e. intermittents, remittent, continuous and pestilential fevers, all of which he considered contagious. Neglected periodical fevers could degenerate into continuous fevers, of which yellow fever was a particularly malignant form. As for therapy Wright was polypragmatic. He recommended Peruvian bark and purging for typhus, in agreement with Lind and Cleghorn, whereas the cold bath was the initial treatment, followed by bark or mercury in yellow fever. In a former paper he had already claimed to have saved with these methods 75% of his fever patients (if he had been called in late) and all his patients (if he had been called early, during the first access of fever).[142]

Wright, whose priority of the use of the cold water bath was recognised by Currie (see p.??) had a long and friendly exchange of letters with him. He became something of an *éminence grise* for younger Scottish military doctors: It was he who introduced Currie to Robertson and to McGrigor: Wright was also among those who urged the modest McGrigor to publish his *Medical sketches of... Egypt*.[143]

Considering that he had kept a regular journal of his practice when he had been younger, he was either too old, or too little of an organiser, to insist on this method when for a short time in the mid-1790s he held a post of responsibility in the Army. Yet, one can imagine that his recommendations, like those of Chisholm, would fall on receptive ears among the contagionist doctors.

d. EDWARD NATHANIEL BANCROFT

Edward Nathaniel Bancroft (1772-1842) differed from the other authors so far discussed for he entered the Army directly as a high ranking physician after taking his M.B. at Cambridge in 1794.[144] In his Essay on yellow fever, first published in 1811, he based his authoritative remarks on therapy on the writings of others, though not without criticizing Chisholm's account of mercury of 1801. The latter's mortality of 21 out of 26 artillery recruits he thought unacceptably high in any fever. On purely theoretical grounds Bancroft recommended bloodletting instead, completed by Currie's cold water affusion.[145] As did many practitioners he mixed the typical recommendations of an anti-contagionist with those of a contagionist. Also in its nosographical part, this book, reflecting the author's training, was a piece of „cabinet learning"rather than of observational medicine. With dialectical skill it dismissed the traditional theory of a local, spontaneous origin of yellow fever (as held by Pringle, Lind, Blane and Jackson for malaria) in favour of a revolutionary doctrine of a propagating, multiplying contagion; nevertheless he ended up identifying yellow fever with malaria. Just as Wright had, Bancroft might have easily verified during his stay in the West Indies that malaria - or typhus - conditions were not conducive to yellow fever. This was exactly what did two Edinburgh trained Scots, one James Anderson, and (Sir) William Pym (1772-1861).

e. A Further Group of Edinburgh Trained Army Surgeons

Except for a cursory note in *Johnston's Roll* I could find out little on James Anderson. But from his *A few facts and observations on the yellow fever of the West Indies..... with the success attending the method of cure* (1798) it can be seen that he had his roots in Edinburgh. [The book was published there. Anderson was a fellow of the College of Surgeons, and an honorary member of the Royal Physical Society, of Edinburgh.] He clearly differentiated contagious yellow fever from the non-contagious, severe remittents and intermittents (malaria), caused by local marsh miasmata, for, by studying the situation of a town where he had observed the former, it appeared to him „perfectly impossible that it would be affected with marsh miasmata".[146]

More sophisticated and more outspokenly statistical evidence lay at the roots of Pym's studies in differentiating fevers. After long service (1794-1816), first in the Navy and then in the Army in the West Indies, Malta and Gibraltar, where he attained the high rank of deputy-inspector of hospitals, he published in 1815 what has been recently called the first clear account of the disease now known as yellow fever.[147] Yellow fever, he said, was different from remittent and intermittent fevers and he based his differentiation on the returns of the sick which he gave in tabular form, as there was no „more powerful argument".[148]

It was, of course, difficult to distinguish yellow fever from malaria or even jaundice. But some distinguishing features were that its incidence amongst native creoles was slight, whilst it was heaviest amongst drunkards and amongst those Europeans of strong build who were fresh to the tropics. In opposition to malaria, Pym defined yellow fever as a contagious form of fever, attacking the human frame but once and capable of naturalising itself in any permanently warm climate.

Typically for declared contagionists,[149] Pym and Anderson vehemently refuted bloodletting as a therapy for their yellow fever, as contagion was the opposite of inflammation. Quite logically, Pym admitted bleeding in continued and remittent (i.e. „inflammatory") fevers, but he relied solely on the authority of other writers. Despite his own differentiation, Anderson thought that the „manner of attack and the progressive symptoms [were] not so essentially different... as to induce us to follow different methods of cure". On the contrary, he adhered to purgation by calomel [With this view calomel was given in much lower doses than when evacuation by salivation was intended, as e.g. by Chisholm.] aided by James's antimonial powder as „superior to every other medicine". As proof, he adduced a trial he had conducted on his journey back to England: About seventy out of 100 passengers on an accompanying transport vessel, where therapy had been the cold bath, the surgeon and several of the crew were buried early in the voyage. Afterwards it lost „a great many men", and the survivors were obliged to be quarantined on their arrival at Portsmouth. As had Robertson and John Clark, Anderson recognised the advantage of studying a fever on board ship, for the physician was then able to observe it, and to prescribe for it, right from its onset and throughout its course.[150]

f. A Group of East India Company Surgeons

Similarly structured reports by two H.E.I.C. surgeons had recommended calomel purges for tropical fever before Anderson.[151] And the most avid anti-contagionist, and defender of mercury in all acute diseases was yet to come: Charles McLean (c.1766-1824) had been with the H.E,I.C. for fourteen years prior to becoming an Army surgeon in 1804, from which post he deserted after some years.[152] His *Results* were first published in 1811, when he lectured on tropical diseases at the H.E.I.C.

In the 1790s he had started trying out mercury on himself for an intermittent fever. He found that „the result was so satisfactory, that I resolved to continue the practice, in future, in every case of this disease and my expectations were not disappointed".[153] By „analogical reasoning"[154] he extended its use to yellow fever in Jamaica, jaundice, ophthalmia, hectic fever, coup-de-soleil, diarrhoea, dysentery and typhus. In 1796 the results - stated only in general terms such as „unequivocal success" were attacked by a comparison of the mortalities of his and his friend's „mercury-wards" in the Calcutta General Hospital with the mortality of other wards. He rejected the inferences against him by pointing out that his patients were generally in the last stages of hepatitis, dysentery, dropsy, etc., „whilst those in the other wards consisted exclusively of young men, from the European corps, [who were] seldom labouring under diseases severer in degree, than gonorrhoea, or slight intermittent". His proposal to compare his patients with patients of his critics, taken under similar circumstances of constitution and disease, was apparently evaded

„by pretending a reluctance to try experiments with the lives of men; as if it were not manifest, that *my* experiments, which were always tried upon myself, were capable of being conducted with perfect safety; or as if the practice of medicine, in its conjectural state, were anything else, than a continued series of experiments, upon the lives of our fellow-creatures.“[155]

McLean's long attack on the practice of bloodletting and the kind of inconsistent evidence forwarded in its support was perfectly appropriate for the literature he chose to review. This review gave him occasion for some methodological remarks; „the days of miracles are past“, he wrote, and even the influence of the Pope could no longer maintain an argument based on tradition, romantic tales, hearsay, ample experience, and testimony of respectable gentlemen. Instead he pleaded for „principles which are deduced from numerous and undoubted facts, and which can be put to the test of experiment by all mankind.“[156]

Such a programme would have been an advance, but in its execution McLean fell into the same trap which he reproached his opponents (i.e. those advocating bloodletting as a panacea) to have fallen into. As a justification for his views, as early as 1796 McLean had collected reports of „some of the cases“which he had treated successfully with calomel and opium.[157] In 1818, in a privately printed volume entitled *Practical illustrations of the progress of medical improvement for the last thirty years*, he published seventy selected cases, ranging from scurvy to pneumonia all of which he had cured with mercury; in contrast there were nine cases of burns which had proved fatal, he claimed, because they were treated anti-phlogistically. It was always easier to see the faults with others than with oneself!

g. Two Knights: James Fellowes and William Burnett

Doctors of steadier temperament than McLean made their start in the armed forces the beginning of fine careers. Besides McGrigor and Pym there were three high ranking military doctors who received knighthoods after 1815: James Fellowes, William Burnett, and George Ballingall (see below, p.??). Fellowes (1771-1857), an Edinburgh born (but Cambridge educated) Scot, published in 1815 a volume of *Reports* on several fever epidemics in Cadiz, Gibraltar, and in Colchester Military Hospital.

He included tables of hospital returns and numerical statements in favour of his particular therapeutic scheme whereas he dismissed mercury therapy merely with the general observation that „I never saw any advantage from the large quantities of mercury, recommended to be employed in this disorder“.[158]

Despite his preconceptions, Fellowes was honest enough to admit the inefficacy of both schemes in view of the „dreadful mortality... [of over 50% in the case of the Gibraltar epidemic with an estimated population of 10,000 and 5,946 deaths] especially as so many of the practitioners were amongst the victims to its fury“. Indeed, from the surviving medical men he could collect nothing consolatory, nor could any of them hold out a *prospect* of success from the use of any particular medicine. Consequently, he felt the most he could do was „to show the necessity of great attention to the means of preventing the progress of the disease, than to establish any fixed rule for its treatment“.[159] This comes as a refreshing statement after a presentation of a long series of numerically expressed, but unilateral statements of success with bloodletting and mercury, which proved both equally useless in these fevers!

The preventive approach was also taken, for the same reason, by William Burnett (1779-1861).[160] Yet another in the long line of Edinburgh trained Scotsmen, he was chief physician in the Mediterranean fleet in 1814. Unfortunately Burnett let himself engage in a polemic with William Pym about the contagiousness of yellow fever which they had both observed in Gibraltar. This issue overshadowed the question of treatment. Pym as a contagionist was against bleeding, Burnett, as a non-contagionist was in favour of it. He wrote that the strongest proofs of the inadequacy of mercury in the cure of yellow fever were Chisholm's latest results and Jackson's trial (see above). His own evidence for the use of the lancet was collected from selected returns, for access to which he acknowledged McGrigor, and from letters of his own subordinates; some of these reports were statistical, using the departmental forms, some private and merely general statements. Burnett's evidence was actually rather shaky since he defended the non-contagiousness of yellow fever on the basis of a referendum. In their answers to a circular letter he had sent out to seventeen surgeons in Gibraltar, only eight had voted „non-contagious“. Two had opted for „neutral“and seven for „contagious“.[161]

Burnett was not a literary man but an organiser and administrator. As chief physician to the Navy from 1822 to 1852 he was responsible for the organisation of the great statistical reports on the health of the Navy which first appeared in 1840, edited by John Wilson.[162] In that sense he can be compared with McGrigor in the Army. Possibly, the forced occupation with the numerical realities of the massed statistical returns led him in 1831 to return to his earlier confession that all the remedies used - even the lancet, blisters, and purgatives - were in vain.[163] [Indeed, in his *Account of a contagious fever amongst the prisoners of war at Chatham* (1831) he reported 61 deaths out of 518 cases.]

3. Appendix: Ophthalmia

As an appendix to my discussion on fevers I shall add a few remarks on ophthalmia. A quite distinct disease, ti was new to the Army when it set out on the Egyptian campaign in 1801. Yet within a short time ophthalmia became one of the most distressing and widespread of army diseases.[164] The infection proved highly contagious and was regarded as a specially malignant type of inflammation,; starting with purulent conjunctivitis it could lead to panophthalmitis, suppuration of the eyeball and total blindness in more than 10% of the cases. This obliged the state to pay pensions to severe sufferers from the disease. Since even after the return from Egypt ophthalmia continued to recur and spread (partly through self-infliction) to units which had never been present in that campaign, the Medical Board took steps intended to limit the spread of infection, and the accompanying costs, and to ensure the best available cure. One of these was the concentration of all cases upon arrival in England in a specialised „Depot"hospital at Selsea, near Bognor in Sussex in 1807 under the care of John Vetch (1783-1835). Yet another Edinburgh trained Scot (M.D, 1804) who made a career in the Army, Vetch treated around 3,000 patients in this hospital until 1812, and he published quite detailed statistics on those 536 patients already admitted with impaired vision.[165]

Vetch was involved in a question of priority for the method of cure of such patients with the civilian oculist William Adams (later Sir W. Rawson, 1783-1827), who at that moment was in favour with the War Office. Cantlie relates this issue in some detail.[166] Statistics of results were the basis of the argument. As with Carmichael Smyth (p.??), the motives behind these statistics were administrative needs, an argument over priority and eventually a hope for a reward to be voted him by parliament. The treatments were in fact hardly different. Vetch copiously bled for a start, whereas Adams believed in promoting violent vomiting by giving tartar emetic. But the subsequent irrigations, local instillations, cold compresses, and surgical procedures were much the same. Not unsurprisingly McGrigor refuted the alleged successes for lack of convincing proof upon later re-examination of the patients.

4. Recapitulation

During the Revolutionary and Napoleonic Wars official returns and other numerical results were repeatedly and deliberately cited as a basis in therapeutics and in nosography. Yet it is doubtful if these really influenced the choice of any particular therapy. With the exception perhaps of McGrigor and Chisholm, they rather served to illustrate a preconceived practice depending in turn on whether the author had come to believe in the contagious or the epidemological origin of the fever in question. This became a major issue when, after 1800, the „old"theory of contagion underwent rational re-examination, as indeed many traditional theories did during these decades.

It was well recognised by the eclectics, (or the „contingent contagionists") such as James Johnson (1777-1845), an international authority on tropical diseases, the facts for both theories (often numerically expressed), were of the same quality. Intellectually and rationally the two theories evenly balanced each other. Under such circumstances the accident of personal experience and temperament, and also external factors such as the economic outlook and even political loyalties, would determine the attitude of a physician.[167] This would also hold for actual therapy, yet it is noteworthy that the writers considered numerical results to be the best proof of the validity of their hypotheses - whereas sixty years earlier John Pringle and Donald Monro had relied solely on deduction from hypotheses without actually stating results.

This development may have been a fruit of the methodical empirical trials during the last three decades of the 18th century, when the value of Peruvian bark, arsenic and the cold water bath were being numerically assessed. However, as I shall illustrate in the next section, the publication of results remained when this "empirical wave" declined after 1800 and when the therapy of fever again became more overtly dependent on the theory of fever.

F. **The Aftermath of War 1815-1830**

1. The Treatment of Fevers 1816-1830

Though the generalisation that the anti-contagionists favoured bleeding, whilst the contagionists were against it, may be useful, I believe that it is a simplification, since in fact most doctors were „contingent contagionists“, holding an eclectic middle position for the majority of fevers.[168] McLean and Southwood Smith (the medical informant of Chadwick) were part of a politically not ineffective[169] minority which defined epidemic and contagious disease as tow absolutely distinct classes. The first class was epitomized by „malaria“, the second by smallpox. For most authors, however, typhus, yellow fever and the remittent fevers fluctuated between these poles and constituted a rather ambivalent group, including, often without distinction, such diseases as diarrhoea and dysentery.[170]

By 1830 there was some weariness over the subject of fevers and contagion, attributable to the academic nature of many of the issues involved, the difficulties experienced in resolving any of them and the quantity of works produced on the subject since the (typhus?) epidemics of 1818-1819.[171] Consequently there had probably been a mixed practice and polypragmatism corresponding in actuality to that of Cullen, whose influence was still able to influence the regular medical education in the first quarter of the 19th century. Cullen had seen yellow fever, for instance, as a variety of typhus. For typhus itself bloodletting now became the cardinal remedy regardless of whether it was considered contagious or not.[172] Yet this remedy was used in combination with purging and cold affusion which Dickson for instance thought mutually assisted each other.[173]

James Johnson (1777-1845), a former naval surgeon who after the end of the long wars founded and edited the *Medico-Chirurgical Review* in London, had become by 1830 an international authority on tropical fevers; he was also physician extraordinary to the King.[174] His book on the *Influence of tropical climates on European constitutions* (1813) became in later editions a compilation of the current views - a new „Lind“. Its fourth edition (1827) was dedicated to the heads of the Medical Departments of the Army, the Navy and the East India Company. Johnson had favoured bloodletting for fevers since the first edition, where he had set himself against Lind’s and John Clark’s recommendations of purging and Peruvian bark on the basis of two unfavourable cases. In 1827 he wrote that the success of bloodletting depended in great manner on the judicious manner of employing it; and to an injudicious manner of using it was attributable not only its failure, but its disgrace. Despite their conviction, Johnson had no difficulty in theoretically explaining away the seemingly strange fact, „that the most climatically opposite plans have succeeded in fever, and been lauded to the skies by their supporters as infallible“, a statement which rightly applied also to the therapeutic nihilists. In accordance with Sir Gilbert Blane (see above, p.??), who was still the great figure in naval circles, Johnson was a therapeutic activist. He wrote that there was only very little doubt that under judicious modern measures, not only a greater proportion recovered from the graver types of fever, but that even more were prevented from suffering from the more dangerous forms than if left entirely in the hands of nature.[175]

Johnson himself had not brought forward any remarkable facts, except an intelligent plea for collection of accurate records by naval surgeons and especially their central analysis which alone would give the surgeons the impression that their efforts were worthwhile. The data on yellow fever of Lind, Robertson, Blane, Wright, Chisholm, Pym, Anderson and Fellowes on one side, and those of John Hunter of Jamaica, Jackson, Moseley, Bancroft, Lamprière, McLean and Burnett on the other, which had accumulated during the great wars, were still being augmented by military and naval surgeons afterwards.

Nodes Dickinson, for instance, (see above) was rather on the contagionist side. He held that the opinions on yellow fever had neither convinced those engaged in the discussion, nor those outside it, and that the discrepancy continued despite observations of the same facts. He designed a critical experiment which would prove whether yellow fever was really or only contingently contagious. But in practical terms he meanwhile recommended a mixed practice of bleeding, purging, warm and cold baths and cool drink.[176] Edward Doughty (t1824) was decidedly an anti-contagionist and proved numerically the superiority of Jackson’s bleeding over Chisholm’s mercury - not without praising McGrigor’s arrangements in Spain and the importance of the monthly returns.[177] Another Army surgeon, O’Halloran testified to the total success of this same mercury treatment.[178] Doughty had very personal reasons for his extreme position: convinced of Jackson’s method since 1802 [i.e. *before* Jackson had published any results.] he had been cashiered in 1811 because of a controversy with his superior at Cadiz, Fellowes, who was a contagionist and it was McGrigor who re-appointed him in 1812.[179]

Thus, from the point of view of my thesis the treatment of fevers between 1816 and1830 was rather like the continuation of a play, the text of which had been written already during the previous few decades. It is true, with McGrigor entering the stage, a new effort to enquire, by eliciting statistical returns, into the much debated origin and nature of the yellow fever in

1816 was made.[180] This he began to do as soon as he became head of the Army Medical Department. Yet, unlike the time of the Napoleonic Wars, the period afterwards was one of consolidation rather than of radical change. The fact of the methodological continuity is well worth pointing out, for it puts the alleged birth of clinical statistics in the 1830s in a new light. In the next section I shall further illustrate this continuity with another example, traditionally not uncommon in military medicine, namely the treatment of syphilis.

2. SYPHILIS

The limitations of this thesis prevent a development of the questions involved in the history of therapy of syphilis at any length.[181] I shall not discuss the issues of diagnosis (of which some contemporaries were perfectly aware[182]). For the present purpose, which is to show how a new therapeutic proposal was handled in the 1810s and 1820s by the leading British military doctors, I shall take the diagnosis for granted.

The generally accepted therapy for syphilis around 1800 was mercury. Yet, when they came to Portugal, British military surgeons were astonished to see that the disease was cured there by simple topical remedies and washings without this potentially dangerous drug, the side effects of which some of them feared as much as the complications of the disease itself. William Fergusson (1773-1846), who was head of the medical department of the Portuguese auxiliary forces, drew the attention of the medical community at home to this fact in a paper read to the Medico-Chirurgical Society of London in 1812. To make his case clear he gave a numerical example.[183] Though Fergusson himself thought that the disease was somewhat milder in Portugal than in Britain, this paper was not without effect in military circles.

Thus on the same 24th January 1812 tow further papers were read on the subject before the same society. The first, by Thomas Rose (born 1782), an Oxford graduate (M. A., 1803) and surgeon to the Coldstream Guards in London, reported on his observations among his own soldiers. He had generally adopted a conservative treatment, consisting chiefly in clean dressings, bark and/or antimony internally, after he had twice tried Fergusson's recommendations with success. The new plan had proved successful in all cases (which were admittedly not all venereal), 28 of which were reported in detail. They had been followed up long enough to be certain that they had been cured, and Rose could refer to successful results in upwards of sixty cases which had been observed by a colleague of his.[184]

These very sixty cases were analysed more precisely in the second paper, by James Guthrie, one of McGrigor's favourite pupils (see below). As in other military establishments Guthrie and his colleagues at Chelsea Hospital had been treating all types of ulcers of the penis (from January 1816 until January 1817) by simple, mild means. Not all of their patients could be followed up later, but out of „nearly a hundred", all had been healed without mercury. This fact of curability without the metal salt was thus established. But there remained still three questions: would this new cure be quicker? Would there be a bigger, smaller or an equal incidence of secondary symptoms? And of what severity would these be?[185]

In a preliminary attempt to shed light thereon Guthrie compared the incidences of secondary symptoms under the new treatment, as numerically reported from his own and the military establishments at Dover, Chatham, Edinburgh and various regiments in Britain and abroad, with his earlier recollections from Spain, France and Britain, where nearly all those cases had received mercury which had both yielded to simple treatment within a fortnight. The proportion of secondary cases in the former was less than 10%. In the latter group, Guthrie said that „the true average"would lie „between two or one in seventy-five". Guthrie's conclusion from these data (probably gathered with the help of McGrigor) was correctly deduced and illustrated his scientific mind: Mercury could often be dispensed with, but in severe cases it was the only reliable remedy. And he admitted that much more satisfactory information was still wanted, and much patient investigation to be gone through in the comparative treatment of these diseases, with and without mercury, „before we can arrive at any fair conclusion on a subject of such great importance".[186] As McGrigor had already bestowed much attention to it, Guthrie had every reason to think that much *would* be done in the course of the next few years. He pointed out, as had military surgeons before him, that they all, if well directed, possessed

„advantages as to ascertain facts which are not to be met with in any other walk in life. The persons affected are completely under [the surgeon's] control he can do what pleases with them under observation for a number of years, certain that they cannot have a change of opinion, and act contrary to his wishes."[187]

The trials at the hospital in Edinburgh castle were supervised by the surgeon in chief, John Thomson (see below) who was at the same time professor of military surgery at the

University.[188] He published them together with the results of a planned controlled study on 54 patients with gonorrhoea. These had been distributed into three groups: fifteen „controls“ were treated with „rest and abstinence“; twenty were given localised treatment of silver nitrate injections, and nineteen received three different internal medicines. All patients were „cured“, the first group after an average of 8½ days, the second after 17 $^{1}/_{3}$ days and in the last group only those eight patients who had taken a plant extract were discharged earlier than the controls, i.e. after an average of 5¼ days.[189]

John Hennen (1779-1828), another Edinburgh trained veteran of the Peninsular War, was appointed by McGrigor as inspector of the Scottish military hospitals in 1815. He encouraged similar investigations by his individual regimental surgeons, knowing that he fulfilled „the wishes of my respected chief.... when I solicit the inspection and opinions of medical practitioners, who are so well able to form a judgement“.[190] He was aware of not yet having collected a sufficient number of comparable facts, especially on the frequency and the nature of secondary complications when reporting, for practical reasons, the cases from one regiment only. Indeed, to give a comparative view from all hospitals he superintended would have led him into a multiplicity of details and calculations. Thus, although he recognised this as the true Baconian method, so far he had no time to do it.[191] One understands this remark, when one sees his „analytical view“ of these cases, extracted from a case book „kept with praiseworthy minuteness“. It consisted of eight tables, in which 105 primary and eleven secondary affections, treated without mercury, were separately broken down into subgroups according to whether they presented or not the „Hunterian“ characteristics of venereal diseases; they were further broken down according to their clinical features, i.e. ulcers only, and buboes succeeding ulcers. The time required for cure was tabulated for each subgroup; a special set of tables gave the maximum, minimum and average,i.e. the arithmetical means of all values of a subgroup expressed to two decimals. For the secondary cases the type of the complication and the interval until their onset were also given.[192] In conclusion, Hennen called for more research.

At this stage, McGrigor again stepped in actively. In December 1818 he sent a circular letter to all regimental surgeons with a series of queries, to which he expected numerical answers, concerning their experiences during the years 1816-1818. In April 1819 already, together with an assistant, he had analysed 1940 cases treated without, and 2827 with, mercury, and sent the results out to the surgeons. In the first group, mercury had become necessary in 65 cases for reasons he precisely classified. In the mercury group all patients had been cured, the time, however had lasted longer than in the non-mercury group, i.e.: 33 as compared to 21 days in the cases without bubo, 50 and 35 days in those with bubo, respectively. Secondary symptoms were scarcer, but more severe than in the non-mercury group, namely in 51 out of 2827 cases with mercury and in 96 out of 1940 cases without mercury, respectively.

McGrigor warned against the possible fallacy of such comparative estimations, as they were only averages, covering great differences within single regiments. He, too, stressed the necessity for prosecuting the inquiry, announcing that new results would be asked for again at the beginning of the following year. In reply to Charles bell's (1774-1842) criticism of the use of British soldiers for experimental purposes, and of the value of the military statistics presented so far,[193] McGrigor held that his investigation was meant

„strictly in that spirit of patience, liberality, candour and fidelity that ought to characterize the inquiries after truth - a spirit altogether remote from the precipitancy of innovation, the acrimony of disputants, or the stickles for any particular doctrine."[194]

He recommended keeping a particularly watchful eye on those patients treated without mercury, just as he made it absolutely clear that he did not enforce the non-mercurial treatment.

McGrigor's analysis was still included in an American compilation on the treatment of syphilis in 1830.[195] And John Hennen, too, included it in his *Principles of military surgery* (1829). In the section on syphilis he pointed out the results of comparative trials conducted during the same six months of 1818 and 1819 at the Edinburgh Castle Hospital, for they had yielded absolutely contrasting results. Hennen commented on this fact as follows:

„A prudent and unprejudiced practitioner knowing that mercury will agree with one set of patients, and disagree with another though their symptoms my be alike, and even contracted from the same source, will not draw hastily conclusions from either, but will wait patiently until, in the progress of events, the respective merits of those plans become more fully developed. Indeed, the numbers subjected to comparison are too limited to deduce from them any positive or fixed corollaries. "[196] [There had been 47 patients without and 18 with mercury in one trial, and 16 and 18, respectively in the other].

Therefore, he now analysed the results of all the hospitals he superintended in Scotland, from June 1812 to December 1819 [In 1820 Hennen became principal medical officer of the Mediterranean fleet.], comprising a total of 407 primary and 46 secondary affections that had been treated without mercury. On this basis he favoured non-mercurial treatment as a first choice, a choice he felt rested on a really sufficient basis, for „few men could have been more fortunate in their opportunities, and I assuredly am not conscious of having either abused or perverted them.“[197]

This short and necessarily selective survey shows that the numerical method was used for investigation into both the course and the therapy of syphilis by leading British military doctors in the 1810s and 1820. Conscious of the importance of mass observation they used their opportunities, not being deterred by the stringent organisational requirements. In therapeutics they seem to have appreciated the necessity of comparing comparable cases and the value of a „control“group. They calculated averages, yet they were also aware of potential fallacies. It is clear that they believed in gaining objectivity by the use of numbers. Indeed, for these doctors the numerical method seemed not only the best, but also the normal way of proceeding. It was non-contentious to them contrasting with the soul-searching thereon in the Paris debates of the mid-1830s. As Hennen wrote in 1818: „The care of syphilis without mercury is now under investigation in the military hospitals... with the same spirit of candid inquiry as the cures of any other disease by any new remedy proposed on respectable authority“. He thought that, because of the objectivity of his approach, his work would be profitable to medical science, though its specific results might become superseded.[198] Whether it was profitable to the patients is, for many reasons, difficult to determine. What can be said is that it helped to change the therapy many of them received, as had been the case when bloodletting had been re-launched for the therapy of fevers ten years earlier.

G. General Development and Conclusion

Since after 1815 the numerical methods continued to be used quite normally by the leading officers and the rising generation of doctors in the Army and in the Navy, it is to be expected that it also found a place in official medical teaching. The sole official chair of military surgery in Britain was that in Edinburgh, established in 1806. John Thomson (1765-1846)[199] a local graduate was the first to hold it. Having heard of the result of the battle of Waterloo, he travelled there, with McGrigor's assistance, and wrote a *Report* of his observations in the British military hospitals (1816). He included a lengthy, historical analysis of the question of amputation.[200] James Guthrie's important statistical work thereon (see chapter seven) weighed heavily in Thomson's conclusions, as suggested by a comparison of Thomson's remarks on him with those on Larrey, the famous surgeon of Napoleon's guard. Thomson, who had acquired the reputation as „the most learned physician in Scotland",[201] had chiefly analysed literary sources previously[202] but now he was impressed by the numerical method and started to use it himself. Thus, he organised a trial of the treatment of syphilis without mercury in his military hospital in Edinburgh. He also sponsored the publication of statistico-numerical papers reporting on the practice of specialised hospitals (e.g. on the Ophthalmic Hospital Chatham[203]). Later, he himself published statistics on the Scottish hospitals which included a special detailed chapter on the results of surgical operations.[204]

Upon his resignation in 1821, [He became later the first professor of pathology in Edinburgh. (1832)] Thomson was succeeded by (Sir) George Ballingall (1790-1855), a friend of Hennen. Ballingall had studied at St. Andrews and served as an Army surgeon from 1806 until 1818, during which time he had become a competent observer and a writer on military medicine. His *Observations fever, dysentery and liver complaints* (1818) centred entirely on his hospital returns and death list, collected from his passage to India in 1806 until 1814. They were arranged in numerous tables, and furnished the data for his calculation of comparative mortalities in percent.[205] As a teacher he wished to impress his students

„with the great importance of ...comprehensive views of hospital practice, convinced that it is by these that the practice of such establishments must be regulated and improved, rather than by the occasional publication of isolated cases, whether successful or otherwise."[206]

It is not difficult to guess that Ballingall's aim was to transfer the military practice of reporting and arithmetic analysing into civilian medical life, as he was at the same time a surgeon at the Royal Infirmary of Edinburgh. This may not have been without effect, for the essay which was awarded the first prize, set out by Ballingall in 1839, was entirely based on comparative numerical evaluation (of two techniques of amputation).[207]

In the introduction to Ballingall's major work, the *Outlines of military surgery* (1833) [The work went through five editions until 1855.] he was rightly aware that „while we have excelled the French in the administration of our regimental hospitals we have also.... been most successfully employed in the publication of official documents bearing on the medical statistics“.[208] Similarly, John Gideon Millingen, (1782-1862) a veteran of the Egyptian, Peninsular and Waterloo campaigns, laid great stress on the keeping of case-records at every level of the medical hierarchy of an army in his *Army medical officers' manual upon active service* (1819).

This booklet, dedicated to Sir James McGrigor with full recognition of the latter's merits, was entirely based and modelled upon the example of organisation the author had experienced in Spain.[209] Accurate records were needed, he emphasised, for giving precise information to the commander-in-chief and also to the inspector general (i.e. the highest medical officer of the army). For the purpose of science it was relevant that

„...... in this duty it must not be allowed to select such cases only as may be deemed important, as this undefined latitude would only lead to the omission of troublesome records, frequently interesting in the very ratio of their minuteness.[210] [Millingen used the numerical method later in his miscellaneous writings. See, for example, his essays on causes of insanity and on longevity of different professions in his *Curiosities of medical experience* (1837)]

A number of McGrigor's subordinates, past and present, continued to work along his lines after 1815. James Guthrie, whose important statistical work is dealt with in the chapter on amputation, became an (unpaid) lecturer on military medicine and a surgeon at Westminster Hospital (1838). Peninsular veteran surgeons like John Hennen (see above, Edward Doughty (see above), Henry Home Blackadder (t1830), Edward Luscombe (t1830) and John Boggie (born 1779) used statistics in their writings,[211] which were important for their time, and acknowledged McGrigor's influence.[212] 48 military and naval surgeons after 1815, or during home leave, wrote theses with the view of obtaining M.D. degrees based on their observations in the Peninsular war.[213] Hennen's *Principles of military surgery* (translated into German in 1820) was considered by Billroth in 1859 still „the most fundamental and reliable English military surgery“, uniting „rare understanding, unusual clarity of conception, enormous knowledge & experience - all the most English qualities“.[214]

McGrigor's post-Waterloo Medical Board became a mine of statistical data, from which individual workers could and did profit in their various ways.[215] The reporting duty extended to the whole Army in McGrigor's reforms led to a type of publication which included numerical results of mass observations. With circular letters McGrigor himself asked for numerical returns concerning specific diseases which he analysed statistically. And, as did Blane, he patronised the publication of especially well-designed reports.[216] Finally, these reforms were, of course, instrumental in creating the even more accurate and sophisticated official statistical reports on the health of the armed forces in the 1830s (which have been recently analysed by Cullen in his book on *The statistical movement in early Victorian Britain* (1975). Henry Marshall (1775-1851)[217] was first placed in charge of this work of publishing official statistical reports on the health of the armed forces. He was a Scot, trained in Glasgow, who had joined the Navy in 1803 before becoming Army surgeon in 1805. As such, he had participated in expeditions to Cape Town and Buenos Aires (1806-1808). The senior medical officer of the latter expedition, the Scot Theodore Gordon (†1843), had published a detailed tabular account of diseases and wounds - broken down according to diagnoses and event.[218] In his extensive post-Waterloo writing, Marshall had shown himself adept at such statistics, as well as at temperature charts and meteorological abstracts for which he had collected data during his duties in Ceylon, Edinburgh and Chatham,. He had also used some of the Army returns stored up in London. He was thus and obvious choice for the new enterprise of the official statistical reports.

An impressive forerunner to these widely published reports of the Government came into being through the sponsorship of the East India Company: in 1828 James Annesly (1780-1847) published two 750-page folio volumes of *Researches into the causes, nature, and treatment of the more prevalent diseases of India and of warm climates generally*.[re-edited and reprinted in 1829, 1831 and 1855.] Annesly, who had been stationed in India for 25 years, used in the first place his „daily and hourly reports of the state and treatment of any individual case" for the period from 1811 to 1824; these reports were „regularly preserved, and arranged, with suitable indices appended to them, for the convenience of reference". Then, he also included returns as received by the Company's medical service, and also some returns from the West Indies which were held by the Army Medical Board. These returns he broke down according to diagnosis. He mentioned how McGrigor had „very liberally"permitted their use as in the case of Burnett's study on tropical fever.[219]

I should like to conclude and at the same time to summarise this chapter to some extent by quoting from Annesly's preface:

„The Navy and Army Medical Officers have already contributed to the advancement of medical science: the results of their experience.... cannot fail to prove still further useful to the public service of the country and the community at large. It is to be hoped, therefore, that they will add to their valuable labours by continuing to furnish the results of their observations to the public. They have before them the examples of Pringle, Cleghorn, Jackson, Blane, and many others to emulate.... the Profession are much indebted to Sir James McGrigor, ... for the very active and useful part he has taken in encouraging medical literature in His Majesty's military service in all quarters of the globe...“[220]

These were some of the great names of observational medicine and also as indicated by this study, of arithmetical observation.

Pringle and Cleghorn had established the framework and taken some modest steps towards a programme of quantification. It was elaborated and used in the Army by Millar, Jackson, Chisholm and McGrigor. A respectable number of colleagues, simultaneously as well as subsequently, more or less tacitly accepted numerically stated results as the only basis for evaluating treatment. Admittedly, the facts were sometimes presented to fit preconceived theories, as is illustrated in the case of the cures of the Egyptian ophthalmia. Yet my survey on syphilis shows that there were also planned, impartial studies. Despite some criticisable aspects, and despite the difficulties arising form the imprecise disease entities of „fevers“ and syphilis (an imprecision recognised by contemporaries) some workers attained what we might label a certain degree of objectivity; even with our hindsight we might reasonably accredit them with a remarkably clear insight into elementary statistical methodology. The method, though crude, was therefore at hand *before* modern scientific medicine could differentiate diseases, „fevers“ for instance, into specific entities. And this seems to indicate that a complex pattern of motivations was at least as powerful a determinant in making therapeutic judgements as were the bare statistical facts, which could always be accordingly „twisted“.[221]

H. REFERENCES TO CHAPTER FIVE

[1] Cantlie 1974, pp. 273-281
[2] D.N.B. Vol.4, pp.481-482
[3] Cleghorn 1751, pp.vii-viii
[4] *ibid*., pp. 135,139-147,169
[5] *ibid*., p.137
[6] *ibid*., pp.197-198

[7] *ibid*., pp.199-200,217
[8] *ibid*., pp.119
[9] Underwood, 1977, pp.43,156
[10] Hillary 1759, pp.39,157,174,179
[11] Miller 1962: see also this thesis, pp.17-18
[12] Hillary 1761, pp.ix-xiii,23,36-37,61
[13] *ibid*., pp.37, 411-413
[14] Med.phil.Comment., 2nd decade, 5; 477-485, (1790)
[15] Singer 1949-1950
[16] Garrison 1924, p. 373; Blanco 1974, p.11; Underwood 1977, p.135
[17] Howard 1958
[18] Sand 1948, p.96; Selwyn 1966
[19] Underwood 1977, p.135
[20] Pringle 1752, p.ix
[21] *ibid*., pp.v-vi
[22] Ranby 1744, pp.4-5; see also chapter six of this thesis, p.??
[23] Pringle 1752, p.viii; see also pp.290-333
[24] *ibid*., pp.15,36-40,142-150
[25] *ibid*., pp.21-22
[26] *ibid*., pp.ix-x
[27] *ibid*., pp.142-150
[28] *ibid*., p.x
[29] *ibid*., pp.185,188,191,278,317,328
[30] *ibid*., pp.15,212
[31] Quoted by Singer 1949-1950, p.244
[32] Boerhaave 1744, p.211 (Aphorismus 977)
[33] Quoted by Singer 1949-1950, p.245
[34] *ibid*., pp.245
[35] *ibid*., pp.127,240-242
[36] *ibid*., pp.127
[37] Selwyn 1966, p.272
[38] Quoted in Pringle 1742, p.218
[39] Phil. Trans. Roy. Soc.Lond., 50; 205-227, 383-392, (1757); Singer 1949-1950, p.146
[40] Quoted by Singer 1949-1950, p.245
[41] Quoted by Selwyn 1966, p.268
[42] Selwyn 1966, p.271
[43] Phil.Trans.roy.Soc.Lond., 68; 180-193, (1778)
[44] Underwood 1977, p.134
[45] Hume 1942; Enders 1964
[46] Home 1759, pp.1-120, 270-288
[47] Brocklesby 1764, p.27
[48] *ibid*., pp. 48,122-125,231,310-320
[49] Baldinger 1765, pp.22-23, 106-110,265
[50] Wright St.Clair 1964, p.63
[51] Monro 1764, pp.397-398;1780 Vol.1, p.146
[52] Monro 1780 Vol.1, pp.160-218
[53] *ibid*., Vol.2, pp.320-340
[54] L.&C. Vol.3, pp.101-103
[55] Hume 1778, pp.196,205-209,228-229,238
[56] *ibid*., pp.242-245; reprinted in L.C. Vol. 3, p.107
[57] Rollo 1780, pp.1-10,16-27
[58] Rollo 1801, pp.81-89, 122-128
[59] *ibid*., pp.172-173
[60] *ibid*., p.80
[61] *ibid*., pp.84-85,123
[62] Garrison and Morton p.344, No. 3930; Anderson 1965
[63] Blanco 1974, p.43
[64] Hunter 1788, pp.viii-ix, 321
[65] *ibid*., pp.39-75
[66] *ibid*., pp.321-322

[67] Moseley 1789, pp.237-238
[68] *ibid*., pp.iv-v
[69] *ibid*., pp.271
[70] *ibid*., pp.vi,xiv
[71] Blanco 1974, pp.38-39,43-44; Johnston's Roll p.54, Bo.946
[72] Reide 1793, pp.95-96; Johnston's roll p.65, No.1104
[73] *ibid*., see e.g. p.161
[74] *ibid*., pp.xiv-xv, 39-49,191-197,220,228
[75] Millar 1798, pp.17-24,81-84
[76] *ibid*.; Johnston's Roll p.40, No.729
[77] Marshall 1833, pp.36-37
[78] Reide 1793, pp.xi,xii-xiii
[79] Millar 1777, pp.14-17,26-27,34
[80] *ibid*., pp.20-21
[81] *ibid*., pp.29-30
[82] Millar 1770, pp.231-256
[83] Millar 1778-1779, p.2
[84] *ibid*., pp.103-104,104-105
[85] *ibid*., pp. 106, 141
[86] *ibid*., pp.107,110-112,138-141
[87] *ibid*., pp.150-152,158,175-176; See also Singer 1949-1950, pp.245-246 on the Pringle Haen issue
[88] Pringle 1776, pp.5,34,35
[89] Millar 1778-1779, pp.188-200,226-227,305
[90] *ibid*., pp.208-209,231-232,295
[91] Millar 1798, p.75
[92] Cantlie 1974, pp.143, 151-156
[93] Millar 1783, p.41
[94] Millar 1798, pp.8-11,63-73
[95] *ibid*, p.80
[96] *ibid*., p.76
[97] Black 1789, p.ii; L.&C. Vol. 3, pp.334, 387; Abraham 1933, pp.128, 152-154; Hunt 1972, pp.15,65,108; D.N.B. Vol. 13, p.403
[98] Reprinted in Rollo 1801, pp.122-128
[99] Clark 1801, pp.vi, voo-viii
[100] *ibid*., p.v,139
[101] *ibid*., pp.19-22,24-26,139-240
[102] Blanco 1974
[103] Gray 1952, p.315; Blanco 1974,p.6
[104] Blanco 1974,pp.62-64,68
[105] *ibid*., pp.74-76,80-83
[106] McGrigor 1810
[107] Blanco 1974, pp.113-114,117,119
[108] Quoted by Blanco *ibid*., pp.122,126
[109] Quoted by Blanco *ibid*., p.127
[110] Blanco *ibid*., pp.125-126,128-129,136-139; Guthrie 1815,p.xi-xii
[111] See reprints of Blane's papers in Blane 1833, Vol.1, pp.135,218,233-234,245-248
[112] Cullen 1975,p.45
[113] Niebyl 1977,pp.475-476
[114] Sutton 1806,pp.16-17
[115] Wilson 1812; Dickson 1816
[116] Edinb.med.surg.J. 9; 53-58, (1813)
[117] Niebyl 1977,p.472
[118] Blanco 1974,p.53
[119] Crummer 1922;Cantlie 1972
[120] Blanco 1974,p.53
[121] Jackson 1791, pp.213-218
[122] *ibid*., 218,389
[123] Dickinson 1819, p.69
[124] Chisholm 1795, p.166; see also this thesis p.320
[125] Chisholm 1795, p.xv

[126] *ibid*., pp.76-78,102,144,158-159
[127] Chisholm 182, pp.8-10,206,208,212
[128] Bryce 1796, pp.7,88-94,97
[129] Jackson 1798,pp.294-297
[130] Jackson 1808, p.80
[131] *ibid*., pp.81-82,86-88
[132] Trans.med.Soc.Lond. 1; 196, (1817)
[133] Jackson 1817, pp.281-282,284-295
[134] Laws 1955; Cantlie 1974, pp.287-289
[135] Jackson 1808, preface, p.88; 1817, p.283
[136] Chisholm 1801, Vol.1, p.133
[137] Lemprière 1799, pp.xi, 323-339
[138] *ibid*., p.261, Tables I-IV, VI-VIII
[139] Lemprière 1812, pp.76-77: L.&C. Vol.3, p.175
[140] Wright 1828
[141] reprinted in Wright, *ibid*., pp.95-109
[142] *ibid*., p.373
[143] *ibid*., pp.122,143-147
[144] D.N.B. Vol.1, pp.1026-1027
[145] Bancroft 1820, pp.59-61,76-77
[146] Anderson 1798, p.15
[147] Blanco 1974,p.190
[148] Pym 1848, pp.26-27,106
[149] Niebyl 1977, pp.467-471
[150] Anderson 1798, pp.2,28-29,31-34,36-37,40; Pym 1848, pp.102-106
[151] Wade 1791, pp.ix-x; James Clark 1797, pp.v,37
[152] D.N.B. Vol. 12,p.645
[153] McLean 1817-1818, Vol.2, p.501
[154] McLean 1818, p.xxiii
[155] McLean 1817-1818, Vol.2, pp. 500-504; see also 1818, pp.xxiii-xxiv
[156] McLean 1817-1818, Vol.2, pp.432-454;1818, p.xxvi
[157] McLean 1817-1818, Vol.2, pp.504,507
[158] Fellowes 1815, pp.327,340,373,389,390,406,450
[159] *ibid*., p.154
[160] Burnett 1816, pp.232-242; L.&C., Vol.4, pp.3-4
[161] Burnett 1816,pp.38,287,345-348,359-361,400-402
[162] Cullen 1975, p.50
[163] D.N.B. Vol.3, pp.414-415
[164] Cantlie 1974, p.273
[165] Vetch 1807, p.69; 1818, p.20
[166] Cantlie 1974, pp.275-281
[167] Ackerknecht 1948
[168] Parsons 1978, Pelling 1978, p.18
[169] See Ackerknecht 1948
[170] Pelling 1978, pp.17-18,27
[171] *ibid*., p.25, see also this thesis pp.??
[172] *ibid*., pp.14,19
[173] Quoted by Doughty 1816, p.35; see also *ibid*., p.40
[174] Ackerknecht 1948, p.576
[175] Johnson 1813, pp.40-42; 1827, p.38,44-46
[176] Dickinson 1819, pp.vii-viii, 104,151
[177] Doughty 1816, pp.1,8,14-15,32,45-46,231-232
[178] O'Halloran 1824, pp.vii-viii
[179] Doughty 1816, pp.viii,xi,8
[180] See Fergusson 1817
[181] See e.g. Temkin 1955
[182] See e.g. Fergusson 1813; Rose 1814
[183] Fergusson 1813,p.3
[184] Rose 1817, pp.354,358-361,363-415
[185] Guthrie 1817, pp.168-170

[186] *ibid*., pp.170-173,180-183,187-188,192
[187] *ibid*., pp.180,192-193
[188] Thomson 1818 A
[189] Thomson 1818 B
[190] Hennen 1818, p.201
[191] *ibid*., pp.202-203,207
[192] *ibid*., pp.328,333-335,356
[193] Bell 1818, pp.16-18
[194] Quoted in Desruelles 1830, pp.202-203
[195] *ibid*., pp.195-203
[196] Hennen 1829, pp.544-545
[197] *ibid*., p.551
[198] Hennen 1818 A, p.203
[199] Comrie 1932, Vol.2, pp.504-506
[200] Thomson 1816, pp.159-281
[201] Quoted by Billroth 1933, p.66
[202] See Thomson 1808,1813
[203] Edinb.med.surg.J. 17;349-375,495-509, (1821)
[204] Thomson 1843-1844
[205] Ballingall 1818, pp.15-17,23-24,149,153-165,257-261,295-300, Appendix V
[206] Ballingall 1827, p.3
[207] Machardy 1841
[208] Ballingall 1852, p.8
[209] Millingen 1819,pp.iii-v, 72-79
[210] *ibid*., p.75
[211] Blackadder 1818; Luscombe 1820; Boggie 1848 (1st ed. 1826)
[212] Doughty 1816, p.xi; Luscombe 1820, p.7; Hennen 1829, pp.v-vi
[213] Crowe, Ph.D. thesis in preparation
[214] Billroth 1933, p.67
[215] See e.g. Burnett, Blane and Annesly in this thesis pp.320 and 337 respectively; Marshall 1833; also described by Blanco 1970
[216] See e.g. Fergusson 1817; Edinb.med.surg.J. 17; 1-29, (1821)
[217] Blanco 1970
[218] Laws 1955; Cantlie 1974, pp.287-289
[219] Annesly 1828, Vol.1, pp.viii,xvi, 111-133, Appendix III, Vol.2, Appendices No.1 (p.i), 2, (p.xxxii), 3 (p.xlvi)
[220] *ibid*., Vol.1, pp.xv-xvi
[221] See discussions e.g. by Temkin 1955; Ackerknecht 1962

CHAPTER SIX: LITHOTOMY

A. INTRODUCTION

1. GENERAL REMARKS ON SURGERY

In the three foregoing chapters I have attempted to show how enlightened individuals used favourable institutional settings, civil and military, for the statistical evaluation of internal therapy. It should also be clear that such an institutional background was not absolutely necessary, but that accurate book-keeping by an individual doctor could yield valuable results, too. In the following two chapters I shall examine to what extent the numerical approach was used for the assessment of the value of two surgical operations, lithotomy and amputation, in the 18th and early 19th centuries.

The emphasis will lie less on the institutions, the doctors involved being actually illustrations of what has been said already about their respective fields of activity, than on the scientific justification underlying the introduction of new surgical techniques. The eighteenth century saw indeed several important innovations. Around 1700 knowledge about the operation of lithotomy, having stagnated for 1500 years, started to move and grow through work done in Paris and London (see below). Later in the 18th century Jean-Louis Petit (1674-1750), was the first attempting to cure rather than simply to remove cancer of the breasts. He thought that „the roots of a cancer were the enlarged lymphatic glands, that the glands should be looked for and removed and that the pectorial fascia and even some fibres of the muscle itself should be dissected away rather than ...[leaving] doubtful tissue. The mammary gland, too... should not be cut into during the operation.“[1] In the 1750s an equally important contribution was made in Paris by Jacques Daviel (1696-1762), who introduced the technique of extracting the cataract rather than merely displacing it as had been done hitherto in various ways.[2] Similarly Jean-René Sigault (born ca. 1750) recommended the section of the *symphysis pubis* instead of the traditional caesarean section (see above p.??). As a result of the War of the Austrian Succession (1742-48) the exact timing of amputation of the limbs for gunshot wounds was discussed, whereas at the end of the Seven Years’ War (1756-63) the value of this operation became doubtful. Yet in the 1760s and 1770s British civil surgeons revolutionised amputation technique.

Thus from a present-day point of view the history of surgical operations between 1700 and 1830 would appear to have afforded ample opportunity for the use of comparative statistics. *A priori* this might be expected for two further reasons. First, an external disease was more easily and objectively diagnosable by 18th century standards than most internal ailments: surgical pathology existed well before Morgagni.[3] Second, and operator and even his testimonies could see what he did and which were the consequences of his therapeutic activity, the parameters being simply cure, failure or death. In fact, Hirschberg has shown in a specific chapter of his standard *Geschichte der Augenheilkunde* that the history of statistics of operations for cataract started with Daviel's presentation of his new operation to the Académie des Sciences in Paris [Underpinned by 182 successes out of 206 cases, operated partly in the kind of private hospital he had at his house, between 1750 and 1752.] and the examination of his cases by a correspondent of the Académie.[4] Similarly Fasbender mentions in his *Geschichte der Geburtshilfe* the early rejection of Sigault's operation for its higher mortality than that of the caesarean section.[5]

As case studies I have chosen samples of the history of lithotomy and amputation, partly because their technical improvements started earlier, partly because there were major British contributions to these fields in the period embraced by this thesis. In addition amputation of the limbs was considered the most frequent of the great operations of pre-antiseptic times in both civilian and military practice, next to it coming the surgical treatment of bladder stone, necessitated by the still partially unexplained high incidence of the stone disease in these times.[6]

For the sake of chronology, I shall discuss lithotomy in this, and amputation in the following chapter. The conclusion of the latter will embrace general aspects of both these surgical chapters. It must be noted also, that sidelights on the development of both issues in continental countries will have to be more frequent than in the preceding chapters.

2. Lithotomy up to 1700

Bladder stones were much more common in ancient and early-modern periods than in our times, probably due to a poor and unvaried diet containing many impurities, to chronic uncured inflammations of the urinary organs, as well as other factors,[7] not understood at the time and still defying accurate historical explanation. The oldest recorded operation for a stone was indeed that described by Celsus. A rather harsh procedure, it was only possible when the stone was big so that it could be felt and pulled down *per rectum* into the perineum. Once localised under the skin in the previously dilated bulbous part of the urethra a cut was made directly on it and it was extracted. Interestingly, Celsus remarked that his operation was to be used only in boys who were not older than fourteen years and only in springtime. These restrictions were the subject of much discussion when the operation of lithotomy was gradually taken over by professional surgeons in the 18th century. Up to that time it was clear that in adult men, because of the size of the prostate, the technique of pressing the stone and the bladder into the perineum was fraught with danger: the operation on adults fell within the sphere of keen itinerant lithotomists. Possibly they kept as family secrets some modifications allowing for a certain success. But for the 1500 years after Celsus wrote, no other method was described.

The tradesman-like aspect of the history of this operation was interrupted only by two renaissance surgeons, Mariano Santo (born 1488) and Pierre Franco (around 1550) who published methods for cutting through the prostate into the neck of the bladder, methods from which those practised in the 17th and 18th centuries evolved. Franco is particularly remarkable, for he also published a successful case of suprapubic lithotomy. However he thought that it had been accidental and urged his readers *not* to use this method.[8]

Although lithotomy was far more complicated than amputation and needed more knowledge of anatomy and more sophistication in surgical techniques, the chief lithotomists from the mid-16th century well into the 18th were a branch of self-made wandering stone cutters like the French Colot family, the young Friar Jaques (1651-1714) and Rau (1668-1719), Pauloni (around 1680), Collot (t1706), Pajola (1741-1816) and others. Especially with the appearance of Friar Jaques amongst the established surgeons in Paris in 1697, and with the achievements of John Douglas (t1743) and William Cheselden (1688-1752) in London, the operation acquired the semblance of a scientific procedure in the first third of the 18th century: all three brought about a radical change by abolishing the old Hippocratic belief that a wound in a „membraneous organ“, i.e. the bladder in our case, was necessarily fatal. They actually convinced their contemporaries that cutting directly into the bladder either by the suprapubic or the perineal route was a much quicker, more convenient, and more successful way of extracting a stone in both sexes and at all ages than trying to pull it through an incision in a painfully dilated urethra. A further major change in the surgical treatment of bladder stones occurred one hundred years later, when those operations were challenged by a radically new method practised by a new type of medical specialist: the intravesicular mechanical crushing of the stone, or lithotripsy, of Civiale, and Leroy in Paris in the 1820s and 1830s. This challenge provided occasions for great discussions on the theoretical value of statistics applied to medical questions.

It cannot be my concern in the present work to go into the detailed modifications proposed for the Jaques-Cheselden type and for the suprapubic operation during the 18th century. At the end of it, Vicq d'Azyr remarked that there were so many of them that „the history of the science and its nomenclature are harder to understand than the science itself".[9] Wangensteen *et al* have lately published a thorough and readily accessible digest of these accomplishments reflecting important lessons of pre-Listerian progress in wound-management. I shall discuss instead the kind of evidence on which the two most important changes in the early 18th and 19th centuries were based. But lithotomy being, unlike amputation, essentially an operation of civil life, the development of this question will provide greater insight into civil surgery. It may be borne in mind, too that lithotomy was a voluntary operation, performed upon the wish of the patient himself for relief of intense pain, in opposition to an amputation which was prescribed by a medic allegedly in order to save life.

The persistent severe pain of the bladder stone, and the fact that a number of those operated on for it survived might have made the sufferer willing to chance an operation. „Cutting for the stone" may be regarded as the first elective operation to relieve pain; a patient would be the more willing, the greater the alleged chances of survival were. What the strolling „specialists", who wanted to stay in business, needed therefore, was a reputation for success. It is difficult to say whether it was because of their connection with the market-place and thence a trend to boast themselves, but it is a matter of fact that itinerant lithotomists claimed their successes in terms of straight-forward figures. Wangensteen *et al* (1969) who have scanned the literature extensively were able to compile results of five of the more famous 17th and early 18th century continental lithotomists and to calculate mortality-rates from this data. This is a period for which results of amputation are hardly available at all.

In order to appreciate the background upon which Jaques's contemporaries evaluated his operation I shall briefly consider the conditions under which the quack lithotomists - as indeed the young Jaques Beaulieu was himself - operated in the 17th century. In June 1664 a man named Raous arrived in Paris from the Languedoc „where he boasted to have done great exploits“. He claimed in public to have cut more than eighty people in Bordeaux. But all the expert surgeons of Paris who listened to him could not provide him with enough patients, who would believe in his promises. He had the opportunity to cut only nine, two of whom „were well cut... in the others the operation either badly performed or performed without necessity“.[10] A certain François Tolet, surgeon and lithotomis attached to King Louis XIV, wrote in this *Traité* on lithotomy in 1708 that during a four month's trip to the Netherlands in 1693 he had cut 57 patients and lost six.[11] François Collot, who died in 1706 leaving behind a description of the technique fiercely guarded by his family during eight generations of service as French court lithotomists, only described some of his more noteworthy examples of both success and failure. [Wangensteen *et al* (1969) counted 40 operations with 7 deaths and calculated a mortality of 17.5%]. He did not present them in any systematic form or analyse them numerically, in contrast with the ravages of the Marian operation at La Charité to which his posthumous editor aptly drew attention. From the registers there it was apparent that in 1725 fifteen patients had died out of twenty, and in 1726 of „many operated there were many whom death has relieved from their pains“.[12] Although these data were merely occasional remarks hidden among hundreds of pages of technical and clinical details, they set standards of success.

As from 1681 the Hôtel-Dieu had some associated trained for lithotomy, from whom one or more were annually designated as operators in spring, during the „season“. In 1792 two such surgeons received a small gratification from the First President of the Paris Parliament for having lost only eighteen out of 104 lithotomies.[13]

Thus, even before the knowledge about lithotomy started to grow around 1700, this operation was associated in publications and discussions with numerical data and arguments thereon.

B. INNOVATIONS AROUND 1700

1. FRIAR JAQUES BEAULIEU

Let me now consider how the technique of cutting in the neck of the bladder first unconsciously put into practice by Jaques de Beaulieu was dealt with by the trained masters of the art in Paris. The story still excited attention by 1800 (see below). It was best told in

English by John Bell in 1815,[14] which is in itself a reflection of the growing importance laid by then on the kind of evidence at the basis of a therapy.

Before this singular man arrived in the French capital in August 1697 he had learnt his art from an Italian quack, with whom he had travelled for six years. At 27 Beaulieu left this „master“, returned to France and started practising those operations which he had hitherto only assisted at. In travelling from city to city, and from province to province, he acquired numerous certificates of success. These were hardly ever refused by friends of the patients who witnessed only the singular dexterity with which he extracted the stone. But as Bell wrote:

„…had he waited till time of cure arrived, the magistrates of cities would not have testified his success with so much enthusiasm. He snatched at these certificates of success, with the greedy precipitation of quack, and often, as in Paris while exhibiting his testimonials and boasting of his cures, letters arrived declaring that his patients were all dead.“[15]

A classical illustration of the difference between short-term and long-term effects!

When he was forty years old, in 1690 or 1691, Beaulieu is said to have resolved to devote his life to works of charity. He became a member of a tertiary order, and dressed like a monk. As such he arrived in Paris with letters of recommendation from a successfully-treated clergyman of his native Besançon, addressed to a canon of Notre-Dame in Paris. In August 1697, with the aid of Georges Maréchal (1658-1736), the first surgeon to La Charité, he introduced himself at this hospital and through his ecclesiastical relations he found also a political protector in the First President of the Parliament of Paris. It was this nobleman who raised the question of Beaulieu’s admission as lithotomist to the Hôtel-Dieu; this was resisted by its house-surgeons, although (or because) Friar Jaques (as he now called himself) had come to Paris „... with the sole design of teaching a new and particular manner of cutting for the stone“.[16] This is not surprising considering the fact that in this time an excess of qualified lithotomists was reported among the hospital *compagnons*.[17] On the 7th December 1797 the President commanded Jean Méry (1645-1722), surgeon of the late Queen and of the Hôtel des Invalides, and anatomist of the Académie des Sciences to attend, at the Hôtel-Dieu, an experiment by Friar Jaques, in which a corpse would be cut for the stone, the stone being introduced previously by hand into the bladder. Méry’s first report on this operation, delivered two days later to the President, started with the assertion „that the way of operating by Friar Jaques appears to me to be much more advantageous.... than that in common use“.[18]

But the physicians and surgeons of the Hôtel-Dieu and the established lithotomists immediately required new and strict trials of Jaques’s abilities as an operator. One week after the President’s first order Méry received a second one. This time, Jaques performed two experiments upon a woman and a boy. Upon inspection Méry declared that he had severely lacerated the anatomical structures of the pelvis. Surely, these were due to the Friar’s ignorance of anatomy. But the fact that Méry also condemned in his second report the principles, and the theoretical advantages of the operation that he had recommended so warmly a week before, truly suggests that, as bell stated, „hurried away by the torrent of professional jealousy, [he] was found, acting as the instrument of a malicious party... and, in but a little while, we find him deputed to make a public harangue in the name of the professed enemies of Frère Jaques“.[19] Indeed the old lithotomist, afraid of losing their practice to younger practitioners of the new technique, decided to strive back. The welcome occasion came in April 1698 when a general assembly of all administrators of the Hôtel-Dieu, ist physicians and master-surgeons was called for under the auspices of Monsieur Méry to discuss the matter. In the debate Méry now tried hard to demolish the theoretical basis of the new operation but was fair enough to state his own observations numerically. He declared that it was in the „public interest to know, which [method] is the one with less accidents to fear and after which one sees a greater number of patients recovered to health“. Out of eight patients operated on recently by Jaques in Paris, two were dead two days after the operation, one had an opening of the intestine [He died in September 1698.] and a lady had a wound in the vagina. Of the four others, he had no news. Jaques’s friends did not contend these facts. But they reminded the assembly of Jaques’s well attested successes in the provinces while the few failures he had had in his eight operations in Paris were not enough to condemn the method. Consequently it was agreed that further experiments should be done by Jaques at the Hôtel-Dieu.[20]

Spring was considered the appropriate season for cutting for the stone and Jaques operated on 42 patients at the Hôtel-Dieu, eighteen at La Charité and on a „great many patients privately“ during the next six weeks. Out of his sixty (or 62) publicly operated patients 25 died, Méry being always on hand to do the autopsies by order of the President. But out of 22 cut by the ancient method and by other lithotomists, only three were lost by the end of July 1698:

„Therefore“, wrote Méry, „it is visible by the comparison of the success... that... [the operation] of Friar Jaques is much less advantageous than that of the other lithotomists“. And Méry strengthened his conclusion by mentioning the frequent occurrence of fistula and urinary incontinence after Jaques's technique.[21] [In addition, of the 37 who had escaped alive from Friar Jaques's operation only 13 were cured perfectly. The other 24 remained with incontinence, fistulas, and all with great extenuation, symptoms not reported after the old technique in the „majority“ of cases].

However, as precise as figures may appear, they may also be distorted when they are not backed by complete or tabular details for each case. That such tricks were not only used by itinerant operators for the sake of propaganda, but also by hospital surgeons for their own aims is illustrated by the comment of an English visitor to Paris in the summer of 1698:

„Frère Jaques' reputation mightily slackens, for of 45 cut in the Hôtel-Dieu, but sixteen (!) survive; and of nineteen cut in La Charité only eleven survive: but I am sensible that he has got abundance of enemies, which makes me very often question what I hear; the surgeons have a great mind to shout down this man while they practise his method.“[22]

By then Jaques „wearied and dispirited by unremitting persecution, and affected by the death of so many of his patients“, had left Paris. In early July 1698 he was in Orléans where out of eight operations for the stone he had four deaths and fistulas by 13th September 1698, but at least eleven of his operations for hernia (by castration, even in children!) were cured.[23] Since in the provinces, the name of „taille“ (=cutting) was used for both operations, it is possible that Jaques's reputation was partly nourished from this ambiguity, and partly also, surely, from the fact that he operated *gratis*. On the 28th of July 1798 he arrived at Aachen where he performed about 200 operations [The exact number was difficult to establish one year after Jaques's stay.] in four weeks. The year afterwards a seriously elaborated certificate by a town councillor testified that there had been *only* four deaths by then.[24]

In Paris, Guy-Crescent Fagon (1638-1717), the first physician to the King, who was himself afflicted with a stone, remained impressed by Jaques's manual dexterity. As he was convinced, too , of the advantages of his lateral perineal incision, which surgeons with a proper anatomical training like Maréchal in Paris, and Rau in Holland had started using with apparent success, Fagon recalled Jaques to the Court at Versailles without losing one.[25] This certificate was the key to his astonishing success throughout Europe thereafter. But in Paris itself he was once more unlucky for he lost a high ranking aristocratic patient.[26]

Thus Jaques became again a wandering lithotomist although he was much honoured outside of Paris even by the German emperor and the Pope. He operated no longer for profit but only for charity. This did not hinder him from generously publishing his results. In an anonymous memoir in his defence he claimed in 1702 to have operated on 4500 patients within thirty years. „Of hundreds of healthy subjects cut after this improved method.... none had died or remained fistulous...“, he said.[27]

Johann Jakob Rau (1668-1719), probably Jaques’s imitator and surely his German rival and counterpart before becoming settled as professor of anatomy at Leyden, claimed to have performed 1547 lithotomies without a death [And yet he would not reveal his secret, deceiving even his pupil and assistant Bernardus S.Albinus (1697-1765).]. These figures set some standards as is shown by the fact that subsequent generations tried to emulate them with their own figures: Ferhius, a Swiss physician, said in 1716 „that of sixteen lately cut by Frère Jaques in Strasburgh, only one died, and that was an old man, whose death was predicted from his age and weakness“. Petrus Camper (1722-1789) commented in 1762 on Rau’s boast:

„We will concede the number if it is a question of treatment and not of cures; he [Rau] passes in silence the matter of deaths. However, the record, which by order of the city Fathers preserves to this day the operations for stone in the Surgical College, discloses that of twenty-two on whom he operated at Amsterdam, there were four deaths.“[28]

The trustworthily certified results of friar Jaques at Versailles, Aachen, Amsterdam and Strasburgh had not been able to overcome his lack of prestige and reputation of adventurousness among the Paris professionals striving, at that time, for the recognition of their own privileges. His operation became recognised with them only thirty years later, when one of them, Morand, re-imported it from England. But it is now time to consider how Morand’s British teacher, Cheselden came to revive and improve Jaques’s technique.

2. William Cheselden

The name of William Cheselden (1688-1752) is closely linked with the operation of „lateral“ lithotomy

„which for over a hundred years was one of the most common and most successful operations in surgery... Close enquiry however shows that perfection was only gradually attained after trials of several techniques, about each of which he was for a time enthusiastic, only to give it up when he found out a better way."[29]

In his biography of Cheselden (1953), Cope summarises the passages, concisely related by Cheselden himself, of how he relied on numerical observations for changing his techniques.

When Cheselden tried to make his way in London in the 1710s, John Douglas (t1743), the brother of the better known anatomist James (1675-1742) made in 1719-21 what seemed to be a most successful revival of Franco’s suprapubic lithotomy in search of a replacement of the too cumbersome operation of Mariano Santo called by then „apparatus major“, or „grand apparatus“. Douglas was successful in three out of four cases, upon which he became staff surgeon to the Westminster Infirmary and F.R.S. and was given the freedom of the City of London. But Cheselden soon surpassed Douglas, with eight successful (private) operations out of nine, performed between May and October 1722, which he lost no time in publishing.[30] He had already been on the staff of St. Thomas’s Hospital since 1718 but was not allowed to cut there before 1724-25. He resumed the suprapubic way, but his enthusiasm for this operation soon faded:

„Cutting nine with success it came again in vogue... but the peritoneum being often cut or burst from injecting too much water... . What the success of the several operations was [i.e. of those done by all staff surgeons] I will not take liberty to publish; but for my own, exclusive of the two mentioned before, I lost no more than one in seven, which is more than anyone else I know of could say. Whereas in the old way, even at Paris, from a fair calculation of about 800 patients, it appears that more than two in seven died. [This was probably a reference to the results of the Hôtel-Dieu between 1720-1727, later published by Morand (see below). Morand visited Cheselden in 1729.] And though this [i.e. the suprapubic] operation came into universal discredit, I must declare... that it is much better than the old way, to which they all returned, except myself, who should not have left... it... but for the hopes I had of a better.“[31]

Cheselden's idea was to fill the bladder pre-operatively with water through a catheter, as in the suprapubic approach, and then to cut it through the perineum according to Jaques and Rau, of whose success he had heard. He began this new technique in August 1725 but lost four out of ten patients by 1726. This made him slightly alter his technique. In 1732 he first gave an account of his new results in an 'Appendix' to the fourth edition of his *Anatomy*:

„The first twenty seven patients cut this way recovered, and I believe are all living at this time: Indeed I had cut thirty one who recovered before one died, having cut four more between the 28th was cut, and the time he died; but I scorn to use any fallacious way of representing my success. Some of these being cut in the hospital, and some privately, the truth of this account my be suspected by those who do not know me. I cannot take the liberty to mention the names of private patients, therefore I will give a detail of those only which I cut this way in the hospital, where the first twenty five recovered, to the truth of everyone of which I had above twenty witnesses, and I do believe these patients are all living at this time."[32]

Considering the somewhat irregular past of lithotomy, one is not astonished to see Cheselden going to considerable trouble to assure the truthfulness of his results. He gave a list of all these 46 patients, operated between March 1727 and July 1730, with their ages and dates of operation. Only two had died by 1732, even though „many" among the at least 32 children under fifteen years had had smallpox during their recovery.[33] Cheselden continued to keep accurate records of his pubic practice, for in the next edition of his *Anatomy* (1740) we read:

„What success I have had in my private practice I have kept no account of, because I had no intention to publish it, that not being sufficiently witnessed. Publickly in St. Thomas's hospital I have cut two hundred and thirteen; of the first fifty, only three died; of the second fifty, three; of the third fifty, eight, and of the last sixty-three, six."

This did not include deaths from smallpox during the cure. If the mortality had increased somewhat, this was because in the last series „the operation being in high request, even the most aged and most miserable cases expected to be saved by it". In order to evaluate and to advance lithotomy Cheselden considered the ages of those who recovered and those who died „of most consequence to be known". He grouped those of his 213 patients according to decades and gave the number of deaths for each decade in the text.[34]

I have drawn up the following table from Cheselden's figures

Age/Years	10 or under	11-20	21-30	31-40	41-50	51-60	61-70	71-80	Total
Cut	105	62	12	10	10	7	5	2	213
Dead	3	4	3	2	2	4	1	1	20

This list shows again, that Cheselden's great success was in children. Practically half of his patients had been ten years or younger, with a mortality of one in 34. The mortality of the remaining had been one in 6.3. But this detail was not noticed for a whole century. The overall result was taken as a measure of successful lithotomy for the rest of the 18th century; for instance it was republished unaltered long after Cheselden's death, e.g. in the thirteenth edition of his *Anatomy* in 1792, or by John Thomson in Edinburgh in 1808. Joseph C. Carpue considered them as more trustworthy than Rau's in his comparison of methods in 1819 (see below).[35]

Although not of direct concern for this thesis, Cheselden's conclusive remark upon these results deserved mention:

„If I have any reputation in this way, I have earn'd it dearly, for no one ever endured more anxiety and sickness before an operation, yet from the time I began to operate, all uneasiness ceased; and if I have had better success than some others, I do not impute it to more knowledge, but to the happiness of a mind that was never ruffled or disconcerted, and a hand that never trembled during any operation."[36]

C. The Influence of Cheselden in 18th Century Europe

1. Sauveur-François Morand; The Rotating Platform in Paris

Cheselden's fame spread quickly. It reaches Paris even before he had time to publish his results in 1730. It is worth following how his operation was introduced into the French capital that had so severely censured its principal originator, Friar Jaques, thirty years earlier. There is even an indirect link to Jaques, for this reintroduction was chiefly the work of Sauveur-François Morand (1697-1773), a son-in-law of Maréchal, whom the Friar had taught (see above).

Like Cheselden, Morand changed his technique several times. Convinced that every method had some good points and that a good surgeon could not only execute them, but should also choose the appropriate one,[37] he wrote up review-descriptions of all of them. He started in 1728 with the suprapubic method, generally tried and much discussed since its recent revival by Douglas, Cheselden and a handful of other English authors. These authors had reported all their cases in some detail and Morand, whose book consisted in part of translations of their writings, included these cases.[38]

But furthermore, in an attempt to show the advantages of this method, he added them up (31 patients, 5 deaths) and compared the results obtained between 1720 and 1727 with those of the established methods from the Hôtel-Dieu and La Charité, which he obtained from their administrative records. He arranged these results in tabular form for each year: there were a total of 812 patients operated on, 255 of whom had died. From this comparison, the advantages of the suprapubic method were obvious. Morand concluded his lecture at the Académie des Sciences in Paris: „Everyone who subjects himself to the high [i.e. suprapubic] apparatus exposes his life less than with the grand [apparatus].“[39]

This was an unprecedented instance of comparison of data from a new method with a vast number of results from an older one. But it must be stated that the whole revival of the suprapubic operation was due to Douglas's four original cases. [Douglas complained in a letter to the Paris anatomist Winslow that despite his success, his operation was not accepted everywhere. He asked for an opportunity to demonstrate it in a Paris hospital. Jacob B. Winslow (1669-1760) thereupon started experiments on dead bodies with a surgeon of the Hôtel-Dieu, who however soon died. Thus the matter had remained open until Morand's book in 1728.[40]] And, four cases were again enough to convince Morand of a potentially even better method. For bearing in mind his belief in the value of surgical methods, he set out next to describe the lateral operation. Having heard of Cheselden's discontinuation of the suprapubic technique in order to test the lateral and then to compare them, he asked the Académie des Sciences to support him for an journey to England (which it did).[41] Thus, in

May 1729, he saw Cheselden operate on four patients, three of whom were cured. Together with the questions he asked the patients and the conversations he had with Cheselden this „gave me light which meditation might never have provided me with and the courage to undertake this operation“. He kept on friendly terms with Cheselden, who still in 1729 wrote him letters with detailed descriptions of his method, including his numerical results.

March 1727	June 1729:	47 (hospital & private patients, 4 deaths)
March 1727	July 1730:	46 (hospital only, 2 deaths)
July 1730	End 1730:	20 (hospital only, 2 deaths)

(He also sent them to another surgeon in Paris.)[42]

Impressed by Cheselden’s figures which he had seen in London, Morand, back in Paris, started experimenting on corpses he proposed the „new“ operation to Maréchal, his father-in-law and by then first surgeon to King Louis XV, who showed interest. Under his supervision, Morand and a colleague started operating at La Charité and in town during the next „cutting season“ in Spring 1730. They reported two deaths out of sixteen patients.[43] His superiors, and even the Académie des Sciences who saw eleven of the cases, were highly pleased, especially as during the same time five patients out of twelve died at La Charité with the old Marian method. But in early 1731, the same misfortune happened to Morand that had ruined Jaques’s reputation in Paris thirty years earlier: he lost two prominent patients[44]. However, Morand who was well established in surgical circles could defend himself better than a newcomer and layman had been able to, and autopsies „proved“ in addition that the deaths had been unrelated to the operation.[45] The editor of the *Histoire de l’Académie Royale des Sciences* recalled the argument forwarded by Jaques’s friends in 1698: This incident was not interesting in itself, but was of value for the public

„to whom it is important that a good operation does not fall into discredit, because, as it is practically inevitable, some misfortune in particularly conspicuous circumstances had happened to it, which always induces the jealous to take advantage.“[46]

In a ‘Mémoire’ read before the Académie des Sciences in 1731 Morand was able to refute all allegations brought against him - especially the one of having selected easy cases for testing the new operation. The simple announcement of his results from La Charité justified him in his eyes „and we flatter ourselves that henceforth we shall need to use no other means against the critic“. But nevertheless his best point, the trump card of his lecture, was Cheselden’s results brought up to date:

„Theory will furnish a great number [of motives for the re-introduction of this operation], but the sole ones capable of persuading are the facts. Examining the operations performed according to this method since the month of March 1727... everything must inspire us with confidence. I have just received Mr. Cheselden’s list of patients cut since the one printed in his Appendix [1732] and I learn that he has [since] cut twenty, two of whom are dead.“

Morand rounded off his lecture with a set of precise figures:

„If we join this list to his first and to ours, we find, once the calculations are made, altogether 82 persons were cut according to this method within four years, of whom only six have died and 76 have been perfectly cured.“

This success was even greater than that of the suprapubic method.[47]

Thereupon, Morand, already surgeon in chief at La Charité since 1730, and one of the founders of the Académie de Chirurgie in 1731, became, as a lithotomist, a key figure in the dissemination of Cheselden’s technique, personally introducing it to at least thirteen continental surgeons. He was proud to publish their results in 1772, one year before his death, as they had reported them to him. If we add up those testimonies which are fairly precise, we obtain a total of more than 300 operations with 30 deaths, i.e. the lethality was below 10%, not including Cheselden’s results which were re-published with details in a separate chapter. But Morand listed this time also the original sixteen Parisian cases of 1730 with names and ages.[48]

Morand’s factual argumentation seems to have won more ground for his technique in the provinces than in Paris. He did not quote one Parisian surgeon in his list of pupils who sent him their results. Morand’s foremost pupil in France was Claude Nicolas Le Cat (1700-1768). He reported in 1772 that Le Cat died having performed 310 operations, but did not state the exact number of deaths.]Le Cat attributed this omission of his master to feudal differences between clans in the capital.[49] Henri François Le Dran (1685-1770), another famous 18th

century French surgeon and a representative of the old Paris surgical establishment, particularly defended the *apparatus major* in 1730. He adduced his own results in 1728-1729 at La Charité and referred to the returns both of this hospital and the Hôtel-Dieu, but for different years than those which Morand had used (i.e. 1727-1729 instead of 1720-1727). But these were the results of only one method, showing nothing more than that one did not necessarily die from its application. And results by themselves were not important to him, since they depended essentially on the ability of the surgeon.

„It is by the comparison of circumstances, and thereby only, that the value of each of the methods must be judged; it is neither by the successes, nor by the strangest consequences that one must determine oneself.“[50]

Morand and Le Dran in the late 1720s stressed the preponderance of the surgeon's skill over the method. Yet Morand, himself convinced by numerical comparison of the results, at times seemed to think that the method was at least as important for it is patent that he took over from Cheselden not only two operative techniques for trial, but also the practice of keeping notes, of reporting comparative results and of using them as a basis for argumentation. This was a personal achievement of Cheselden's and of some of his colleagues, since it was to be testified repeatedly later that there was no official register of the operations performed at St. Thomas's Hospital in his time nor in fact as late as 1819.[51] If we now consider the rest of the 18th and early 19th centuries up to the appearance of lithotripsy in 1824, it is apparent that this tradition did not die out. But it did not evolve either, and remained based on personal initiative.

2. The Proliferation of Technical Modifications Up To 1800

Morand exaggerated when pretending in 1772 that the Cheselden operation, as introduced by him, was the only one in use in France.[52] Yet the lateral operation surely became more and more popular, as illustrated by a famous dispute which agitated the Académie de Chirurgie in Paris in the mid-1750s: it merely concerned priorities over its technical details and over special instruments.[53] It was led by Morand's controversial pupil, Le Cat, against Friar Cosme (Jean Baseilhac) (1703-1781)[54] and his invention, the „lithotome caché“. The Friar, of an old family of barber-surgeons in the Auvergne, was an outsider to the Paris surgical world; but he was plunged into it as a monk of the convent of the Feuillants. The *Académie de*

Chirurgie adjudicated the quarrel about the value of his instrument on entirely qualitative grounds, to his disfavour.

But Friar Cosme was an alert man. Despite his claims of success with the lateral method - as expressed in terms of mortality - he was quite aware of its drawbacks, especially as women's urinary incontinence was often the consequence of the incision into the neck of the bladder. From 1758 onwards he operated first on women, and after 1769 also on men according to a new procedure which needed no previous injection of liquid into the bladder. But due to his humility and his being on the defensive, he waited up to twenty years before publishing this improved method. By then (1779), he was able to supply a detailed list of all the 46 women and 36 men he had operated on.[55] He continued keeping a register right up to his death, which was published afterwards by his nephew[56]] and in which „... the facts reported“, wrote the friar, „favourable or unfavourable have been described immediately as they arrived“.[Altogether he had operated on 100 cases, 59 women (9 deaths) and 41 men (10 deaths)]. In his original report, he was able to announce that all the 39 women classed as cured were not only alive, but none were incontinent either.[57] He thus considered he had achieved his main aim, at least in women. His mortality of one sixth was comparable to that of Cheselden's record for the lateral operation. [If one considers the last 50 cases only, when the friar had acquired more skill, his mortality was comparable to that of Morand's pupils.] Clearly the publication of results of all cases based on actual and regularly kept registers as shown by the British in the 1720s had been continued by Friar Cosme, but it was still an exception used in a moment in re-launching a new operation.

Far more common was the association of lithotomy with numerical evidence of a more casual kind. For example, Wagensteen *et al* list the results of eight European surgeons of the 18th century besides Morand's pupils.[58] There were even more, e.g. Le Dran (1730), Earle (1793), Pascal Basheilac (1804) among the established surgeons,[59] and there still existed some itinerant lithotomists, too, whose fantastic claims were around like ghosts: Pajola, an Italian, who operated in Vienna in 1804 was reported not to have lost one patient in 500 operations whereas another author claimed that only three of his approximately '200 patients' had died.[60] The figures of the established surgeons as published in secondary literature were often contradictory, too, as they were quoted from vague testimonies or from memory. This prompted Jean François Deschamps (1740-1824) of Paris to say, in 1796, that

„a work that contains only the non-successes and the errors of the reputed practitioners would pergaps be one of the most instructive books; the art would gain one hundredfold what egotism [*amour-propre*] might lose.“[61]

Thus, in 1812, Guillaume Dupuytren (1777?-1835) in Paris used the early British results of suprapubic lithotomy, as added up by Morand, and those of Friar Cosme for a comparison of this technique with the lateral method, for which he did not give any numbers. To determine the advantages and drawbacks of all techniques of lithotomy he thought to be „the task, less brilliant than useful, which the last century has bequeathed on the century we are living in“. This view, presented in an application for a professorship, arose from the feeling that methods and proceedings had reached the possible limit of perfection and that there were too many of them.[62] Indeed, the author of *Modern Improvements in the practice of surgery* had written in 1780 already that however surgeons might generally agree on the lateral method of lithotomy, there were rarely two of them who operated in exactly the same manner or employed the very same kind of instruments. And in 1786 a table had been published in Paris which, once unfolded, presented a synopsis of 118 „original“, or „corrected“, procedures of lithotomy.[63]

One of these technical modifications consisted in the replacement of the scalpel by a cutting gorget by (Sir) Cesar Hawkins (1711-1786) in 1740. A pupil of John Ranby he became associated with the Royal family, and with St. George’s Hospital. He was a reputed practitioner, master of the new Company of Surgeons in 1748, and portrayed by Hogarth - yet he is not known to have published any literary work.[64] [I found, however, an occasional paper with Ranby in the Wellcome Medical Historical Library].

Benjamin Bell (1749-1806) whose leading *System of surgery* (see below) was in its seventh edition by 1801 contained a section on lithotomy of „a great length“of 145 pages, this being one of the most important operations. He described the Celsian, suprapubic, Jaques and Cheselden techniques in details, mentioning simply that the latter, with a few improvements, was now universally practised. Unavoidably, he found that whilst Hawkins’s idea of a cutting director designed to avoid accidental cutting into the rectum was laudable, the instrument itself had some disadvantages: „This inconvenience, however, is removed by the cutting director that I have ventured to recommend...“. Despite Bell’s insistence on having recommended „nothing that is not either at present very generally adopted, or that I have not myself put in practice“, no results were included in the whole chapter, with the exception of those of Jaques were included in the whole chapter, with the exception of those of Jaques in Paris (25 deaths out of 60; see above), wit a pejorative comment.[65]

Bell himself conceded that a detailed account of all the suggestions for improvement could not „serve any purpose, but to bring into view some particular modes of practice, which were either never generally followed, or which, if adopted, have fallen again into disuse“.[66] He did not change his section on lithotomy significantly for twenty years after the first edition of his *System* in 1783/84. This illustrates further that debates on lithotomy, since Cheselden's major step, were often storms in a teacup, as was well recognised by Deschamps, too. The French counterpart of Bell's *System*, Raphaël-Bienvenu Sabatier's (1732-1811) *Médecine opératoire* (1796) treated lithotomy in precisely the same way.

On the other hand, two among the greatest British surgeons active in the latter half of the 18th century, Percival Pott (1713-1788) and John Hunter did not, to my knowledge, write particularly upon bladder stone. The only time John Hunter mentioned lithotomy in a course of lectures on the principles and practice of surgery in 1785 was to explain the reasons of the surprising infrequency of postoperative inflammation.[67] Perhaps this reticence was partly due to the prevailing „consensus on diversity“ about the lateral method and its modifications. At least, as indicated above, some authors by the end of the century, showed some weariness with the endless discussions on technical details the fruitlessness of which they realised.

3. ANALYTICAL REVIEWS AROUND 1800

Out of a feeling for the need to evaluate, and to simplify a complex situation, a number of historical-comparative works were written in Europe around 1800.[68] Yet it would not appear that these works achieved these aims, for many ended again by recommending their author's own technique.

In 1793, to strengthen his case, (Sir) James Earle (1755-1817) a surgeon at St. Bartholomew's and son-in-law of Percival Pott, added recollections from his practice, admittedly from memory, (and yielding a mortality of 1 in 47!) as the best proof „of the contested safety of lithotomy“. In 1801, Nicolas Saucerotte (1741-1814) was proud of his new method by comparing his results with those in his specialised institution in northern France before he had introduced it.[69] [i.e. 194 cases with 10 deaths (1 in20) compared with 1435 cases with 137 deaths (1 in 10).]. In the same year, Christian Von Klein (1772-1825),[70] a forerunner of scientific surgery in Württemberg included the six unsuccessful cases with his method, finding a mere reporting of all successful ones „useless and irksome“. In 1816 and 1819 Klein consequently reported his results numerically.[71] In 1802 Treyeran would include some isolated, successful cases[72] and Konrad Johann Martin Langenbeck none.

Other writers, as illustrated by the above quotations of Deschamps (1796) and Dupuytren (1812) would stick to the traditional results of 18th century practice in their historical sections, but did not use them as a basis in their final arguments, especially as they themselves brought no new clinical material of their own into the discussion. This is true for John Thomson, the Edinburgh professor of military surgery (see above) who in 1808 reprinted Douglas, Le Dran and Cheselden to end up with his own proposal for a better method. In the same year, and also at Edinburgh, Robert Allan (1778-1826) a former naval surgeon published the same type of survey, extolling one method not with own, but with 18th century results (1808).

In the 1810s other examples were the writings of Charles Bell (see below)[73] and of Joseph Constantine Carpue (1764-1846), a London surgeon who had served as military hospital surgeon from 1799 to 1807. In a clearly structured work (1819) Carpue reprinted all the cases of the British suprapubic lithotomists of the 1720s, and, as had Allan, summarised their results numerically as well as those of Friar Cosme. As to more recent results with this method, he had only isolated cases to offer. Concerning the lateral method, he relied on Cheselden and Alexander Marcet (1770-1822), the chemical pathologist (see below), who had very recently published mortalities from lithotomy for quite different reasons.[74] Whilst reprinting Cheselden’s results, he realized that the latter had been more successful in children. However, Carpue did not even allude to these tables in the nine arguments which made him prefer the suprapubic operation as he had seen it in Paris with a nephew of Friar Cosme.[75]

Comparison of the procedures on a numerical basis again became a major scientific issue in lithiasis therapy after the description of a completely new technique, i.e. lithotripsy, in 1824. But before that, new results of operations were published by a group of British physicians who thought statistical inquiries the chief way „for finding the truth“ about the natural history of bladder stone.[76] This was recognised by 1820 as a valuable method even by a younger surgeon like Henry Earle (1789-1838), the son of Sir James, at Bartholomew’s.[77]

D. THE NORWICH SCHOOL OF LITHOTOMY

1. THE BACKGROUND

The important British work on lithiasis has always been associated with what Batty Shaw called the ‘Norwich School of Lithotomy’ (1970). This has come about for several reasons, the main being that Norfolk enjoyed the unenviable reputation, from the latter part of the 18th century, of having the highest incidence of bladder stone of any county in Great Britain.

„As a result of this high prevalence... a local tradition of surgical skill in the art of lithotomy emerged and when the first general hospital in Norfolk... was founded in 1771-2 there were appointed to its surgical staff local surgeons who were most experienced lithotomists. Their skill was passed on to those who followed them and earned for the hospital a European reputation for its standards of lithotomy.“[78]

On the early staff of the Norfolk and Norwich Hospital were also physicians interested in the medical aspects of bladder stone with particular reference to its incidence and chemical composition. Both for the credibility of their claims concerning the results of operations and their clinical research, their writings were based on hospital registers kept from the hospital’s inception. As Marcet wrote in 1817 the Hospital stood „in this and several other respects,... as a model of regularity and good management.“[79]

But not only were complete registers of all operations kept (including the name of the surgeon, the technique used and the „event“), but the Hospital also preserved all the bladder stones that were removed. Many a hospital at this time made a collection of its stones, as did individual surgeons, but the Norwich collection gained a special reputation, recognised even by Civiale in Paris. Both the registers and the stone collection have survived and were the basis of batty Shaw’s article, on which the general part of this section is based.

There were several specific roots for the exemplary keeping of patient’s registers in the Norwich and Norfolk Hospital, apart from simply administrative ones. In its civic records, Norwich possessed an account (covering the whole 17th century) of bladder stone patients treated by lithotomy. [These records survive in the Mayor’s Court Books of the City of Norwich and this account is thought to be unique in Great Britain].

The writings of Sir Thomas Browne (1605-1682) provided another source of information about stones in 17th century Norfolk. Furthermore, at least from 1704 Norwich had its own Bills of Mortality. Finally Benjamin Gooch (1708-1776), a regular correspondent of leading observationist surgeons such as William Hunter and Joseph Warner who also presented cases and communications to the Royal Society, played an important role as medical consultant in the foundation of the Norfolk and Norwich Hospital. He was a dedicated „observationist“. His textbook (1758, 1767) which I have mentioned (see chapter 2) has been considered one of the most important surgical works by a provincial surgeon of the 18th century.[80] In 1773, at the age of sixty-five, after the opening of the new hospital, he wrote a monograph purporting to be an Appendix to his former Cases and practical remarks. As had the latter, this manuscript too had been approved by Sir John Pringle. Gooch regretted that he himself had not made sufficient observations and had not regularly pursued what he now strongly recommended, namely to take not only minutes upon all extraordinary cases „as well unfavourable as favourable in their event“,[81] but also of epidemic diseases which the different seasons produce, with the state of weather at those times and the most successful of cures, „according to Huxham and Cleghorn.“[82]

Nonetheless it seems, from the example of Edward Rigby discussed above, that the scientific use of the accumulated records was restricted to the problem of bladder stone, although such records were kept for all admissions.

2. Matthew Dobson in the 18th Century

The first to make use of them was Matthew Dobson (1735-1785).[83] He was trained in Edinburgh and settled in Liverpool around 1760. A true clinical investigator he experimented on urine of diabetics and on the influence of heat on the body. He also undertook a now neglected numerical enquiry into the relative incidences of the stone in various regions of England and Wales, taken from the admission lists of fifteen provincial hospitals (1779). [It is neither mentioned in his biography by Dobson (1977), nor in Brickerton’s *Medical history of Liverpool* (1936), nor in an editorial in the JAMA (1968) which all stress Dobson’s original work on diabetes]. He described his survey thus:

„I have been at some trouble to collect a comparative view of the disposition to the stone in several parts of the kingdom. My design was, to ascertain the number of patients who had been cut for the stone, in several hospitals to which I applied, compared with the whole number of both in and out patients; and likewise whether there was any thing peculiar in the food, drink or situation of the inhabitants of the respective districts.“[84]

From the comparison of the figures obtained, Dobson safely concluded as to the unequal geographic occurrence of lithiasis.[85] But as to its patho-physiology he considered his thoughts as far from clear and conclusive. Rather he concluded with a sentence typical for „modern“scientists, namely with a plea for further investigation:

„Other physicians may make different comments, and draw different inferences from the above reports and a still more extensive collection of facts may produce also a more useful, satisfactory and decisive theory."“

The way for arriving at this was now obvious for him. Indeed, from his own experience, Dobson deplored the paucity of complete hospital records which he rated as a major scientific tool: „I cannot conclude“, he wrote in 1779

„without expressing a wish, that the hospital reports throughout the kingdom, were drawn up in a more full and circumstantial manner. The sources and nature of endemics, and of some other diseases also... might be thus more clearly ascertained, and a more successful method of cure consequently adopted.“[86]

This statement is less of a surprise to us when we learn that Dobson was at this time a friend of Haygarth of Chester[87] (where his work was actually printed). He regularly attended the meetings of the Warrington group. He thus was equally a friend of Percival and knew the work of Letttsom. It was Percival who proposed him first as a candidate for the Royal Society. (He became a Fellow in 1778). When moving to Bath for his retirement, Dobson again met Falconer (who became actually a witness to his will).[88]

Dobson’s paper on lithiasis was included in a monograph, in which he had collected evidence of the medical effects of „fixed air“(CO_2).It was published in 1779, 1785, and 1787. The originality of his contribution becomes the more striking if one compares it with the other reports of his correspondents, recounting mostly successful cures of a variety of diseases by fixed air. Among them was for instance his „truly amiable and ingenious friend“, Macbride, who wrote to him all the results of the trials with wort against scurvy, for he recognised that wort liberated fixed air when fermenting. On such an experimental basis, fixed air had also been proposed for the treatment of lithiasis by several physicians. There were experiments by Percival, Falconer and Dobson himself.[89] Such was the background for Dobson's enquiry. Methodologically this was an extension of the old correspondent system, (as used for instance by Pringle), from individuals to hospital. New, were the questionnaires and the numerical analysis, [Dobson calculated the *average* ratio of lithotomies to the total number of patients. They were as 1 to 394, in the cider districts of Gloucestershire, Worcestershire, Herefordshire and Exeter, and 1 to 420 in those of North-East England, but 1 to 3223 in North-West England.[90]] which occurs somewhat unsuspected in this booklet. Yet it can be understood if seen within the context of the interests of the Warrington Group. As others of its members Dobson saw the work with hospital returns quite generally as the appropriate means to achieve a better understanding of diseases and to ascertain the success of treatment.

3. The Years 1817-1823

Dobson's type of research was taken up in enlarged form forty years later, when a series of British statistical publications appeared at a rate of one per year between 1817 and 1823. Batty Shaw has analysed them from the medical and chemical point of view whereas I shall focus more on their data concerning operative mortality.

The first of these writings was by Alexander J.G. Marcet (1770-1822),[91] a Swiss born, Edinburgh trained physician and animal chemist at Guy's Hospital. His aim in his *Essay on the chemical and medical treatment of calculous disorders* (1817) was

„to describe, and illustrate... the characters by which the different calculi may be distinguished; to indicate the easiest analytical methods by which their chemical nature may be ascertained; and to point out the modes of medical treatment which afford the best prospect of success.“[92]

As for the latter purpose, there were according to him, good prospects for curing lithiasis medically in females and in fairly early cases, which could be diagnosed by chemical tests, in order to avoid the pain and danger of the formidable operation of lithotomy. He illustrated its awkward consequences by comprehensive statistics collected from 1772 until 1816 at the Norfolk and Norwich Hospital.[93]

Although he had been a physician at Guy’s since 1804, Marcet had been unable to find any regular or at least any ostensible records of cases of lithotomy in his own and the other large hospitals of London, such as St. Bartholomew’s, St. Thomas’s, and the London Hospital. Therefore he had travelled to Norwich for the purpose of inspecting the interesting documents kept there. In his analysis Marcet first put the number of deaths in relation to age, as Cheselden had done, whose results he quoted. He also broke them down according to sex, and presented them all in a table. The overall mortality of 506 cases was 70, or 1 in 7.25, which was illustrative enough in his eyes. [Broken down according to age, the mortality was 12 out of 227 male children under 14 years (1 in 19) and 56 out of 251 adult males (1 in 45). In females the figures were 1 in 8, and 1 in 20, respectively.]
Marcet made a plea for compiling Norwich-like data to find out the relative frequency of lithiasis in various periods and geographical areas. For this he needed only the total number of admissions and of lithotomies, but not their results. For recent years, he was able to procure, or more often to infer or estimate the former, from metropolitan and provincial hospitals in Britain and on the Continent. As for earlier periods he referred to Dobson’s statistical inquiry of 1779, but results of operations were not available. His work, conceived only as a commencement of a new type of investigation, allowed him nevertheless „to draw results from a larger scale of observations than was perhaps ever furnished ... and to present a point of comparison to which other records of the same kind may in future be referred“.[94]

This proved to be right in two ways, i.e. the clinico-pathological research and the gathering of operative results. In 1818, Hutchison, the former naval surgeon who had written statistically on amputation (see below, chapter 7), published his statistics on the relative infrequency of the calculous disorder in seamen (1818). In the same year Marcet’s book appeared in a German translation, and in 1819 in its second English edition, in which he commented very favourably on Hutchinson’s type of work, (which corresponded to his own).[95] It clearly exercised some influence, for in 1820 Richard Smith (1772-1843),[96] a remarkable senior surgeon at the Bristol Royal Infirmary, reacted to Marcet’s outcry about the lamentable paucity and incompleteness of hospital records.

In between Smith had taken the liberty of addressing a frank letter to every provincial hospital for information with the precise aim of drawing up statistical memoranda on the occurrence of lithiasis and the results of its surgical treatment. He received polite answers as to the numbers of operations from all of the more than thirty charitable institutions for the poor all over Britain to which he had written. The Dobson study served again as the source for 18th century data. Smith's most detailed account was of his own hospital, which had kept records since its opening in 1735, a fact which had escaped Marcet. Large tables gave information not only about the geographical provenance of its patients and the relative occurrence of lithiasis in different periods, but also on mortality according to age and sex of the 354 patients admitted over a total period of 82 years. Besides Marcet's indications from Norwich he also included precise data on mortality from the returns of Devon and Exeter, Birmingham City and Leeds. Thus more or less accidentally a „statistical" inquiry, by which was meant a numerical description of the „state" of things (rather than the modern idea of calculating probabilities), produced new data on the results of operations, too. Mortality was not a prime concern of the author, who in his conclusion did no mention it, but he was struck by the decreasing incidence of lithiasis in recent years, its unequal geographical distribution and its likely causes. [Overall mortality in Bristol was 1 in 4.5, in Leeds 1 in 7 (as in Norwich), but also in the age-dependent mortalities were greater in Bristol than in Norwich]. He was also greatly satisfied „to lay before the public a proof that those [charity] institutions are open to medical research upon proper application". And he hoped that in the future there would also be contributions from the great charities of the capital which hitherto had „set such an example of remissness in the non-preservation of chirurgical documents". Such exhortations must be seen as the forerunners of Wakley's campaign of making publicly known, in medical journals like the *Lancet*, what was going on in those hospitals (see above, chapter 3).[97]

In early 1821, William Prout (1785-1850),[98] an Edinburgh M.D., follower and friend of Marcet and another important animal chemist, published his *Inquiry* into the chemical analysis of stones and the chemical explanation of stone formation, which he had begun already in 1817. Prout mentioned the mortality data of Marcet and Smith. To the latter he referred in the following terms:

„Mr Smith's paper was published after most of the present volume was written; but for the sake of incorporating the valuable data it contained, the second chapter of this section was partly remodelled."[99]

Marcet’s and Smith’s mortality tables had not remained unnoticed among practising surgeons either. Samuel Cooper (1780-1848) included them immediately in the fourth edition of his *Dictionary of practical surgery* (1822)[100] (which, however, did not contain any such data on amputations). It is worth emphasising that, since he was a surgeon presumably not wishing to cast doubt on the value of lithotomy, he added the tables to the chapter on the natural history of urinary calculus, and not to that on lithotomy!

Philip Martineau (1752-1829), trained at Edinburgh was the senior surgeon to the Norfolk and Norwich Hospital, where he was closely associated with Rigby.[101] He reacted against an imputation hidden in Marcet’s and Smith’s results, namely the high overall mortality of lithotomy (one in 7 ¼) in his own hospital. One of the motivations for a paper he read to the London Medico-Chirurgical Society in May 1821 was clearly to show that *he* was not responsible for it... but that his colleagues were to blame.[102] In that paper Martineau attacked Carpue’s reasons for preferring the suprapubic operation because, he said, one was left in ignorance of an account of its success by its recent propagator in Paris. He saw no reason to resort to a new technique unless its success was demonstrated to be greater or it was shown to be technically easier. For his part, he provided a table containing names, ages, dates of operations, duration of recovery and weight of the stone for all 84 patients he had cut by his „improved“ lateral method from 1804 until 1820. Only two had died, he announced, and there had been no selection of patients „as I never rejected anyone who was brought to the operation“.[103] However, John green Crosse, his assistant and later his successor claimed exactly the contrary.[104] Yet Martineau was successful with his claims, for in 1835 Sir Astley cooper said of him that „no surgeon in London, I am certain, can boast of similar success at lithotomy“, and Jean Civiale in Paris called him „le lithotomiste le plus éminent et le plus heureux de son époque“.[105]

Since a French edition of Marcet’s book appeared in 1823 (and was promptly abstracted in a current French periodical[106]), Dupuytren and/or the other co-editors of Sabatier’s classic textbook on operative medicine (1824) had probably heard of Smith’s results from Bristol. At least it suited them to indicate - without reference - a mortality of „1 in 5 or even 1 in 4“ for Cheselden’s method (rather than 1:7 from Norwich or Leeds) or even Cheselden’s own result when they compared his method with the new French technique of recto-vesical lithotomy, to which they happened for the moment to adhere for „rational“ reasons, and which had resulted, in more than thirty separately published cases in France alone, in only two deaths since 1818.[107]

E. THE APPEARANCE OF LITHOTRITY IN 1824

1. JEAN CIVIALE IN PARIS

Such was the background of which Jean Civiale (1792-1867)[108] was aware[109] when he submitted the first results of his preparatory work on lithotrity, i.e. on the intravesicular destruction of the stone by mechanical means, for judgement to the Académie des Sciences in Paris in January 1824. The history of the treatment of lithiasis in France in the next thirty years corresponds to battles between the lithotomists and the lithotritists on the one hand, and among the lithotritists themselves (about the priority of invention) on the other hand. These battles were chiefly fought with the statistical weapon. Indeed the topic would merit a separate detailed study, for so far as France was concerned, it was important for the general recognition of statistics as a method for evaluating therapy, and for recognising the rules for its correct use and its limitations. The issues prompted two formal discussions on the value of statistics as applied to medicine in both the Académie de Médecine and the Académie des Sciences in the mid-1830s and again in the former throughout 1847.[110]

2. THE TREATMENT OF BLADDER STONE AFTER 1824 IN BRITAIN

In Britain, the problem of therapy for lithiasis developed somewhat differently than in France. Since the works of Marcet, Hutchison, Smith and Prout, discussion on lithotomy had acquired an additional numerical dimension in this country even before the invention of lithotrity. Indeed, for the first time non-lithotomists had published results of lithotomy which they had extracted themselves from hospital records. Together with the work on the natural history of lithiasis (Hutchison 1830) non-specialists' analyses of mortalities continued throughout the 1820s and early 1830s. Not astonishingly, perhaps, their results showed mortalities much higher than those previously published by lithotomist-surgeons. The surgeons' discussion of lithotomy and of lithotrity became partly a reaction modelled upon these publications and escaped their long-lasting entrenchment in merely technical debates.

Considering first the physician-chemists, in 1821 there was a sober debate between Prout and John Yelloly (1774-1842) about calculations of average mortalities.[111] Yelloly then continued Marcet's work. A graduate of Edinburgh (M.D. 1799) he had moved to London (where he had joined Lettsom's Medical Society, and later the Medico-Chirurgical Society). He had become associated with the London Fever Hospital, and published the results of treatment of fever in his wards at the London Hospital (see above, chapter 3). In 1818 he resigned from his London appointments, settled at Norwich and became from 1820 until 1832 physician to the Norwich and Norfolk Hospital. In 1828 and 1829 Yelloly read two papers on lithiasis to the Royal Society in which the Norwich material was used again, and which completed the earlier analytical work. In these papers, operative mortality was for the first time not only correlated with age and sex, but also with the size of the calculus. Besides, Yelloly indicated that the whole number of Martineau's operation at the Hospital had been 147 with seventeen deaths, the mortality then becoming one in eight, rather than the pretended one in 42 (see above). [There had already been a slight undertone of incredulity about Martineau's results in Prout's contribution to the calculation of operative mortalities (1821)]. Furthermore he analysed Cheselden's famous list of 213 cases. Besides noticing that Cheselden had included only the results of his improved lateral method, he pointed out that his outstanding success had been limited to children, and that he had counted all those who had died from smallpox during convalescence as successful.[112] At the same time Yelloly mentioned the impossibility of Rau's results in a letter to Sir Astley Cooper.[113]

The transition between the physicians' predominantly „natural history“ approach to the stone problem and the surgeon' more practical one is marked by Yelloly's senior surgical colleague at the Norwich Hospital, John Green Crosse (1790-1850).[114] He had *not* studied at Edinburgh, but in London, Dublin and Paris before settling in Norwich. In 1828 Crosse succeeded Martineau, whose manual skill he greatly admired. That was perhaps why he was polite when commenting on the latter's statistics of the 84 patients mentioned above:

„It is singular, and worthy to be noticed, that the next two patients publicly operated on by him, at the Norfolk.... Hospital, both died; which is enough to abash the surgeon who should presume upon success according to his experience [only].“[115]

Crosse tried to live up to this early experience, and indeed, in 1833 the classified collection of calculi and the records of the Norwich Hospital served, once again, as a basis for an important paper: it was Crosse’s contribution to the topic announced by the Royal College of Surgeons of England (London) for the Jacksonian-Prize of 1833, i.e. „The formation, constituents, and extraction of the urinary calculus“(1835). He represented a mood of his time when summarising his monograph (for which he actually received the prize):

„To boast of uniform success in any capital operation, is not the dignified course of a surgeon, any more than that the physician should quack [!] of universal cures. Experience in lithotomy, like victory in battle, is seldom gained, without counting a certain number of slain.“[116]

His tabular appendix was the best proof that he had acted upon his principles. The first three tables listed all 704 cases operated on since the foundation of the institution in 1772 until 1833, with data that had become standard with the more methodical British workers since Marcet i.e. age, result, number of days between the operation and either cure or death, and the weight of the stone (Table I). He calculated mortalities for females and males (Table II), arranged in decennial periods of age (Table III) and according to the weight of the stone. He had eight groups of stones (Table IV), the largest group being sub-divided into eight further subgroups (Table V): Moreover, he broke down the 704 operations according to the months of the year (Table IX). Since there had been 93 deaths in the whole series, Crosse brought them up to a hundred with cases from his private practice to obtain percentages. (Tables VI, VII, X arranged like Tables I, and IV and IX.) He also included a table of twelve relapsing cases with the interval before relapse and the weight of the calculi, (Table VIII).[117]

If one overlooks some wrongly calculated (or misprinted) percentages, Crosse’s figures were a nice illustration of the age-dependency of mortality. They showed, too, as Malgaigne would do later in France (see above, chapter 1), the erroneousness of the old belief that spring and autumn were the best seasons for the operations. Crosse’s monograph represented the highest standards of his time in terms of credibility and presentation. In 1839 even the French defender of lithotomy, Alfred Velpeau (1795-1867) recognised his work and that of Marcet, Smith and Prout as the most reliable.[118] However, in our eyes, Crosse’s comment on the inconclusiveness of Martineau’s data must apply also to his own break-down of his 204 cases into many small groups.

As to the publications of other English surgeons, they were in the 1820s subject to the same criticism as those mentioned in the case of Martineau. Some mortality figures attributed to a

Mr Green, apparently a well known lithotomist at St. Thomas's Hospital, London, allegedly based on „faithful and authentic reports", were promptly denied by the *Lancet*. The journal quoted information by Dr Green himself on about forty cases, „the precise number he does not know".[119] The most incredible rumours circulated about the fatality of this operation in those years. Incidentally the editor of the *Lancet* was drawn into court for having aggressively criticised a fatal lithotomy performed by the nephew of Sir Astley Cooper. (The operation had lasted 55 minutes but the body, upon autopsy, had not been found different from any other fatal case...) In connection with this case, results of an Austrian (334 operations, 31 deaths), and a Dublin professor, (more than 100 operations, no failure) and of an "average rate of failure in Britain" [2 deaths in 15 cases (source: Marcet?)] were cited as standards without any details.[120]

A group of Scottish hospital surgeons started publishing their results of lithotomy in the late 1820s, too. They went back in their private recollections as far as 1792 (Crichton 1826) or 1821 (Liston 1828) or indefinitely (Syme 1830). But their countryman Alexander Miller sharply criticised these accounts, which showed mortalities of one in eight, one in fourteen, or even „one ever", throughout a long career. Their lack of authenticity and reliability became patent when one analysed them closely and compared them with other publications of the same authors. Miller fully agreed with Yelloly's analysis fo Cheselden's and Martineau's data and greeted the chemist-physician's work as outstanding. The Edinburgh surgical establishment did not like this little monograph, and its author was compelled to compose another one in order to become a member of their Royal College of Surgeons.[121]

3. The Introduction of Lithotrity Into Britain

The introduction of lithotrity into Britain began on the 24th July 1829, when Baron Charles-Louis S. Heurteloup (1793-1864), one of its French inventors, visited London, showed his instruments and operated publicly. A British pupil of Civiale, William Costello (1800-1867) arrived only a few days later and operated on the 1st August.[122] The *Lancet* was the English arena in which the Paris priority-quarrel went immedicately on stage between Heurteloup's protector, D.O. Edwards, a young resident surgeon at the Westminster Hospital[123] and Costello. This word-fighting lasted four years until August 1833 despite Wakely's attempt to bring it to an end a year earlier.[124] At least, the histories - or tales - about this invention would have escaped nobody by then! In a first comparison of the two methods later in 1829, William

Lawrence (1783-1862) of St. Bartholomew's Hospital was quick to realise that numerical comparison of results would be inadequate, since only the cases for lithotrity were selected.[125]

Thomas King (1802-1839), one of the many British physicians who visited Paris wrote in 1832 favourably on lithotrity on purely qualitative grounds. Alexander Miller too, had been in Paris, but he had gained an opposite personal impression. In support of his view he quoted Larrey's rightly reprobatory report to the Académie des Sciences in 1831 of Civiale's first account of his ward at the Hôpital Necker: and abstract of it had just been published in the *Lancet*.[126] (Upon personal inspection, Larrey had realized that Civiale had not reported on *all* his cases). Heurteloup published his results in the *Lancet* not without being challenged by British colleagues.[127]

Lithotrity remained in Britain largely in the hands of Paris-trained specialists. In Norwich, for instance, it was tried twice in 1832 and 1834, but not again until 1855.[128] The first Scot to use lithotrity was William Keith (1803-1871) the Paris trained surgeon of the Royal Infirmary of Aberdeen, in 1840. His 'Hospital statistics of stone in the bladder' (1844) were quite characteristic of the British lithotomy/lithotrity situation of the time, as well as of the spreading fashion for comparative hospital statistics, used overwhelmingly to illustrate institutions' operative mortalities.[129] Keith was convinced that

„Whatever the amount of an individual's success might be, yet that the testimony of a person practising one branch exclusively could aid the profession very little in rightly estimating the relative value of the different modes by which stone in the bladder is to be cured."[130]

Thus he had waited five years exactly in order to have enough material for fair *comparison* and safe induction. By then he had compiled 39 cases, sixteen lithotrities (one death) and thirteen lithotomies (one death). Every case occurring in the hospital during this period was, for reasons of credibility, described in great detail, going much further than Yelloly had gone. Comparative tables for cases of lithotomy and of lithotrity were drawn. Yet, because lithotrity was always his first choice in the most favourable cases of lithiasis Keith presented statistics not as a basis for the selection of a proper technique in a given case but to show that both lithotrity and lithotomy offered the doctor and the patient a safe, and therefore fair, alternative in slight cases of lithiasis. As with other surgeons it must be stated that Keith, too, used statistics to support his own technical contributions to both procedures: His mortalities gave him the edge over the pretentions of Dupuytren who had boasted with a much higher one as a proof of success of his „new“ method.[131]

F. Conclusion

In conclusion, numerical comparisons of results from British hospitals played a considerable role in the introduction of Cheselden’s new surgical procedure for bladder stone therapy in the 1720s. Subsequently his statistics were recognised as the only reliable standards of success in Europe, until a new set of data was published around 1820, chiefly from the records of the Norwich Hospital. These latter were in turn accepted even by leading French lithotomists *and* lithotrotists.

Despite Dobson’s original statistical study on the occurrence of lithiasis and his appeal to use correct hospital records to solve clinical questions there was thus a gap of critical analysis of nearly 100 years. Indeed, Cheselden had recognised the possible influence of the age and sex of the patient might have on his chance of recovery from the operation. Yet, after him, surgeons became chiefly interested in details of (their own) operative technique. Results were, if at all, expressed simply in terms of overall mortality and some leading British surgeons of the time did not participate in the debates thereon.

Dobson was not a surgeon. He was a member of the informal Warrington Group which centred on Percival, Haygarth and Charles White. Like his colleagues he was interested in epidemiological aspects, in the natural history, of his topic. He used their technique of sending out questionnaires and of arithmetical analysis of the answers for the elucidation. But unlike theirs, Dobson’s work found no immediate continuation.

There were, around 1800, attempts to bring neatness and order into the jumble of over eighty published methods of lithotomy. Yet attempts of critical evaluation were illusory as long as there was no quest for reliable new results from hospitals. With reference to Dobson, such data were again published in the late 1810s and early 1820s. Only then was it again realised, by some neutral, unspecialized physicians with more interest in the natural history of a surgical disease than in specific surgical techniques, that the age and other parameters might be more important in evaluating its therapy than the hitherto much discussed technique of operation; there was no clear-cut yes-or-no for the choice of any method on the basis of overall mortalities. Thus their analysis of data like e.g. those from Norwich, was much more sophisticated. The greater numbers of data, the recording of the time between operation and recovery or death, and the statistical relation of the weight of the stone and of the seasons to operative mortality, were the new features in the works of the 1820s, Cheselden having already grouped his results according to age and sex of the patients. New was also the epidemiological approach introduced by Dobson in the 1780s (when it also was applied to questions of midwifery). The handling of these new data allowed British physicians to appreciate the nature and limitations of statistics concerning results of operations even before the introduction of lithotrity into this country.

From such evidence as I have given in this chapter, I would tend to accept the opinion on the specialist lithotomist and lithotritist of the Scottish surgeon, Alexander Miller, in 1831.

„.....In some measure [the specialist] still holds his ground, - at times itinerant and strictly empirical, and though brought within the pale of the profession, always affecting mystery and concealment of method; and above all, persevering in endeavours to prove that his operations are uniformly successful.“[132]

In such instances, the odd unhappy case was forgotten or denied to have had any connection with the operation. It was „natural for these men, claiming as they did the merit of discoverers, not only to hold up the fair side of the question to the public“. If they were lithotomists they endeavoured „to prove their operation to be simple, of easy execution, causing no great suffering and above all, [to be] successful“, and the lithotritists attempted „to throw lithotomy into the shade and to induce the substitution of their supposed improved operation for it“.[133]

Perhaps because so much personal antagonism was involved, the discussions on the evaluation of two methods launched, for example, by the appearance of lithotrity in 1824, quickly and publicly emphasised the complexity of numerical evaluation of results in clinical medicine. Finally, by no means least, this case-study illustrates strikingly that the *unbiased* compilation and use of simple but valuable statistics at all times depended on the physician not cheating to suit his own ends. Thus the worth of such statistics was as much due to individual temperament and moral integrity, qualities which in my opinion Cheselden particularly possessed, as to organisational skill and willingness to rely on sheer numbers.

G. REFERENCES TO CHAPTER SIX

[1] Power 1934, p.5
[2] Hirschberg 1908, pp.470-516
[3] Temkin 1951, p.254
[4] Hirschberg 1908, pp.484-496,499;1912, pp.269-277
[5] Fasbender 1906, pp.869-870
[6] Billroth 1933, p.47; Cartwright 1967, p.13
[7] Shelley 1958, p.50
[8] Wangensteen et al. 1969
[9] Quoted *ibid*., p.932
[10] Méry 1700, pp.10-11
[11] Tolet 1708, pp.255-256,262
[12] Collot 1727, pp.xlii, 315
[13] Gelfand 1973 A, pp.78-79
[14] Bell 1815, Vol.2, pp.87-117
[15] *ibid*., p.89
[16] *ibid*., p.90; Morand 1728, p.52
[17] Gelfand 1973 A, p.79
[18] Méry 1700, pp.18-20
[19] *ibid*., pp.25,28-31; Bell 1815, Vol.2, pp.94-95
[20] Méry, *ibid*., pp.1-2,35-36,42-44
[21] *ibid*., pp.44,48-63,74-75,78,85-94; bell 1815, Vol.2, pp.100,104
[22] Quoted by Bell, *ibid*., p.104
[23] *ibid*.; Méry 1700, pp.75-88
[24] Schmitz-Cliever 1964
[25] Jaques quoted by Bell 1815, p.107; Certificate reprinted in Morand 1768-1771, Vol.2, p.89-93
[26] Deschamps 1796, Vol.2, p.83; Bell 1815, p.109
[27] Morand 1768-1772, Vol.2, pp.59, 87-88; bell 1815, Vol.2, p.107
[28] Quoted by Bell, *ibid*., p.106 and Wangensteen et al. 1969, p.936 respectively
[29] Cope 1953, p.19
[30] Douglas 1722; Cheselden 1723; Cope *ibid*., p.21
[31] Cheselden 1732, pp.341-342
[32] *ibid*., pp.342-343,344
[33] *ibid*., pp.344-347
[34] Cheselden 1740, pp.332
[35] Cheselden 1792, pp.332-333; Thomson 1808, p.68; Carpue 1819, pp.36,144
[36] Cheselden 1740, pp.333-334
[37] Morand 1731, p.144
[38] Morand 1728, pp.38,54-56,108-153,196-211
[39] *ibid*., pp.212,221,263,301
[40] Letter from Winslow to Morand quoted *ibid*., p.309-313
[41] *ibid*., p.222;1768-1772, p.115
[42] See Cheselden 1832, p.346; Morand 1731, pp.144-146,158-159
[43] Hist.Acad.roy.Sci.1731, p.23; Morand 1768-1772, Vol.2, p.121
[44] Morand *ibid*., p.148; Hist.Acad.roy.Sci. 1731,p.23
[45] Morand 1768-1772, pp.127-133
[46] Hist.Acad.roy.Sci. 1731, pp.23-24
[47] Morand 1731, pp.148,158-159
[48] Morand 1768-1772, Vol.2, pp.118,121-125 Morand 1768-1772, Vol.2, pp.118,121-125
[49] Le Cat 1766,p.5
[50] Le Dran 1730, pp.81,171
[51] Marcet 1819, p.24; Carpue 1819, p.167
[52] Moran 1768-1772, Vol.2, p.126
[53] Louis 1757; Le Cat 1766; Baseilhac 1779, pp.175-250; Deschamps 1796, Vol.2, pp.145,287
[54] Chevreau 1912
[55] Baseilhac 1779, pp.79-127,130-174
[56] Baseilhac 1804, pp.332-333

[57] Baseilhac 1779, pp.77-78
[58] Wangensteen et al. 1969, pp.936-938,947
[59] Le Dran 1730, p.66; Earle 1793, pp.94-95; Baseilhac 1804, p.81
[60] Klein 1816-1819, Vol.3; Wangensteen et al. 1969, p.937
[61] Deschamps 1796, Vol.2, p.68
[62] Dupuytren 1812, pp.4,42
[63] Manning 1780, Vol.2, pp.210,212; Bérard 1786
[64] James 1916, p.9
[65] Bell 1801, Vol.6, pp.148,152,202,204,206
[66] Bell 1784, Vol.2, p.133
[67] Pott 1771; 1790; Parkinson 1833
[68] See e.g. Earle 1793; Deschamps 1796; Saucerotte 1801; Langenbeck 1802; Treyeran 1802; Baseilhac 1804; Thomson 1808; Allan 1808; Dupuytren 1812; Klein 1801,1816-1819; Carpue 1819
[69] Earle *ibid*., pp.10,95; Saucerotte 1801, p.552
[70] Toellner 1965
[71] Klein 1801, pp.49,57; 1816, preface; 1819, dedication
[72] Treyeran 1802, p.176
[73] Gordon-Taylor 1958, p.77
[74] Carpue 1819, pp.36,77-110,143-146,159-169
[75] *ibid*., 173-174; Chevreau 1912, pp.91-92
[76] Hutchison 1830 A, pp.119-120
[77] Earle 1820,pp.211-212
[78] Batty Shaw 1970,p.221
[79] Marcet 1819, p.24
[80] Batty Shaw 1970, pp.233-234
[81] Gooch 1758, pp.xiii, xvi;1767, p.vii
[82] Gooch 1773, p.xvi
[83] J.amer.med.Ass. 1968; Dobson 1977
[84] Dobson 1779,pp.148-149
[85] *ibid*., pp.167,170-171
[86] *ibid*., pp.178-179
[87] Haygarth 1779,p.138; 1805,p.183
[88] Dobson 1779,p.179; Dobson 1799, pp.4,8,10
[89] Dobson 1779, pp.63-69,93-94,128-131
[90] *ibid*., pp.170-171
[91] Coley 1968
[92] Marcet 1819, p.vii
[93] *ibid*., pp.viii, 26
[94] *ibid*., pp.24,25-26,27-47
[95] *ibid*., p.46
[96] Smith 1917,pp.308-309,464-465 and passim
[97] Smith 1820, pp.2-3,8-9,23-28,31-32,39-40,50-51
[98] Kasich 1946; Brock 1965
[99] Prout 1821 A, pp.207,208-224
[100] Cooper 1822, pp.1101-1103
[101] Batty Shaw 1970, p.248
[102] Martineau 1821, p.403
[103] *ibid*., pp.403,405,409
[104] Crosse 1835, p.155
[105] Quoted by Batty Shaw 1970, p.248
[106] Arch.gén.Méd. 2; 474-475, (1824)
[107] Sabatier 1824, Vol.4, pp.303,307
[108] Huard and Vetter 1967
[109] Civiale 1827, pp.li-liv
[110] See chapter one of this thesis, p.28
[111] Yelloly 1821; Prout 1821 B
[112] Yelloly 1829 A, pp.60-63
[113] Yelloly 1829 B, pp.367-368
[114] Thomson-Walker 1935; Crosse 1968
[115] Crosse 1835, pp.155-156

[116] *ibid*., p.95
[117] *ibid*., p.158-165
[118] Velpeau 1839, Vol.4, p.656
[119] Lancet, i; 61, (1827-1828)
[120] *ibid*., i; 353-373,660-662,784 (1828-1829)
[121] Miller 1831, pp.1,4-5
[122] Lancet, ii;669,762-763, (1828-1829)
[123] No data are available on Edwards according to Davis 1952, p.187
[124] Lancet, ii 599-601; (1828-1829); ii; (1831-1832); ii; 626-628
[125] Lancet, i; 238, (1829-1830); see also, ii; 683-690, (1833-1834)
[126] *ibid*., ii; 229, (1830-1831); Miller 1831, p.38
[127] Velpeau 1839, Vol.4, p.651
[128] Batty Shaw 1970, p.251
[129] Inman 1844; Woodward 1974, p.91,187
[130] Keith 1844 A, p.123
[131] *ibid*., pp.123-124,129; 1844 B
[132] Miller 1831, pp.6-7
[133] *ibid*., pp.37-38

CHAPTER SEVEN: AMPUTATION

A. Introduction

1. The Indications for Amputation Until About 1750

Amputation was not an elective operation. Therefore, more than in the question of lithotomy, the elaboration of precise indications for it must be considered. As a background I shall briefly review the kind of basis on which this mutilating operation was performed up to about 1750.

The 17th and early 18th centuries were the great period of amputating limbs, „which was done with reckless profusion by the half instructed surgeons of the time", especially after the introduction of the tourniquet, which replaced cautery to check haemorrhage. This lessened one of the most imminent dangers of the operation: death by shock and haemorrhage. The principal indications for it were cold, dry and moist gangrene, but unfortunately the surgeons' apprentices resorted to any excuse for practising on their patients to bolster up their own conceit.[*][1] Early amputation for severe injuries had been recommended by Joseph Du Chesne (1546-1609), and it is evident from the writings of Richard Wiseman (1622-1676), the leading 17th Century exponent of English military surgery, that it had begun to be performed by the military surgeons of his time. French surgeons, followers of Joseph De La Charrière (t1690) and Barthélémy Saviard (1656-1702), availed themselves so quickly of the advantages of the tourniquet that the frequency of amputation appears to have become a common topic of raillery and reproach. According to Pierre Dionis (t1718), another great name of French surgery, even King Louis XIV was impressed with the popular belief that his soldiers were as much in danger from the chirurgical ardour of the surgeons as they had ever been from the fire of the enemy.[2]

After the end of the War of the Spanish Succession in 1713, a period of relative quiescence followed on the European political scene until the War of the Austrian Succession (1742-1748) and the Seven Years' War (1756-1763). This allowed the military surgeons of the long-lasting campaigns of Louis XIV (1672-97, 1701-13) to write down their experiences. Lorenz

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[*] Garrison (1922) draws attention to the grotesque mendicant cripples commemorated in „contemporary" art by Bosch, (†1516), Brueghel (†1569) and Callot (1592-1635).

Heister (1683-1758), after having fought with the Duke of Marlborough (1650-1722), became a professor and wrote a much-translated Chirurgie (1718), a work which remained authoritative for over a century.[3] It contained accurate technical indications but none detailing the actual need for the operation. Henri François Le Dran's (1685-1770) *Traité et réflexions tirés de la pratique sur les playes d'armes à feu* was first published in 1737, and an English translation in 1743. It was constantly quoted with respect to gunshot wounds until the early 19th century. Le Dran advocated immediate amputation in cases of severe crushing with fractures, in crushed joints, and when a limb was completely shot off.[4] This strong precept relied on a memory-based general experience of such cases: „It is very true that some limbs have been conserved... but it is also true that many more of these injured have perished than were cured," or, „it is true that one has seen cured some of these wounds without amputation; but so many patients died because it was not done, that it is a necessity to promptly perform this operation".[5] In his earlier and more general *Observations de chirurgie* (1731), which were still being edited in England in 1758, Le Dran had given a quantitative estimation of his experience:

„The cutting of the limb must not be deferred, and experience teaches that for one patient whom a triumphant nature will have saved without amputation, ten will perish if one retards it."[6]

As illustrated in the foregoing chapter on lithotomy, Le Dran knew how to handle hospital statistics as proofs of the success of a technical method. Yet in peace time severe injuries needing amputation according to his rules were rare, and success rates from his earlier military practice were probably not available to him, especially since he wrote on this issue twenty years later. Also it is doubtful whether such data would have been of interest to him in the case of severe injuries for which he saw amputations as the last resort of the art, rather than as an elective operation to relieve pain. Thus, the two main objections to early amputation, namely the hope of saving the limb, and the shocked state of many a badly wounded soldier were refuted authoritatively: „The first will not be found among true practitioners who know at the first glance if a wound may heal with or without amputation". The second, too, was unreal, for „weakness, it is true, is an advantage for the patients, if it is not occasioned by the perversion of the fluids [liqueurs]."[7]

Le Dran’s precepts, presented in a clear style and originating from a man who had made significant contributions [Having been a surgeon to the French Army in Germany Le Dran had become consultant surgeon to the camps and armies of Louis XIV. He was among those who brought the operation of lithotomy into good repute in France (see p.366).] had a powerful influence over the opinions and practice of his contemporaries and also his successors in Britain, since part of his work was translated into English by Cheselden.[8]

In Britain, John Ranby (1703-1773) was the leading military surgeon of the time. He must have known of Le Dran’s work for he was involved with Cheselden in the separation of the surgeons from the barbers in 1745. Ranby was the first Master of the newly founded Surgeons’ Company, Cheselden a Warden.[9] After Cheselden’s death, Ranby succeeded him as surgeon to the Royal Hospital in Chelsea. In his *Methods of treating gun-shot wounds* (1744, still re-edited in 1781) he drew conclusions from his experience of a campaign during the War of Austrian Succession; they were in Le Dran’s manner but without the latter’s relative precision:

„If a wound be of such a desperate nature, as to require amputation (which is often the case, when it happens in any principal joint) it would certainly be of consequence, could the operation be perform’d on the spot, even in the field of battle.“

Examples of extraordinary success justified this recommendation.[10] Francis Home (1719-1813) (see above, chapter 5) set up general rules *à la* Le Dran on exactly the same basis.[*] [11]

In the civil field, Cheselden complained in 1731 of not having enough facilities for amputating in London’s St. Thomas’s Hospital.[12] In 1737 Alexander Monro *primus* underpinned his special method of amputation and after-care by statistics from the Edinburgh Infirmary (see above, chapter 5). In 1752 he listed 99 amputations of large extremities with only eight deaths. These encouraging results were still being published unaltered in 1781 by his son, Alexander Monro *secundus*.[13] This is the more astonishing, as both father and son showed great care in the evaluation of the value of mastectomy for breast cancer. Yet, it is true that while they carefully examined and published the success-rate of this operation and the proportion of patients who survived after a given number of years, they indicated no such data for untreated patients. The fate of untreated patients was tacitly assumed to be worse than that of the operated: for instance, Monro asserted that whilst in a series of seventeen patients, eight, or near one half, were still alive after a certain time; „if no operation had been performed, there would not have been above two or three alive“. He was „much inclined therefore to think that we ought to attempt the cure by an early operation“.[14]

Coming back to amputation one sees that frequent failures on the one hand, and on the other hand the recoveries which sometimes took place in cases which had seemed at first to require amputation, but in which it had not been performed, kept doubts on its value alive in many a practitioner.[15] In the next section I shall therefore discuss how Le Dran's and Ranby's boldly outlined indications were questioned scientifically after 1750.

B. The Main Topics 1750-1790

1. Doubts On The Value Of Amputation

The doubts on the value of amputation found expression, especially in France, in the aftermath of the War of Austrian Succession when Le Dran's principles had first been applied on a large scale. At least ten memoirs were published on the question around 1755.[16] A formal discussion on the value of amputation and of some of its technical aspects began in Paris, the leading surgical school of that time, and particularly in its unique *Académie Royale de Chirurgie*. Significantly, the question for the Academy's annual prize for 1754 was:

„Amputation being absolutely necessary in wounds complicated by shattered bones especially those arising from fire-arms; determine the cases in which amputation should be done immediately, and those in whom it is convenient to defer it, and give all the reasons.“[17]

The prize and official sanction were given to a '*Mémoire'* by Faure (1759), a French military surgeon. He favoured delayed (secondary) operation, on the basis of a deliberate, planned experiment of his. It had been carried out on ten wounded Englishmen after the Battle of Fontenoy in 1745, and the results had been compared with the overall mortality of immediate (primary) amputation after the same battle.[18] This decision of the *Académie* made secondary amputation respectable, but the question of primary versus secondary amputation still continued furnishing one constant point of debate throughout the next hundred years. It is

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[*] Home's book was also translated into German in 1768 and French in 1772.

noteworthy that the main issue considered by the *Académie* in its first discussion of amputation was not whether amputation should be done in a given type of case, but a particular technical point, namely the timing; and this was so even though the indiscriminate use of amputation had come under attack.

2. The „proof“ of the Inutility of Amputation

However, the question of the usefulness of amputation was re-launched most vigorously and cogently during the Seven Years' War by the Swiss Johann Ulrich Bilguer (1740-1796) one of the three surgeons-general in the Army of Frederick the Great of Prussia. Horrified by the mutilations carried out by barely trained craftsmen-surgeons,[19] Bilguer wrote a thesis during the winter war-break in 1760-1761,[20] in which he had the courage to throw the gauntlet down against the prevailing practice of his time. As its original title, *Dissertatio de membrorum amputatione rarissime administrada, aut quasi abroganda...*(1761)[21] indicated, he declared amputation needless in most cases. Indeed, for the first time Bilguer was able to show results of conservative treatment carried out on a large scale during the War. He also gave figures, reminding his readers of Celsus's words „that diseases are cured by proper remedies not by display of eloquences“. His motivation was clear: „My first thought of this subject arose from observing what passed under my own inspection in the military hospitals“; amputation was often done to preserve life but almost never answered this end, and by conservative treatment he was able to cure patients with severe injuries, even those with limbs shot off by cannon balls.

His actual method has been described by a 20th century surgeon.[22] Bilguer's theoretical argumentation, although interesting enough in certain instances,[23] is also of less concern to us than the way in which he presented his results, for he himself valued a practice confirmed by repeated experience higher than one conformable „to sound reason“:[24] At one time during the War he had in a military hospital 6618 wounded who were all treated according to his directions, some of whom he attended himself. 653 died. 5557 were perfectly cured so that they could again endure all the fatigues of service; 195 could again do duty in garrison („semi-invalides“); 213 remained incapable of any labour, civil or military („grand invalides“).These two groups, i.e. 408 soldiers, must have corresponded to the number of those with compound fractures and complicated and dangerous wounds. Men with wounds of

the head or the muscles were not included in the list of invalids in the Prussian Army, being allegedly satisfactorily cured with conservative treatment.
The precise source of these data is lacking. They were probably drawn from an administrative report, since the following more specifically medical figures were produced by mere estimation:

> „Let us at present suppose, that of the six hundred and fifty-three who died, no more than two hundred and forty-five died from the consequences either of a violent concussion, from wounds of the head, thorax, lower belly or spine; from a complicated fracture of the os femoris, or from putrid fever, fluxes and other inward diseases, which often happen in military hospitals, even in cases of slight wounds, from the bad air which is breathed there: there will remain four hundred and eight, who may have died from the consequences of wounds with shattered bones; and this number is equal to that of those who were cured without amputation, although their wounds had been of the same kind. If, after making these calculations, we compare them with the prodigious number of wounded men, who, at the beginning of the war, had their limbs taken off on account of dangerous wounds, of whom scarce one or two escaped with their lives; we may very safely conclude, that much the greater part of those four hundred and eight men cured and sent to the invalides, would have died if amputation had been performed on them, and this shocking artificial wound added to what they had already received.“[25]

Bilguer’s Dissertation undoubtedly made what we would call nowadays a sensation in the whole of Europe. In a century dominated by French surgery it was the first German surgical text since Heister (1718) to be translated into a foreign language, and rather extensively and quickly. First there was a German edition in 1761, English and French ones followed in 1764, Italian and Dutch translations came out in 1771 and finally a Spanish one appeared in 1773. Why was this booklet so successful? Bilguer’s style in German and the translations had nothing of the elegance of the French ‘Mémoires’ of the 1750s. But he was concise. Instead of relating cases over hundreds of pages he explained his practice simply and condensed the results on one page providing rather impressive figures. Moreover, his mood was aggressive. This was especially so in the French translation by Simon André Tissot (1728-1797) who added acid comments on the Paris Academy, and from which the English translation was made. Thus the original, more subtle Latin title bluntly came out as Dissertation on the inutility of amputation of the limbs (1764). (It was dedicated to Sir John Pringle). From both the enthusiastic approvals - Frederick II forbade amputation in his Army except in fully developed gangrene[26] - and equally harsh condemnations of this book, I could collect hardly any comment on the large number of Bilguer's observations as given in his figures. One may conclude that it was its strident tone which caused most reaction, and there was also, of course, something to be said against the painfulness of his method of repeated wound incisions as compared to the single one needed for an amputation.

In England, Percival Pott of London's St. Bartholomew's Hospital took exception to Bilguer's „indecent as well as untrue reflections on the profession in general and those who have the care of hospitals in particular". And he further insinuated: „The boast of universal specifics... and of means whereby chirurgical operations may be rendered totally unnecessary, is the language of quackery, and not of science".[27] Although Pott wanted amputation to be limited he refuted all five instances, for which Bilguer had denied its necessity, by mere argumentation and without any word on the results of his own practice, from 1769 till his death.[28] Despite Bilguer's results he continued to reason exactly as Le Dran had done some fifty years earlier: he claimed that from general experience „the chance of death from amputation is by no means equal to that arising from [compound] fractures". And although desperate cases were sometimes cured, such escapes were „much too rare to admit of being made precedents".

Benjamin Bell (1749-1806)[29] one of the influential surgeons of the Edinburgh medical school,[*] although very wary of amputation for chronic diseases on purely authoritative grounds, strongly recommended it for compound fractures after gun-shot wounds, again on authoritative basis.[30] The only figures he adduced concerned the danger of the operation, which, as he said, had greatly decreased: „In the present improved state of the operation I do not imagine that one death will happen in twenty cases" in hospital practice, and in private practice "the proportion will not be so great".[31] Bell's own wording shows that these figures were not based upon actual recording of his own practice. But given the wide distribution of his *System of surgery* they might have set wrong standards all over Europe, which it took time and courage to disprove.

[*] Bell's books on ulcers (1778) and his System of surgery (1783-1788) were both translated into French, German, Italian, Spanish and Portuguese.

Another Scotsman, Thomas Kirkland (1721-1798), together with an Irishman, Sylvester O'Halloran (1728-1807) became more objective supporters of Bilguer's ideas than even Tissot had been. Kirkland,[32] and Edinburgh-trained country practitioner, stated that Bilguer was „far from having proved the inutility of amputation, yet he certainly has given proof enough to show that immediate amputation is not often necessary". Kirkland brought forth a new argument, inferring that hospital surgeons might have been induced to amputate frequently from a principle of humanity, since it had been found that conservative treatment was much less efficient in crowded public hospitals than in country and private practice.[33] However, he did not give any facts in support of this statement. Neither did Sylvester O'Halloran,[34] the most distinguished Irish surgeon, experimentalist and historian of the 18th century. In agreement with Bilguer, he criticised the frequent abuse of amputation. Amputation, he said, was sanctioned by antiquity, even if the patient died; whereas the ignorant public would not accept death after conservative treatment.[35] But the main point of his book was, as we shall see below, the revival of the technique of flap-operation whereby he hoped to increase the success rate of amputations.

Nonetheless, one of the most important questions about this most important operation was frequently asked: What is the value of operation compared to conservative treatment? No good writer on the subject of amputation could go without at least mentioning it. After the new invention of the tourniquet had made amputation relatively less dangerous, amputation had been done for nearly a century until it was realised that its chances of success were perhaps not much better than those of conservative treatment.[36] O'Halloran, a typical enlightened surgeon of the 18th century trained in London, Leyden and Paris, set the question of indications for this operation above all others in surgery: „The clearing up of this single point alone, is unquestionably of the greatest service to mankind, particularly to the military".[37] But significantly he would not, in his investigations, enter more deeply into this question but rather into the perfecting of the operation itself. Innovations of this kind I shall discuss in the next two sections.

3. The Flap Operation

One achievement of O'Halloran was the revival of the flap-technique for amputations which he introduced as follows:

„It is true, this operation will be, by this means, more rare, but then it will be more successful: the general estimation of recoveries, to deaths, in this single article is as thirty to one hundred,[*] but, by the principles here laid down, we boldly affirm, because justly, that in an hundred amputations, *ninety-five shall recover*! An acquisition of knowledge, highly honourable to surgery, and acceptable to humanity!“[38]

But the announced „complete discussion of this affair“ was illustrated by only three cases, Two operations for breast cancer and one amputation. Thus O'Halloran's revival[39] of the flap-technique i.e. the covering of the wound with a skin flap, was introduced in a purely qualitative way in 1765. It proved very useful, when used together with the immediate union of the wound edges, in reducing the duration of wound-healing as compared to the older circular technique in which the wound was covered with charpie (impregnated lint). „Yet all the proofs necessary, to a mind open to conviction, I think, are here advanced“. And, likewise, four theoretical objections to his new method were explained away by argumentation rather than by presenting relevant results. Nevertheless, O'Halloran's work also contained some „quantitative“ aspects. In his attempt to improve the operation he took Faure's side in the question of its timing, flattering himself, after long reasoning about the Paris quarrel, „to be able to throw this whole affair into an higher degree of certainty“: He added five of his own observations to Faure's, namely one successful early and four successful delayed operations. [40][Paradoxically enough there is no mention of the technique (circular or flap) used in these cases.] Quite understandably, his work was approved of by the Académie de Chirurgie in Paris.[41]

Two of O'Halloran's influential friends also became the best propagators of his method in his own country. The first was Charles White (1728-1813),[42] the surgeon who had taken a leading part in founding the Manchester Infirmary in 1752, and who had introduced cleanliness into midwifery. Together with his friend Percival he was also involved in the foundation of the Manchester Literary and Philosophical Society, and he participated at the meetings of the Warrington Group. White had used hospital records as a basis for a paper to the Royal Society in 1762 on the topical application of the sponge in the stoppage of operative haemorrhages, and in midwifery reports (see above).[43] In February 1769 one of

White’s papers was read by his friend John Hunter in a London medical society. White reported on all his seven amputations performed by O’Halloran’s method at the Infirmary between 1766 and 1768. Having read O’Halloran’s book, White said he tried the method immediately on his next case.[44] In his *Cases of surgery*, which appeared a year later he included the same table, which had meanwhile increased to eight cases with one death, „since I was in the habit to keep minutes of all my cases“ (as his father - a Manchester accoucheur - had done). This table, giving name, age, date of admission and discharge, written with the „only aim to represent facts as they really were, not as they would tell the best“, represents after Alexander Monro the first comprehensive report of a practice I could find in the history of amputation - and the first in tabular form at all. At the same time White’s book made a plea, on Monro’s lines, that „men in every science would... divest themselves of that illiberal spirit of prejudice and jealousy which is too apt to prevent the mutual assistance which they owe to one another, and to the public.“[45]

The other of O’Halloran’s friends who propagated his method in London was Sir William Bromfield (1713-1792).[46] A pupil of Ranby he became associated, as Ranby had been, with St. George’s Hospital and the Court. Bromfield was also acquainted with Charles White.[47] The account which this fashionable London surgeon published in his Surgical observations and cases in 1773 is rather typical of the later 18th century in its mixture of adherence to Authority and factual observations and experience:

„I have such authority in my possession, as induces me to believe, that Mr O’Halloran’s method deserves preference to that I have recommended [myself]...; and I shall not ever be ashamed to retract my methods..., by adapting a practice recommended by others, which, experience in repeated instances, has proved successful... . The many idle reports that have been spread, relative to the disadvantages from this new method of amputating, and the conclusions drawn from false facts determined me to make enquiry, as to the success in general, from those who had performed the operation repeatedly with the flap. I shall not trouble the public at present with the authorities in my possession, by way of answer to the objections I have heard made by some, who have never performed the operation themselves in this manner, nor even seen it performed by others... But ... if we give credit to the cases related by Mr White, we shall find that... [these objections are] ill grounded.“[48]

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[*] A figure quoted during the Paris discussion in the 1750s.

It is true, Bromfield added two successful cases in support of O'Halloran but he did not give White's figures!

James Lucas, a surgeon appointed to the Leeds Infirmary at its foundation in 1767,[49] continued white's pattern of publication by sending a paper on the amputation of the ankle with the flap technique to the Medical and Chirurgical Society in London. It was read, probably in 1775, and subsequently published in the Society's Medical observations and inquiries (1776). Lucas also included all his nine cases, from March 1st 1772 until September 30th 1775, tabulated exactly as White had done, to whom he referred.[50] His table was even enlarged by a column stating the date of application of the flap (which according to O'Halloran was not meant to be an actual healing by first intention (prima intentio). (s in White's table, Lucas's were all cases of joint-tuberculosis. He had lost two patients, and reported two more still in cure 25 days and 1.5 years after the operation, respectively. Lucas also published a complete series of his seven operations for cataract from 1769-1782.[51]

4. The Immediate Union of the Wound Edges

Such reporting of the results obtained by one surgeon over a length of time was the prelude to what Edward Alanson (1747-1823)[52] did four years later. With his step of combining the flap technique with the immediate post-operative union of the skin-edges by apposition he hoped to achieve true healing by first intention. It constituted a „revolution" in amputation technique.[53] This also appears to be quite true for our present concern, as this Liverpool surgeon numerically compared his results of amputation by his new method with those he had previously achieved by the old technique.

Alanson, apprenticed to a local master and later a student of John Hunter in London, had been associated as a surgeon with the Liverpool medical shake-up when within a few years of 1770 all six of its consultant posts, three surgical and three medical, fell vacant, and six young men with original and enquiring minds were appointed. Some of them were interested in statistical recording. In 1767, for instance, Henry Park (1744-1830),[54] a pupil of Pott in London and Le Cat in Rouen, was appointed a surgeon. Throughout his career he kept a careful record of all his obstetric cases, which from 1761 to 1830 amounted to nearly 4,000.[55] In 1770, when Alanson was appointed a surgeon, Matthew Dobson was appointed a physician.[56] These young men were more than colleagues at one hospital. „There was a remarkably close friendship and co-operation, not only with the surgeons but with the three equally young and brilliant physicians“. There was also teamwork, the consultants helping each other in their many research projects.[57] It was in this atmosphere that Alanson and a little later Dobson and James Currie wrote.

Alanson described his method of numerical comparison, its deficiencies, and results frankly in the preface to his influential Practical observations on amputations (1779). [A second English edition appeared much enlarged by the favourable judgment of many colleagues in 1784 together with a French translation. There was a German translation in 1785.]:

„When we attempt to introduce any new and important deviations from the common mode of practice into general use, and, particularly... in the mode of performing and after-treating one of the principal operations of surgery, the public have a right to be fully acquainted with the author’s reasons and motives...; and such trials should likewise previously have been made, as are sufficient to demonstrate that the doctrine recommended will bear the test of general experience.. Had I been aware of the utility of such an attention, I would not have omitted taking an accurate history of every amputation at which I have been present. However, the following heads of success may be relied upon, and I hope will answer my present purpose.“[58]

Previous to his „improved“ plan Alanson had been present at 46 amputations, the after-treatment of which he had had an opportunity to inspect. Ten of them had died.[59] He listed the causes of death numerically, as well as the complications. In more or less all surviving cases violent symptomatic fevers and exfoliation of the bone were reported to have occurred. Alanson believed that most surgeons had had no better results with the old method. In fact there was - as far as I can see - no such detailed account published at all at this time.

We may imagine that such results called for an improvement. Influenced by O’Halloran, White and Bromfield, with whom he was in contact, Alanson adopted their method together with a revival of the immediate reunion of the wound-edges. His report on the results with this new method shows yet another facet underlying the collection of facts to be presented as „statistics“: One has to be aware not only of a selection made after the operation according to the outcome, but there may already be a bias in the admission or exclusion of certain cases from surgery at all. Alanson asserted: „I have never refused to operate upon any case that has presented, where a single person in consultation has thought such operation adviseable". Thus, since he began his new method he had „operated in thirty-five cases, such as promiscuously occurred at the Liverpool Infirmary without the loss of a single patient". Symptomatic fever and other complications had in all of these been slight, and with one exception there had been no haemorrhage. After one month the wounds had either healed or become smaller than a sixpenny piece, and all patients had ultimately been cured.[60]

Alanson also requested information on the results of his colleagues since they had adopted his technique one or two years before.. He published some of their observations together with some of his own as selected illustrative cases, broken down according to the anatomical localisation of the operation. [i.e. thigh, above the ankle, arm, forearm.] This break-down served, in a qualitative way, to illustrate that the new technique was applicable to all parts of the extremities rather than to establish a comparative view on the successes at each site of operation since there were no indications with this respect. Neither were there indications on the diseases necessitating the operation.

The technique of immediate union of the wound-edges was also propagated by Benjamin Bell's System of surgery (1782-1788), which he considered to be the successor of Heister's,[61] and which became the foremost handbook in Europe in the 1780s and 90s (see above). Although Bell claimed some priority over Alanson[62] he did not deem it necessary to give detailed results of his practice, as Alanson had done. As a matter of fact the technique was quickly adopted in Great Britain and on the Continent with the exception of France, where its advantages were still a point of discussion in the 1840s.[63]

5. Recapitulation

Despite the two promising innovations of the flap- and prima-intentio-techniques, some British surgeons such as White and Park still attempted to avoid amputation. In cases of „white swelling"of the joints (joint tuberculosis?) they tried „conservative"surgery, i.e. the mere excision of the joints, in the 1770s and 1780s. Park excised his first knee-joint successfully in 1781 but had to wait until 1789 for another, albeit unsuccessful, case.[64] It is indeed difficult to assess the impact the discussions on when to amputate, and the innovative publications had on practice in peacetime, when the incidence of amputation, both for disease and injury, was small.[65]

Thus, previous to the French Revolution four main topics had emerged concerning amputation: the indication for the operation, its timing and the technical innovations of the flap and the immediate union of the edges of the wound were discussed with passion and indeed with some objectivity. It is noteworthy that the first question, which would need a comparison of results between a conservatively treated group and one operated on, was treated only one-sidedly by the representatives of either camp, the ‘interventionists’ and the ‘conservationists’, whereas questions of a technical innovation for the operation itself were already approached by a sort of „*a posteriori*“ programmatic comparison, involving indiscriminate reporting of all unselected cases occurring during a certain length of time. In this latter context I also found the first tabular accounts, designed to give concise information on the relevant points for a number of patients. It is noteworthy that the first numerical reports came from two sources, i.e. from foreign military surgeons, (e.g. Faure and Bilguer), and from a group of surgeons who knew each other and worked at provincial hospitals where other arithmetic observationists were also active (White, Lucas, Alanson). By contrast, the representatives of the great hospitals in the capitals possibly relied on equal experiences, but, with the great exception of Monro primus, their statements were obviously based on memory only. (Bromfiels, Bell, Pott).

As for amputation in the Army, I have not been able so far to find any quantitative works on surgical practice in the two major British wars at the end of the 18th century, i.e. the West-Indian Campaigns and the American War of Independence, with the exception of those reported by Blane from Haslar Hospital (see below). Surgeon Neale’s account (see below), reported single cases of gallant officers and general statements still exhibiting the influence of Le Dran. I shall thus consider next how the surgeons dealt with the multitude of injured limbs they suddenly had to treat during the nearly 25 years of the Revolutionary and Napoleonic Wars. Accordingly, the following section will be concerned mainly with the amputations for gunshot wounds in military conditions.

C. The Experience of 25 Years’ Wartime

1. The Indication for Amputation for Gunshot Wounds by 1800

In Britain, there was not, as in France, a Royal Academy which might have officially sanctioned and ruled out on a certain practice. The surgeon general of the British Army at the outbreak of the war in 1792 was John Hunter, perhaps the most original scientific surgeon of the 18th century. Yet he had only a small personal knowledge of gunshot wounds gained as senior staff surgeon of the Belle Isle expedition (1761) and in Portugal 1762-1763.[66] In his famous posthumous book on Blood, inflammation and gunshot wounds (1794), which comprised 540 pages, he devoted only four pages to the indication and timing of the amputation. For him, the „physiologic surgeon“, primary amputation was „violence superadded to injury“, which instead of fighting inflammation, brought along inflammation, loss of substance and most probably more loss of blood. Therefore it could be indicated on the battlefield only in wounds of a large blood vessel, or when a limb had been almost completely separated; and it was meant chiefly to ease the transport of the wounded. Contrary to his usual method Hunter did not refer to his own experience at all, but rather to the results of the Paris discussions of the mid-1750s when he wrote:

„I believe it is universally allowed by those whom we are to esteem the best judges, those who have had opportunity of making comparative observations, with men who have been wounded in the same battle, some where amputation had been performed immediately, and others where it had been left till all circumstances favoured the operation; it has been found that few did well who had their limbs cut off on the field of battle, while a much greater proportion have done well, in similar cases, who were allowed to go on till the first inflammation was over, and underwent amputation afterwards.“[67]

Hunter had a certain theoretical influence in his day, Walter Weldon, a surgeon at Southampton,[68] entirely supported his views without details of cases; and he thought it impossible to lay down general rules.[69] Thomas Chevalier (1767-1824), a Cambridge graduate and surgeon to Millar’s Westminster Dispensary, obtained the premium of the Royal College of Surgeons in 1803 for his Treatise on gunshot wounds (1804). This monograph, a sign of the war times, was in its third edition by 1806. Chevalier’s opinions closely approached those of Hunter. Yet he recognised that the operation had been improved since Bilguer’s time by the flap and immediate reunion techniques, and that immediate amputation might be useful when the patient had to be transported.[70] Indeed, the foremost objections of military surgeons to conservation of joint excisions, once they were out in the field, were that the conditions, „were not always such as to admit of such attempts; and that the hurry of an action may often oblige a surgeon to have recourse to the most expeditious method of saving his patient".[71]

With this statement even Park admitted that the „environmental" arguments were pertinent in a certain degree. But he thought that military surgeons were also

„so strongly impressed by the fatal consequences that await the attempts to cure compound fractures [conservatively] in the London Hospitals, though under the management of, perhaps, the first Surgeons on the surface of the Globe, as to think of amputating the limb."

This was especially so as the ease with which severe injuries of this kind recovered under the care of surgeons of very inferior abilities, but in rural practice, even in country hospitals, was „universally known".[72] John Neale, at least, the only author with long practical experience, recommended amputation as early as possible in cases of open fractures, especially if a joint was affected. This advice was based allegedly on his scattered notes from the American War, recollected now in order to contribute to the great national effort.[73] (John Hennen, however denounced it in 1829 as a compilation and translation from Le Dran).[74] And Benjamin Bell, as we have seen above, greatly favoured primary amputation and even rejected excisions for theoretical reasons in his System of surgery which still enjoyed wide circulation.[75]

As a matter of fact, according to Hennen, the practice of the naval and military surgeons seems to have been in favour of intervention right from the start of the long period of war, be it on the Continent, in the Egyptian expedition, in Portugal or later in Spain. And amputations, „when indispensable", were done at once. He said that upon his arrival on the Peninsula the advisability of the practice was impressed on the surgeons by their seniors and that it was constantly followed. For him differences of opinion on this question had only existed in books by civilian authors. Yet this testimony was written with the hindsight of 1829, by a man who was convinced that the War, and especially the work of James Guthrie (to be discussed in the next section), had fully proved the necessity of immediate amputation for a wide range of wounds. He hoped indeed, that the question was by the „set at rest for ever".[76]

At the onset of the War, however, there must have been some uncertainty in civil and military practice, as there was uncertainty and contradiction in the literature and teaching, which were, at that time, dominated by civilian surgeons. These relied only on a few observed cases, supplied occasionally by an accident, a duel or a volunteer-field-day. The only British textbook of military surgery was the old one by John Ranby (see above, chapter 5), and we may probably trust Hennen when he wrote that „many ponderous tomes were therefore dragged from their dusty abodes“.[77] It is to the scientific credit of John bell and Samuel Cooper to have recognised this uncertainty at that time - even if the London Medical Review regretted that Bell's style and spirit were so little worthy of imitation.[78]

John Bell (1763-1820), the Edinburgh anatomist[79] in his Discourses on the nature and cure of wounds (1798) wisely left the question of indication undecided, just as had Samuel Cooper in his important works (see below). Bell wrote that all the surgeons of Europe with their collected experience had left it so. In this attitude he was quite consistent with his own ideas about opinions, authorities and names, which „might put us wrong“, since he had only a very small number of his own cases to quote himself.[80] His book was more a witty, sometimes a journalistic analysis, (e.g. when he quoted sentences out of their original context to fit his ideas[81]) of the value of the arguments of others rather than an advocacy of his own views from his own experience or that of the older military men (which seems to have been generally recognised):[82] How could he trust the latter, convinced as he was of „how strangely a man's opinions grow up in his mind, distorted by thousand circumstances“. Indeed, the operations of the 1740s were no longer the same, technically, in 1800, nor were the Prussian successes under Bilguer comparable to an alleged failure rate of two thirds in France, nor was the practice in one hospital an appropriate model to set up rules for country infirmaries or a great general hospital.[83] Published cases of success, although surely trues, were often mere exceptions, which he felt it was better not tot trust too much:

„The true appearance of these cases is really amusing to a deliberate observer... We could, I think, upon an emergency, produce ten or twelve tales of knives cut out from the stomach safely, as many cases of gangrenous herniae cured, a hundred wounds of the brain with great spoonfuls of it discharged: the person continuing very sensible and witty, and sometimes... wittier than before... And yet, notwithstanding all this, no man will believe that knives are easy in the stomach, strangulated herniae safe, or wounds of the brain without danger; neither should Mr Belguer's twelve cases, nor any twelve cases produced by any other man, induce a surgeon to believe that gun-shot wounds, with lacerated arteries and broken bones are safe.“[84]

What was wanted, thus, was large series of observations comprising all cases of amputations, successful and unsuccessful alike, in well described, comparable circumstances as a basis for judgement. And for this as it had been with the treatment of fevers, naval surgeons had led the way.

In 1799 Blane, not being a surgeon, felt incompetent to choose between John Hunter's recommendations or those of the active naval surgeons.[85] But he knew that at Lind's Haslar Hospital, some books of the surgical operations were kept. Hoping that they might „serve as a subject of comparison to those who perform amputations on board of ships at sea", Blane included these results from 1772 till 1778 in his Observations (1779).

[i.e.	4 thighs	with 1 death
	27 legs	with 10 deaths
	2 forearms	with no deaths
	7 arms	with 2 deaths
Total:	40 amputations	with 13 deaths]

It is not indicated what these operations were made for, but Blane could provide the long-term results of 28 primary amputations for gunshot wounds made on board the ships during an action in 1778 which were afterwards brought to the Hospital. These cases were classified according to anatomical localisation,

[i.e.	7 thighs	with 1 death
	5 legs	with 2 deaths
	14 arms	with 5 deaths
	2 forearms	with no deaths
Total:	28 amputations	with 8 deaths]

So were also eight operations made afterwards in hospital (three deaths). Clearly, he thought that an answer might be found by comparison for results.[86] John Rollo, the surgeon general of the independent Department of the artillery (called Department of the Ordnance) and surgeon in chief at its Hospital at Woolwich, also kept exact records of his operations there and was able to publish the results of his first five years' practice in 1801: 27 simple or compound fractures had occurred, for which 22 amputation (three deaths) had been necessary. However there were no further details.[87] As the following section will illustrate Blane's and Rollo's attempts showed the way that British Army surgeons, led by Wellington's surgeon-general McGrigor, were going to take in order to elucidate the still imminent question of primary or secondary amputation.

2. Primary or secondary amputation for gunshot-wounds

How right Mc Grigor was in his prediction of the possible scientific value of statistical returns is clearly shown in the history of amputation by the contribution of one of his deputy inspectors on the Peninsula James Guthrie (1785-1856).[88] Guthrie was the most brilliant of the active British surgeons of the Peninsular War. Afterwards he became a major figure in English hospital medicine [Surgeon at Westminster Hospital, and President of the Royal College of Surgeons from 1834 till 1842.] and a consistent fighter for the reform of medical education - not only for the Army doctors. In his book *On gun-shot wounds of the extremities* (1815), dedicated to his former chief, Guthrie laid out his experience of the Peninsular campaigns according to the new standards set up by McGrigor. For him the question of indication of amputation was largely contained in that of its timing.

Guthrie was not the first to claim that he stated nothing but facts, (which still may lead into error as we have seen), and that he trusted „...in no part to theory or opinion of authors not supported by actual experience". He excused Hunter by conceding that none of the authors the latter had relied upon had given immediate amputation a fair trial, or if so, that the constitutions of their patients must have been different from his own.[89] He described clearly how such a trial should be conducted:

„It is not sufficient to perform twenty amputations on the field of battle, and contrast them with as many cases of amputation, done at a later period. The twenty cases for delayed operation must be selected on the field of battle, and their result compared at the end of three months with that of the others when the value of the two modes will be duly estimated."

This was truly a programme for what we would call today a prospective study, and it was better and more objective than anything published earlier in this field. However, Guthrie immediately set out to explain why he had never actually done this

„.....because I had ascertained the safety of immediate amputation in all cases that required it, after the first battles of Rolica and Vimiera in 1808; and when circumstances would have enabled me to have done so, I did not feel myself authorised to commit murder for the sake of experiment."[90]

This profound point of view is still relevant today when the application of „a sole possible treatment“ is discussed. However, everything would depend on the prior determination of the „safety“of early amputation. Thus in his four tables Guthrie presented data, which he himself considered insufficient. Two tables comprising returns from June to December 1813 compared secondary with primary operation. 551 secondary operations (i.e. after three to six weeks) in hospitals were opposed to 291 primary amputations (i.e. within 48 hours) [These groups were further subdivided into amputations of the upper and the lower extremities.]. The number of patients dead, cured and still under treatment were given, but the latter were considered as cured for the further calculations which showed that „the comparative loss, in secondary.... and primary... operations is as follows:

	secondary		primary
Upper extremities:	12	to	1
Lower extremities:	3	to	1 „

[My recalculation gave an even more favourable relation, i.e. 12.6:1 for upper extremities and 3.9:1 for lower extremitie].

The two other tables made the same type of comparison between 48 early operations and 51 delayed cones on the occasion of the battle of Toulouse in 1814. The average rate of failure - not of success as erroneously printed- in primary operations was one-fifth; in secondary amputations it was one in two and a half, as estimated three months after the battle. According to his reports the success rates of primary amputation for the upper limb was 95%, compared to 75% after the secondary operation. (He would write: the operation is safe in nineteen or in fifteen cases out of twenty, respectively). [I calculated 80% and 50%, respectively for the lower limb]. This difference was „certainly very remarkable,“and it quickly became known to all surgeons in the British Army.[91] Guthrie gave another table of the same kind concerning the safety of immediate amputations at the shoulder-joint, which were not included in the two above-mentioned tables.[92]

Concerning the indication for amputation of the thigh he described an interesting trial: after the battle of Toulouse in 1814, 43 of the best fractures of the thigh were treated conservatively, because within a short distance there were good hospital facilities. After three months, of the

43 patients

13 had died	12 suffered second amput. 7 of whom died	18 had retained their limbs		
		5 were cured completely	2 more or less cured	11 wished to have been amputated as now not likely to recover, or only as invalids.[93]

According to his above mentioned success-rate of primary amputation of the lower extremity, Guthrie was surely entitled to conclude

„that if the thirty-six of the forty-three who died and have only partially recovered [in this trial], would had been amputated on the first day, the country would have had at least twenty-five [i.e. 72%] stout men, able, for the most part, to support themselves by their labour, instead of five, or at most ten....“[94]

Since primary amputations of the upper extremities had given him up to 95% success, the case for a wide range of indications for amputation, and for primary amputation, was henceforth settled. Guthrie complimented himself for being in agreement with Baron Dominique-Jean Larrey’s (1766-1842) „decided manner with which he had supported the propriety of amputation on the field of battle“. He stated nevertheless that he had had no notice of Larrey’s publications until after the battle of Toulouse.[95]

These figures may easily be criticised, especially for considering all patients still under treatment as cured. Yet even if one takes into account that some of them must have died later and that the secondary operations were not done on the same type of case as the primary ones (see above), Guthrie’s precepts stood methodologically on a much firmer ground than those advanced by Larrey (see below). (The average rate of these losses was higher than one tenth. In the fifth edition of his book (1853) Guthrie estimated that one sixth of those remaining under treatment after primary operation must have died.[96] I have calculated that even if one sixth of those primary operated remaining under treatment had died, and none of those remaining after secondary operation, the mortality after primary operation would still have been three times less for the upper and 2.5 times less for the lower extremity, than after secondary operation). The trial with the 43 fractures of the thigh shows that in 1814, when the French surgeons would have amputated such cases, Guthrie was still busy ascertaining the value of this risky operation. He even recorded in print, on the termination of the Peninsula War in 1814, his regret that „we had not had another battle in the South of France, to enable me to decide two or three points in surgery which were doubtful“.[97]

It was the battle of Waterloo (18th June 1815) that „afforded the desired opportunity“. And indeed, although medically speaking, this battle was a disaster due to over-hurried preparation of the medical services. Guthrie, who volunteered in the Hospital of Brussels for five weeks, managed again to collect detailed records.[98] These were of all the operations recklessly performed there between 16th June and the 31st July. Guthrie distinguished accurately [Contrary to what Blanco states.[99]] between 146 primary and 225 secondary amputation. These were further broken down according to six anatomical localisations and were summarised in a clear table in which the proportion of deaths was calculated for each category. Although in itself this was the most complete and informative table published so far, Guthrie himself warned that it was rather an approximation to the truth, as it did not include those operated on who died on the battlefield of Waterloo and those who were transferred to the Hospitals of Antwerp.[100] But, as the *Edinburgh Journal* declared in 1816, praising Guthrie’s work, the proper time to amputate „seems now finally decided by the experience of military surgeons in favour of early and immediate amputations when the limb cannot be saved“.[101] This was especially so when in 1820 Guthrie published his data on officers. They showed that in the healthiest subjects, i.e. where according to the prevalent theory the most furious inflammatory reactions might be expected, immediate amputations could be done with greater advantage than secondary ones.[102]

In summary, Larrey and his English counterpart Guthrie, „the English Larrey“ as he was often called,[103] had come independently to the same general conclusions in very different ways and both changed the practice of their countries regarding amputation. To begin with, Larrey had let a short and accidentally recorded experience that disproved the similarly based concepts of his countrymen act upon him. For the rest of his life, the events always proved him right „once more“.[104] Guthrie took the recorded experience of many years, completed, revised and shaped it „with the support of all [his] junior medical officers, the approbation of all seniors under whom... [he] had serve, ... and the esteem and recommendation of most of ... [his]

equals".[105] He fully took advantage of the military conditions with their opportunities to study great numbers of cases for the elucidation of his question. John Thomson duly considered the resulting report „much more minute, accurate and useful than any to be found on the record of military surgery".[106]

On such grounds Guthrie found it easy to defend his views even against those of a John Hunter or a John bell. Hunter, he asserted, had written

„from his knowledge of principles... unbiased by any particular theory and from having had some opportunity of practice [during sex weeks in 1761, whereas Bell who] had no practice of his own and little opportunity of enquiry into that of others... [reasoned] from theory, probably on an individual case, and not from actual observation made on many."[107]

Yet neither were Park's exhortations of 1806 without effect, and at the end of the Wars the indication for amputation became more restricted. For instance „arms that would have been amputated in 1800 were preserved in 1814, from the knowledge acquired by experience of the liberties that may be taken with this extremity".[108] An open gunshot wound of an arm joint would have invariably been followed by amputation above the joint, or an exarticulation, according to the recommendations for the civilian surgeons or the rare accounts from military practice.[109] Guthrie and other senior surgeons such as John Hennen would alter recommend the excision of the shattered joint or the broken head of the humerus only. A functionally hampered arm was still better than none.

However, while the Army surgeons seemed to have resolved the great questions on indication and timing of amputation after gunshot wounds in their own way, the discussion on this operation did not come to an end. This I shall show in the next section, which deals first with a technical detail and then examines how the conclusions of the military surgeons were received by their civilian counterparts after the War was over.

D. Years of Relative Peace After 1815

1. Determination of the Time for Primary Amputation

The indication of primary amputation for severe gunshot wounds being quite unanimously approved of by military surgeons after the 25 years of warfare, there still remained some doubt on what „primary“ really meant. Should the amputation be delayed for some hours after the accident to allow the constitution to recover from „the shock of the injury as to bear the additional one of the operation, as Guthrie advocated for the practice in the Army?[110] Or was it to be performed as soon as possible after the injury, as had been the practice for forty years aboard the ships of the Navy? The naval surgeon Alexander Copland Hutchison (†1840) (see above) was surprised by Guthrie’s precepts „which appear to be founded on plausible reasonings deduced from effects theoretically supposed to occur rather than [on] the result of actual experience or just observation“.[111] [In fact, Guthrie had illustrated this particular point by only two problematic cases amputated „on the spot“and two successful cases amputated 4 hours after the injury.[112]] He decided to settle this question on the next occasion.

It presented itself soon when a British naval expedition to North Africa culminated in a battle before Algiers on the 27th August 1816. Hutchison had not himself participated, but sent off a circular letter on the 29th October to all surgeons of the eleven ships then engaged, [With the help of the London offices of the Commissioner for Transport, Sick and Wounded Seamen.] asking them four questions: 1) The number and nature of wounds needing amputation; 2) Was amputation performed during action or was it slightly delayed?; 3) Exact time after injury at which amputations were performed; 4) Number of recoveries and deaths and exact time of the latter. By the 18th November 1816, Hutchison was in the possession of returns from all but one ship (still at service abroad). An excellent collaboration! He published abstracts of all these returns, showing that one ship had amputated usually with several hours delay and had lost nine out of eleven patients, whereas two ships amputated immediately, and had lost only four out of nineteen amputations. Although he did not make a summarising table, he concluded that from the results of „.... the contrasted practice... so amply adduced, ... the question will now be considered by the profession as fully illustrated and finally settled... *Magna est veritas et praevalebit*“.[113]
[The detailed indications for the returns allowed me the following analyses:

1. Amputation „on the spot“ i.e. within the first 2.5 hours:

upper extremity	12,	cured	9
lower extremity	13,	cured	11

2. Amputation within 3-7 hours after the injury:

upper extremity	12,	cured	8
lower extremity	10,	cured	4

Secondary amputation (not from choice but from necessity)

Total	4,	cured	2

(Further anatomical breakdowns would be possible)]

Hutchison published these results in a monograph in 1817 [dedicated to Gilbert Blane]. One of the surgeons of Algeria even preceded him with an account of his ship in a paper read to the Medico-Chirurgical Society of London.[114] Guthrie, in the second edition of his Observations on gun-shot wounds replied to Hutchison in 1820, and the latter, in second edition of his Practical observations in surgery [This book contained plenty of valuable information based on statistical returns from the Navy.] re-replied in 1826.[115]

The character of a party question between Army and Nay that some apparently were disposed to read into this difference, was removed by the publication of Sir Stephen Hammick's (1777-1867) statistical report on his activity at the Naval Hospital in Plymouth from 1809 until 1829, wherein he admitted the necessity, in some cases „of allowing the patient to recover a little from the shock of the accident" previously to the amputation.[116]

Thus, as they did in the question of treatment of syphilis, British military surgeons attempted to resolve a technical question about amputation by the comparative numerical method. However, the numbers involved were sometimes small because of the relative peace during the next twenty years.

2. The applicability of military results to civilian practice

The conclusions of the military surgeons were accepted in the textbooks of civilian surgery with some warnings of the different conditions in military and civil accidents. The standard works of the time were the various editions of Samuel Cooper's *First lines of the practice of surgery* [First edition 1807, seventh edition 1840.] and of the *Dictionary of practical surgery*. [First edition 1809, seventh edition 1838] Samuel Cooper (1780-1848)[117] had received his surgical training entirely at St. Bartholomew's Hospital. At the beginning of the century he had served a short time in the Army and then set up practice in London, where the first edition of his two books had appeared. After the death of his wife he rejoined the Army in January

1813, served (under McGrigor) in the Peninsula and operated at Brussels during the campaign of Waterloo [where he must have met Guthrie (see above, p.??)]. In 1816 he retired on half-pay, became attached to a London dispensary and finally in 1831 succeeded Charles Bell (see p.??) as professor of surgery at University College London.

Meanwhile he had also worked up the several editions of his *First lines*, which was meant to be „an elementary work for students and a concise book of reference for practitioners“,[118] and of his encyclopaedic *Dictionary*, which was translated into French, German and Italian. In the *First lines* the change of contents due to the writings of the modern military surgeons is particularly striking. In the first edition Cooper avoided a clear discussion of the indication of amputation and of its timing. In the fourth edition (1819) he discussed both issues at length, not neglecting the indications for conservative surgery.[119] As for the indication of amputation Bilguer's arguments „however great their influence might once be... can no longer misguide any practitioner of common sense“. For this statement Cooper relied on Guthrie, „whose evidence is valuable, as being founded upon the great opportunities of observation and comparison of which he availed himself during the war in the Peninsula“, whilst criticising an American surgeon, who rejected amputation for gunshot wounds of joints „from one example“ only. As for the timing, Cooper mentioned Larrey's works as valuable and containing „most decisive facts“ in support of early amputation, while „nothing.. [could] be more unequivocal and convincing, than the important cases and observation“ in the writings of authors such as Le Dran, Ranby, Kirkland, Guthrie, Hennen and Thomson: „Reason, experience and authority are strongly against delay“.[120]

It is striking that Cooper should appreciate the evidence of the three former as much as that of the three latter authors. Yet he made a difference between authority and experience: concerning the applicability of these results to civilian practice he weighed up the available authoritive evidence saying. „If we can believe the testimony of the most experienced writers, especially Mr. Pott, we are bound to conclude that the operation should be done, in every case of this kind, „(because one could consider gunshot wounds as accidents). But he mentioned, as Guthrie had done, the lack of a planned trial comparing delayed with immediate operations in cases without hope for saving the limb. Cooper also mad it unequivocally clear that the results of amputation for injuries had nothing to do with those for chronic white swellings which were allegedly less dangerous and had hitherto provided an argument for delaying the operation for injuries. Only appropriate, direct inquiry could resolve this problem.[121]

The recognition of ignorance on this point shows how Cooper had moved away from pure rationalism, which would have continued to resolve the question by analogy.[122] Cooper accepted the clear-cut exposition of the problem of amputation forwarded by Guthrie and his way of finding the solution. In this respect, it is also noteworthy that as from the fourth edition of his *Dictionary* (1822), Cooper dropped a paragraph on mortality in amputation which had been included in the former editions and which had been very vague: that paragraph had stated that „perhaps, not more than one individual out of twenty loses his life after the operation even taking into account all those on whom it is practised in hospitals“. By 1822 he might have realised that such an indication was meaningless, for in the same edition he included for the first time Smith's, Marcet's and Prout's differentiated tables on the occurrence of lithiasis and the mortality of lithotomy.[123]

Cooper's position on amputation based on the data of his contemporaries is the more remarkable as he was not a champion of the knife. In this respect he attenuated even Guthrie's, Hennen's and Hutchison's recommendations, arguing that the factor in military practice of having to render a patient fit for transport did not apply in civilian life. And, in the foreword to the fifth edition of the *First lines* (1826) he exhorted

„the admirers of operative surgery... never to suffer their love of extraordinary feats with the knife, or their greedy desire of fame, to make them forgetful of the truth, that there is more real merit in removing the necessity for the practice of any one severe operation already familiarly adopted, than in the invention and performance of a hundred new ones.“[124]

Such remarks, aimed at the prevailing French interest in new surgical methods rather than being a reflection upon the real contribution to public welfare, can be found among other contemporaneous British authors, who had had occasions to witness their enemy's practice during the wars; „Too often their boasted success is only in their imagination“, wrote a British surgeon from the Peninsula.[125] But even fifty years earlier John Aikin, (1742-1822) a pupil of Charles White, in his *Thoughts on hospitals* had noticed the same trend in France.[126] The awareness had led in Pott's time to a warning to the students not to trust favourable cases only, which writers were in general too much inclined to relate, but to take into consideration also „how many sink for one that is recovered, and how many lucky circumstances must concur... to produce a happy event in... very deplorable cases“.[127] Through the works of the military surgeons these proportions were numerically indicated and related to some of the important exterior circumstances after 1815. Speaking of civilian surgery, however, it must be remembered that the incidence of severe injuries needing amputation was rare, that the indication for it in cases of white swellings of the joints was diminishing because of the possibility of conservative surgery, and that there were other scientific interests: for example as hinted at by Ackerknecht (1976) and Cranefield (1974), the leading British hospital surgeons around 1800 were pioneers in pathological anatomy, as were their Parisian colleagues, and even pursued experimental research in physiology.

Sir Astley Cooper (1768-1841),[128] one of the most brilliant London surgeons in this line, asserted in 1825 that amputation had become much rare than thirty years earlier. Due to better knowledge of the pathology of fractures and better treatment (to which he had greatly contributed),[129] amputation was only occasionally required in compound fractures of the limbs and compound dislocations. He thought it necessary for laceration of limbs from machines, for effects produced by firearms (rare in civil life), and for chronic and scrofulous complaints and malignant tumours.[130] He gave no further attention to gunshot wounds. As for the timing of amputation after civilian accidents, he clearly favoured delay:

„For compound fractures we seldom amputate directly; they are seldom so severe as to require immediate amputation, and it is not until gangrene or disease of the bone had taken place, that it is deemed necessary to amputate; compound fractures, however, treated do much better than formerly, and very severe injuries... will often terminate more favourably.“[131]

By then, Sir Astley had thirty year’s practice at Guy’s Hospital and we have his work but no results to underpin these statements. They show that the question asked by Guthrie for the Army and Samuel Cooper for civilian practice were not equally relevant to everybody, and that, with the cases of a civilian hospital alone, it was difficult to resolve them.

All this also holds true for the views of (Sir) Charles Bell (1774-1842),[132] another great London surgeon of the time. However with him there was yet another perspective, namely that of social difference between military and civilian surgeons. More outstanding perhaps as anatomist and experimental physiologist than as practitioner, Bell was the discoverer of the different functions of the anterior and dorsal roots of the spinal nerves.[133] The younger brother of John bell, he had been trained by him in Edinburgh prior to his move to London in

1804. There he lectured privately before receiving a hospital appointment in 1814 (to the Middlesex Hospital). In 1809 and 1815 he acquired some limited personal experience in gun-shot wounds when he went to Haslar to help treat the wounded arriving from the battle of Corunna, and - as John Thomson had done- during a journey to Belgium after Waterloo.

Bell's writings of his London period (1804-1836) reflected, as did those of Guthrie, Samuel and Astley Cooper, the teaching done there for he wrote them for his pupils and they were partly selections from his lectures.[134] Bell began in 1802 with a *System of operative surgery founded on the basis of anatomy*. As had his brother John nine years earlier, Charles expressed his perplexity and doubt on the questions regarding amputation, especially in cases of fracture, aneurysm and gangrene - which corresponded only to about 10% of the civil practice, the other 90% being cases of ulcers with carious bones, and white swellings.Yet, with Hunter, he believed that the idea of amputation in cases of compound fractures had been laid aside; anyhow he favoured the secondary operation in cases of accidents. In the second edition (1814) there was a complete change. Bell now admitted amputation if a joint was shattered and he was in favour of primary operation, too: „If a man receive an injury so severe that he must lose his limb, the sooner the amputation is performed, the better“. He included specific remarks on military surgery which reflected the practice of early amputation he had seen among naval and military surgeons. The Hunterian objection to amputation being an injury superadded was now circumvented by stimulants like wine, with the aid of which the patient's spirits and animation would soon return to allow the operation to be done.[135]

In 1816 and 1818 Bell published two volumes of *Surgical observations being a quarterly report of [selected]cases treated in the Middlesex Hospital...* As the title and the preface suggest, he meant to imitate the military practice of regular reporting which he knew of as he was acquainted with the military literature. He bitterly complained of not having received the clear returns of the twelve amputations he had performed at a British *ad hoc* hospital near Brussels thirteen days after Waterloo. With these cases he had meant to determine for himself the chance of success of secondary amputations.[136] According to him five patients of the twelve had survived, but some colleagues pretended that there had been 35 patients operated on. This led Bell to adopt an aggressive stance, especially against Guthrie whose book had appeared in 1815, and with whom he had a priority issue because of the technique of excision of the head of the humerus.[137] In his *Surgical observations* Bell admitted once more the rule that some military authors (i.e. Guthrie) had treated the subject too definitively. He denied them the right to set up strict principles for civilian - and even for military - surgery:

„Every hospital surgeon and teacher claims the privilege of examining these operations, and the principles on which the military surgery has been conducted. I have sacrificed much to see our navy and army surgeons on duty, and in doing so, I have shown my respect for them; but I will not lose the vantage ground which a laborious life has given me, nor yield it to them as principle of the profession.“[138]

He defended „the gentleman, who has written on .. wounds, the best perhaps of any“, and who was treated very cavalierly by some military surgeons, only because he was not one of them.[His brother John, of John Hunter, of Astley Cooper?] Since the 1750s the military surgeons, claiming the value of observation only, had „for want of a little scholastic discipline in aid of military ardour“ treated the subject always *de novo*. This had led to contradictory „evidences... arranged in opposite columns; and, when we seek to found on the authority of experienced military surgeons, we are jostled between them, and find no ground to rest on.“.[139] Bell added a series of pertinent questions, concerning post-traumatic phenomena and their pathophysiological mechanisms. But it was grossly unfair of him to blame the methods used by military surgeons for the fact that they were still unresolved. Rather their detailed results because of their very contradictions had drawn attention to existence of phenomena in injuries which were not assessable merely from figures of simple overall mortality.

The teaching at Edinburgh at that time probably did not differ very much from that in London. John Thomson, the professor of military surgery, would take Guthrie's views (see above, p.??), whereas Robert Allan (1778-1826) (see p.??), for instance, would be more prudent. Allan had actually been a naval surgeon from 1787 till 1805; then he became the pupil and partner of John bell. He was a public teacher of surgery in Edinburgh as surgeon at the Royal Infirmary after 1814.[140] His System of surgery (1821-1824) was designed as an elementary book, in parallel to Samuel Cooper's First lines. On the question of amputation he shared Charles and John bells' point of view that no definite rules of indication could be set up. As for the timing, too, there were according to him, high ranking advocates for both immediate and for secondary operation. What was sure was that one could operate safely before and after the inflammatory symptoms commenced.[141]

To summarise, in one way or the other civilian surgeons took notice of the results of theirs military colleagues. Be it scientifically or more outspokenly socially motivated, their reaction varied from full acceptance to complete rejection. In general, as in the question of contagion, civil practitioners by 1830 would probably hold a middle position, recognising that many questions were still unresolved. Yet I do not think that there had been no progress since, say, John Bell had left the main questions purposely open in 1798. Since it is easier to fight against a known enemy, the presentation of results in opposing columns, which his brother Charles criticised, helped o show more broadly and to shape the complex problems of amputation. It also showed that they could not be solved by comparison of simple overall mortalities. As I shall indicate in the next section, both military and civilian surgeons were aware of this. They would apply the numerical techniques in a more differentiated way in the next decades. Not in vain, perhaps, McGrigor and his staff had attended Charles Bell's lectures after the War.[142] McGrigor exemplified the method, bell a more general appreciation of the problems.

E. Conclusion and Outlook

As in lithotomy, the main technical problems concerning amputation up to 1830 had emerged in the 18th century: they concerned the time of operation and the innovations of the flap, the *prima intentio* and the joint excisions. Later than in the question of treatment of bladderstone, numerical statements became an element in the discussions on amputation. This was in the second half of the 18th century when an additional questions, inherent in all non-elective operations attracted attention namely that of the value of surgical as compared to conservative treatment.

Again, as in the case of lithotomy, the technical issues captivated much of the surgeons' interest during the whole period up to the 1820s. This interest found expression in numerical comparisons of unselected cases which had occurred in the 18th century. In Britain these reports came from authors such as Monro, or White and Alanson. The latter two were closely associated with what I have called the Warrington Group around Percival, Haygarth and Dobson. Compared with the contemporary reports on lithotomy or fevers these reports concerned only a very small number of cases due to the low incidence of severe accidents and an apparent lack of surgical information from the British military doctors of that time.

The question of the relative value of surgical and conservative treatment, introduced on a numerical basis by Bilguer, was dealt with by 18th century British authors non-numerically and only one-sidedly according to their scientific interest and temperament. The non-interventionists had the support of John Hunter. His argument, that such an operation was an injury superadded to an injury, was taken up again in the 1820s by Charles bell, for instance, as a reaction against the dominance that the „interventionist“ military surgeons had gained during the Revolutionary and Napoleonic Wars.

As had those of the mid-18th century, these Wars made the surgeons reconsider their opinions about amputation for injuries. The basis on which a position was taken by the leading British military surgeons was very much a continuation and enlargement of the tradition of 18th century British Naval and Army medicine and finally of the Edinburgh medical school: James McGrigor, the surgeon general of the British Army in the Peninsula was clearly aware of the scientific potential of statistics to elucidate clinical problems. He required regular returns from his superordinates amidst all the difficulties and unstable wartime circumstances, analysed some of them himself and encouraged others to do so. Therefore it is only superficially true that the result of the British and French experience concerning the treatment of compound fractures were the same, namely „early amputation when in doubt“: the British recommendation was based on a hundredfold greater number of cases, analysed numerically for example by Guthrie, who had drawn up a prospective programme. This basis was more distinguishing and therefore open to further differentiation, as shown by the example of the more precise delimitation of the time of „early“ amputation.

As in lithotomy, a number of new parameters such as anatomical localisation of the lesion, age and way of living of the patient, were now considered. In addition, the need for prospective comparative trials was recognised even if not pursued for ethical motives. The clear-cut statistical results of the military surgeons, contradictory as they sometimes were, laid open questions for further investigation. The equally clear-cut conclusions some of them drew impressed and challenged the view of their civil counterparts after the Wars, with the result that by 1830 it was admitted that most of the questions were unresolved.

A few examples will show that the numerical method continued to be used for their elucidation. In 1832 Benjamin Philipps (1805-1861)[143] a surgeon at St. Marylebone Infirmary, London, criticised in another context the fact that

„to the disgrace of the medical profession, no clarification of the symptoms, no observations of the complications, no comparative estimate of success of certain modes of treatment... was formerly made. [But] the importance of those subjects is now, however, fully admitted, and the laws of induction are rapidly attaining superiority.“[144]

Admittedly, his tables on the comparative frequency of complications in urethral disease drew a reviewer's comment that he did „not attach much value to such calculations, for we are not disposed to think the time very near when medicine will become an exact science“.[145] Such criticism shows, however, that the method as such was recognised to exist. Phillips was not disconcerted by such objections and he aimed, in 1836, to conduct a great study of amputation with the use of the only logically available material in peace time: the records of the great hospitals.[146] But there he was quickly disappointed: „When he commenced his inquiries there was no hospital in London, except that of University College, at which any information regarding the general results of amputation could be obtained“.[147]

At that time a thorough study of the question of amputation, as suggested by Samuel Cooper and Charles Bell, was already beng conducted, in a statistical manner, by Rutherford Alcock (1809-1897). He was the medical chief of the British expeditionary corps in the Carlist War in Spain (1835-1836). With Sir James McGrigor still head of the Army Medical Department this is no surprise, especially when we learn that Alcock had been a house surgeon at Westminster Hospital and a pupil of Guthrie, whose precepts were his guidelines when he joined the Army in 1832.[148] Alcock's detailed statistical reports, cleverly analysed, were published in 1838 and 1840. The results were also embodied in a series of twenty 'Lectures on amputation' published in extenso in the Lancet in 1840-1841. Alcock criticised Guthrie's recommendations, asserting that more numerous data and better analysis were required, taking into account a number of external circumstances and comparing conservative with operative treatment.[149]

The priority of the military surgeons, and the lithotomist in submitting their operations to the test of publishing their results was admitted by James Adair Lawrie (1801-1859) of the Glasgow Infirmary[150] in a much applauded statistical report before the British Association for the Advancement of Science in 1840.[151] It is noteworthy that Lawrie himself had started his professional career in the East India Company.

Preceded thus by Scottish and provincial hospitals in the late 1830s, and by University College Hospital, London, the results of amputations and other operations in the great London hospitals were also published in the early 1840s. All these statistics served as a basis for much needed research into the value of amputation for well described conditions.[152] This work preceded Malgaigne's *Etudes statistiques sur les résultats des amputations dans les hôpitaux de Paris* (1842); and, rather than being innovative, it was the product of the steady development which had its roots in British military medicine and in specific aspects of British hospital medicine of the 18th century.

F. References to Chapter Seven

[1] Garrison 1922, p.128; Wangensteen et al. 1967, p.101.
[2] Thomson 1816, pp.160-164; Dionis quoted ibid., pp.164-165.
[3] Billroth 1933, p.47.
[4] Le Dran 1737,pp.33,37,199.
[5] ibid., pp.33,224-225.
[6] le Dran 1731, pp.356-357.
[7] Le Dran 1737, pp.249-250.
[8] Billroth 1933,pp.48-51.
[9] Cope 1953,pp.49-50.
[10] Ranby 1744,pp.24-25,33,37.
[11] Home 1759, pp.113-114.
[12] Cope 1953,pp.49-50.
[13] Monro 1737,p.342;1781,p.481; Med.Ess.Obs., 4th ed., 4;276, (1752).
[14] Monro 1776-1784, pp.284-285.
[15] See e.g.Bordenave 1753, pp.526-527.
[16] Boucher 1753A; 1753B, Faure 1753; 1759; Bagieu 1757, pp.iv-v.
[17] Prix Acad. Roy. Chir. (Paris), 3;353, (1769).
[18] ibid., 489-520.
[19] Bilguer 1769, p.39.
[20] Vogeler 1929, p.752.
[21] For recent reference to the literature on Bilguer see Kaiser 1970 and Müller 1968.
[22] Vogeler 1929.
[23] Bilguer 1764, pp.32-33, 92-100.
[24] ibid., pp.37,59-62.
[25] ibid., pp.60-61.
[26] Quoted by Billroth 1933, p.51.
[27] Pott 1779, p.46; 1790, Vol.3, p.374.
[28] Pott 1769in: 1790, Vol.1, pp.349-477; 1779, pp.46,52,57.
[29] Bell 1868.
[30] Bell 1778, pp.246,396,398; 1783-1788, Vol.5, pp.347-356.
[31] ibid., Vol.6, pp.329-330.
[32] Koelbing 1978.
[33] Kirkland 1770, pp.18-19.
[34] Lyons 1963.
[35] O'Halloran 1765,pp.vii-viii, xxxix-xl.
[36] ibid., p.ix.
[37] ibid.
[38] ibid., pp.215-226.
[39] See ibid., pp.206-226, and Serre 1830 for historical studies on immediate reunion.

[40] O'Halloran ibid., pp.vii, 182-197,258-264.
[41] Lyons 1962-1963, p.32.
[42] Cullingworth 1904; Brockbank 1904, pp.27-65; Adami 1922; Sheehan 1942; see also this thesis, p.152.
[43] Brockbank 1904, pp.36,40.
[44] White 1771, pp.174-175.
[45] White 1770, pp.vi,viii.
[46] Peachey 1914; 1916.
[47] Bromfield, 1773, Vol.1, p.317
[48] ibid., pp.322-323.
[49] Anning 1963, Vol.1, pp.4-5,31,35,83.
[50] Lucas 1776, p.328.
[51] Med.Obs.Inq., 6; 250-265, (1784).
[52] Power 1937; Oldham 1977, pp.20-24.
[53] Serre 1830,p.24.
[54] Power 1936.
[55] Oldham 1977, pp.17,19.
[56] Cohen 1972.
[57] Oldham 1977, pp.25.
[58] Alanson 1782, pp.vii-viii.
[59] ibid., p.xiii.
[60] ibid., pp.xv-xvi.
[61] Bell 1783-1788, Vol.1, pp.v-vi, Vol.6, pp.334-336.
[62] ibid., Vol.6, pp.334-336.
[63] Serre 1830, p.38.
[64] Park 1783; Park and Moreau 1806; Sheehan 1942.
[65] See e.g. White's and Lucas's tables in this thesis pp.409,410 and Clark 1802.
[66] Gask 1950; Cantlie 1974, pp.133-134.
[67] Hunter 1794,p.562.
[68] Callisen 1830-1845, Vol.20, pp.526-527.
[69] Weldon 1799, pp.40-42.
[70] Chevalier 1804, pp.97-99,102.
[71] Park and Moreau 1806, p.64.
[72] ibid., pp.64,66-67.
[73] Neale 1804, pp.xv-xvi, 221-222.
[74] Hennen 1829, p.17.
[75] Bell 1787-1789, Vol.6, p.130.
[76] Hennen 1829,pp.42-43.
[77] Hennen 1818B, pp.11-12.
[78] Lond.med.Rev.Mag., 3; 261, (1800).
[79] Walls 1964.
[80] Bell 1812, pp.424,452.
[81] e.g.ibid., p.462 on behalf of Boucher.
[82] See e.g. Lond.med. Rev.Mag., 3; 261, (1800); Guthrie 1815, p.22.
[83] Bell 1812, pp.469-475.
[84] ibid., p.491.
[85] Blane 1799, pp.581-582.
[86] ibid., pp.582-583.
[87] Rollo 1801, p.114-117.
[88] Lancet, i; 726-736, (1850); D.N.B. Vol.8, pp.818-819; Cornelius 1968; Blanco 1974, pp.156-158,173-177.
[89] Guthrie 1815, pp.viii,38.
[90] ibid., p.39.
[91] ibid., pp.39-45,47.
[92] ibid., p.312.
[93] ibid., p.192.
[94] ibid., p.194.
[95] ibid., p.xxii.
[96] ibid., p.45; Guthrie 1853, p.152.
[97] Guthrie 1853, p.v.
[98] ibid., p.vi.
[99] Blanco 1974, p.158.

[100] Guthrie 1853, pp.150,154.
[101] Edinb.med.surg.J., 12; 221,(1816).
[102] Guthrie 1820, pp.150-152, Tables I and II.
[103] Blanco 1974, p.156.
[104] Tröhler, unpublished observation.
[105] Guthrie 1838, p.9.
[106] Thomson 1816, p.223.
[107] Guthrie 1820, pp.81-83.
[108] Guthrie 1853, p.513.
[109] Neale 1804, pp.221-222.
[110] Guthrie 1815, p.47.
[111] Hutchison 18170 p.5-6.
[112] Guthrie 1815, pp.24-25,49-50.
[113] Hutchison 1817, p.16.
[114] Quarrier 1817.
[115] Hutchison 1826, pp.61-70.
[116] Hammick 1830, p.39; Ballingall 1852, pp.391-392.
[117] S.A. Lane in: Cooper 1872, Vol.2, pp.ix-xii; D.N.B. Vol.4, pp.1073-1074.
[118] Cooper 1807, title page.
[119] ibid., pp.205-209; 1819, Vol.1, pp.196-204.
[120] Cooper 1819, ibid., pp.197-198.
[121] ibid.
[122] King 1976.
[123] Cooper 1818, p.27; 1822, pp.1101-1103.
[124] Cooper 1819, p.202; 1826, pp.iii-iv.
[125] John Murray to Benjamin Travers, quoted by Crowe, in preparation.
[126] Aikin 1771, pp.28-30.
[127] Pott 1763, pp.120-121.
[128] Brock 1952.
[129] Dlugatz 1968.
[130] Cooper 1825, Vol.2, pp.414-415.
[131] Cooper 1832, Vol.2, p.404.
[132] Gordon-Taylor 1958.
[133] See Cranefield 1974.
[134] Bell 1814, Vol.1, p.ix; 1816, p.v.
[135] Bell 1807, pp.336-337,369-370; 1814, Vol.2, pp.1-2,6,494-496.
[136] Bell 1816, pp.322-323,327,329.
[137] ibid., pp.236-237; Gordon-Taylor 1958, pp.93-95.
[138] Bell ibid., pp.227-228.
[139] ibid., p.334.
[140] Allan 1821-1824, Vol.1, p.viii-ix.
[141] ibid., p.vii; Vol.2, pp.74-76,81.
[142] Gordon-Taylor 1958, p.68.
[143] Lancet, ii; 26, (1861).
[144] Phillips 1832, p.226.
[145] Lond.med.Gaz., 13; 84, (1834).
[146] ibid., 2nd series, 2;458, (1838).
[147] Lancet, ii; 342, (1840-1841).
[148] Alcock 1838, pp.60,62,70.
[149] ibid., Alcock 1840; 1840-1841.
[150] Coutts 1909, pp.530-531; Comrie 1932, Vol.2, p.525.
[151] Lawrie 1841; Lancet, i; 57, (1840-1841).
[152] Phillips 1837; 1838; Lawrie 1841; Parker 1841; Potter 1841; concerning the operations in the great London hospitals 1840-1841 see Woodward 1974, pp.84,91.

CHAPTER EIGHT: CONCLUSION

A. The Problem

As pointed out in the first chapter, the principal reasons traditionally advanced for the „delayed" introduction of quantification into therapeutics and nosography needed to be questioned, and with them the existence of such a delay itself. In the second chapter I outlined why I concentrated my inquiry on particular aspects of British medicine between 1750 and 1830, which seemed potentially rewarding fields; those aspects were fevers, scurvy, syphilis, midwifery and the major surgical operations within the growing towns and in the Army and the Navy, as they were the major medical problems of that period.

B. The Phenomenon

The results of my research, presented in chapter three to seven, confirm the validity of my working hypothesis. There is hardly any doubt but that a number of British doctors, civil and military perceived in the second half of the 18th century the need for adequate verification of the value of empirical or theoretically founded remedies. They saw extensive, comparative trials with results expressed by numbers as the only way to this end. With the same motive of verification, they also used numerical methods in nosography. Neither of these early applications of quantification to resolve clinical questions in medicine and surgery has, to my knowledge, hitherto been sufficiently recognised.

It was around 1780 when some doctors saw an answer to the contemporary discussions on methodology and certainty in clinical medicine in the application of arithmetic. They did indeed speak of „medical arithmetic". Since they also stressed, in the prevailing spirit of the Cullen school of rational empiricism, the primacy of observation of facts over speculative theories,[1] I have occasionally called men like Millar and Black, John Clark, Haygarth and Percival, Alanson and Dobson, Robertson and Blane, Charleton and later McGrigor, Ballingall and a number of their younger pupils all „arithmetic observationists".

What were the distinctive features of arithmetic observationists? Mere observations of facts, even if their number increased as shown in my analysis of four medical periodical (1733-1830), were insufficient for them. They wished to put clinical medicine on a basis of elementary numerical analysis of such facts. In this, they saw themselves making a departure from the acknowledged methods of reporting, especially from the authoritative aphorisms of Boerhaave (and to a lesser extent from those of Hippocrates and Sydenham), in the spirit of Francis Bacon. In 1777 John Millar answered a question often asked in the period: What is the way out of the maze of observations opposed to observations, facts to facts? His response was that „detached cases, however numerous and well attested, are insufficient to support general conclusions.... The test of arithmetical calculation [ought not to be] evaded“.[2]

In therapeutics this test would involve mathematically the formation of sums, the calculation of arithmetic means and possibly of ratios, (e.g. success-to-failure ratios). In nosography the occurrence of certain symptoms would be expressed as a fraction of the number of cases studied. The arithmetic observationists would rely on results numerically expressed, if possible in tabular form, as they realised that this was the sole concise method of including all cases which had occurred during a given time. Cullen’s nosology would provide John Clark or James Clarke with a skeleton for the arrangement of diagnoses in full dispensary or hospital reports. As their analysis would seldom give uniform results, they would emphasise the necessity of further inquiries. Those who belonged to the generation at the height of its activity around 1780 would write on medical arithmetic programmaticality, whereas after 1800 the method continued to be used generally without many programmatic accessories, as a „standard“ technique, by civilian and military doctors right up to the time of Louis.

It has been noted by Ackerknecht that the numerical method was used in France before Louis, yet that with him it became the central interest of research, the basis of medicine.[3] This, in my opinion, may also be claimed for Millar, Black, John Clark, Robertson and Blane, and McGrigor. For Black the „science of Medical Arithmetick“ was the dawn of a new era in medicine. It was opposed to reverence for oracular aphorisms (Boerhaave!) and for the mere opinions of individuals. Black asserted that „however it may be slighted as an heretical innovation, I would strenuously recommend Medical Artithmetick, as a guide and compass through the labyrinth of therapeuticks“.[4] As they were for Black, arithmetics were for Millar the candle which could save the investigator from groping in the dark. Robertson deemed Millar to merit the title of the Sydenham of his age.[5] He made no secret that he used his position in the Navy for large-scale therapeutic experiments - and thereby becoming a science like any other. Currie and McLean, too, would speak of clinical experiments.

The existence of opposition against the open publication of results of hospital treatment from around 1780 (Donald Monro against John Millar) down to the 1820s (Charles bell against John Hennen), suggests that contemporaries saw the distinctiveness of the method. But despite such opposition the method spread steadily.

Indeed, its early propagators were not isolated workers. Rather they knew each other personally or through their writings. Two eminent groups were the Medical Society of London and the „Warrington Group". They promoted each other's works, orally and by including them in their publications. Furthermore, around 1800 the method became a „must" among the scientifically interested doctors in the Army (including the Department of Ordnance) and in the Navy; and there was not, after 1815, a head of their medical departments who had not championed or used it. One might, therefore, speak not only of arithmetical observationists but of an arithmetic observationist „infiltration", or movement.

As does any definition of a historical movement, that of the arithmetic observationists (which I have described as the saw themselves, in the 1780s) raises at least three main questions: !9 What were its origins? 2) What men promoted it? 3) What was its significance? These questions I shall try to answer in the next three sections of this concluding chapter.

C. The Origins

This question helps to delineate further the distinctiveness of arithmetic observationism in clinical medicine. My introductory remarks showed a number of reasons why, broadly speaking, British medicine during the reign of George III seemed not a priori unfavourable to the introduction of this method. Now I shall scan the evidence presented in the foregoing five chapters for specific interrelations.

Would James Lind, for instance, fall under the heading of the new „ism"? This is a valid question, for although in 1763 Lind admitted „historical" facts (i.e. properly recorded observations) as „the only basis of all physical disquisitions",[6] he devoted much time and space to speculative theories of scurvy. In clinical descriptions and therapeutical trials on this disease, although he constantly referred to a great number of experiences, he did not include precise figures, except in his first trial with the twelve patients. Nevertheless, I think that my question can be answered positively form the consideration of Lind's other works.

In 1762 Lind added a hospital report as a postscript to the second edition of his Essay on... preserving the health of seamen, namely the list of the 5,743 patients treated at Haslar from 1758 till 1760 which was broken down according to 37 diagnoses. [Excluding the venereal and surgical diagnoses.] His aim was to show that diseases of seamen were no different from those of non-seamen.[7] Especially in his works on fevers Lind relied on numerical data - although they are sometimes vaguely expressed. But his precise trial with opium, his results of the treatment of typhus in the Russian fleet, and his statement that „The best proof of the efficacy of any method, is the success with which it is attended“ followed by a mortality rate, definitely marked a departure from Boerhaave and even, in this respect, from Pringle, who did not give results of their recommendations apart from the description of individual cases. Furthermore, in his late Essay on diseases... in hot climates Lind relied on the „very accurate“ journals kept by such arithmetical observationists as Robertson and John Clark. Lind’s hospital was also the source of numerical information for others who worked along this line. Blane, for instance, addressed himself to Haslar for data on the fatality of certain diseases and operations to be compared with his own returns;[8] and compilatory textbooks, like Manning’s (1780)[9] or Turnbull’s (1806), included some of Lind’s statistics.

Theoretically, Lind gave the important example of a planned „controlled“ trial. As did Hillary he stressed the scanty value of single cases for the evaluation of therapy. He realised that any statement thereon could only bear a character of probability; and such probabilities Falconer and Haygarth tried to calculate. It is true that he did not cement these views into a published theoretical concept of arithmetical observations, but he acted frequently as if he had. Indeed, a historical movement is seldom found without any warning or precursors.

Such warnings were contained in the works of Lind’s contemporaries Pringle, Cleghorn, Home, Brocklesby and Hillary. But they went even further back to Clifton and Alexander Monro primus. In this respect the inquiries into the weather - disease relationship, already mentioned, merit attention It is probably no coincidence that Lind,[10] Pringle, Robertson and John Clark all reported regular readings of the weather parameters, some of which were analysed arithmetically. Such readings were alter also included by Rollo, Withering, Chisholm, Thomas Clark and McGrigor. Blane and James Clarke related their periodical analysis of returns to the maximum-minimum reading of temperature during the period in question. Regular weather readings were already a feature of the much read Medical Essays and Observations edited by Alexandre Monro primus. Clearly Monro was at least a precursor of arithmetic observationism, both in theory and practice. His emphasis on reporting all the results obtained with a particular treatment over a certain period was important. As noted by Pearson (1978), it is especially in his answers to the questions on inoculation by the Paris Faculty that he stands out „with something like a [modern] scientific frame of mind".[11] [Pearson erroneously attributed this work to Alexander Monro secundus] It is apparent from his replies that Monro saw the importance of statistics in medical investigations, for he had recourse to opinions and impressions only when he had nothing better to offer.

This leads us to another root of arithmetical observations in clinical medicine, the question of smallpox, which was indeed being discussed on that basis since the 1720s. The correlation between smallpox and arithmetical observation was less prominent with the Army and Navy doctors than with the civil doctors I have studied. As was the correlation between the weather recordings and arithmetical observations, it is not per se relevant. There were many doctors keeping registers of weather parameters and many writers on smallpox, who were not necessarily clinical arithmetic observationists in the sense of my definition. The interest in the weather and the discussions on smallpox were both too widespread among contemporaries and too restricted a field to justify the establishment of a significant correlation, if there were no specific incidences. Lettsom, in his Memoirs (1774) entered at length into the question of using calculations from vital statistical data for the evaluation of inoculation.[12] John Clark, in 1780, considered the evaluation of inoculation. John Clark, in 1780, considered the evaluation of inoculation as an example to be imitated generally in clinical medicine. Black, in 1789, directly attributed his discovery of „the great utility of medical arithmetic" to the „violent literary warfare" respecting inoculation. With Millar, (1769) there is similar but circumstantial evidence. Haygarth later applied his calculations on the contagiousness of smallpox to prove the contagiousness of continuous fever.

In the context of the value of inoculation frequent reference was made of the London Bills of Mortality. The medical interest in them was activated in the 1750s when the bills from 1601 were twice reprinted.[13] At the same time their inadequacy for medical inquiries was realised by Millar (1769), Leake (1772), Percival (1773), Lettsom (1774)[14] and others, whose arithmetic observationist programmes involved the application of better bills to hospital practice. The bills were a major tool in the hands of medical and social reformers, both for showing the needs for reform and eventually for its success. Yet again, the case must not be overstated. The notorious Dr James used the bells to suit his own ends. And a social reformer, or a medical reformer, using vital statistics, eve n if he were a doctor, did not need to be a clinical arithmetic observationist; only sometimes did he happen to be both.

From the point of view of social reform, the case of smallpox ultimately falls under the heading of social and preventive medicine, or public hygiene, - a product of the Enlightenment's increased attention to diseases of the community, especially of the poor. I have singled out inoculation only because of its quantitative importance and its chronological primacy over other preventive measures which also yielded numerical results. It was in his dealing with the prevention of fevers that Pringle started to work numerically, illustrating how by 1750 the old methodology still persisted in his own approach to therapy whilst the new one was coming up. The same transition could also be observed in Lind's work on fever, and in a number of authors on perpueral fever. The correlation between the Bills of mortality and the completion of „clinical bills", to favour both social and medical reform and to improve medical knowledge, is quite obvious in the work of Black, Lettsom and Millar (who furnished their won data for comparison with the Bills) John Clark, Percival, Haygarth and Blane,[15] and the promoters of specialist hospitals for children, midwifery and fever patients.

As a matter of fact the idea of „reform" or „change" was probably the strongest and most frequent of the roots of the arithmetic observationist movement in clinical medicine. This included reform of medical care for the poor, reform of the traditional treatments of diseases, reform of medicine as such and of its professional organisation, and the elimination of quacks. The laborious task of compiling statistics was always undertaken initially with a definite aim, even if it was only the hope of some pecuniary reward, as in the cases of the French lithotomists around 1700, of Carmichael Smyth and of the physicians treating the soldiers affected with ophthalmia.

For Millar, medicine was just one amongst many fields in which reform was necessary. This is illustrated by the title of his collected works (1802-1803?) which reads: Observations on the change of public opinion, in religion, politics, and medicine; on the conduct of the war; on the prevailing diseases in Great Britain; and on medical arrangements in the army and navy. The emergence of the phrase „public opinion" in the latter part of the 18th century has been noted by Brock: "The demand for information about public affairs increased", he has written; „so did the capacity to meet it; for among the industrial processes revolutionised were those of paper making and newspaper printing“.[16] Such demand for information applied to medicine as well as to politics. As “proprietary politics“ came under attack, the “proprietary medicine“ of the secret remedies and nostrums was criticised and with it sometimes the confused state of scholarly medicine as a whole. Arithmetic observationist, by publishing the results of their practice, aimed at answering both types of criticism. As shop arithmetic offered the tradesman a clearer view of his business, as political arithmetic made possible the collection and interpretation of mass observations for the guidance of the statesman, so would medical arithmetic enable the doctor to elevate medicine to a higher lever of certainty. In this perspective, the coining of the term „medical arithmetic“ by Millar and black at the same time as increasing trade, political and social reform debates and a growing awareness of „public opinion“ on economic issues was surely no coincidence. Neither was it an accident that they referred to the vital statistics of the Reverend Price, which were, for instance, used in the debates on the Poor Law at the end of the 18th century.[17]

Finally, besides all this evidence for an altruistic spirit of reform fostering the use of figures in clinical medicine, one ought not to forget the tangible reform of these men’s own status and of their place in society, which many of them must also have had in view. There was the question of getting on in the world after all, and because English society of the 18th century was a dynamic, open society,[18] there was always a possibility of getting on, provided one had achieved something with which to impress a potential patron. But, as was well expressed by the unlucky Rowley, there was a strong case for a more extensive meritocracy to counter the dominance of patronage and nepotism.[19] In either case, one way for advancement as a doctor was to prove, most plainly by figures, one’s success for instance in saving lives by creating and running a dispensary or by curing a particular disease. These considerations lead us naturally to an inquiry into the personal social background of the early arithmetic observationists.

D. The Men

The fact that the early arithmetic observationists knew each other - or at least knew of each other - justifies the question whether they had common social roots, which might also partly account for their common social and professional aims. I am not able, yet , to give a full social history of the men involved in the movement I have described. Nevertheless I can draw a picture which seems to emerge quite clearly from a cursory analysis.

The „typical“representative was either a Scot or a man of provincial stock. His family background was not so modest as not to allow him to be apprenticed to a local doctor and to study for some time at Edinburgh. Then he entered the Army, the Navy, or if he was lucky the East India Company, with the rather low status of surgeon. There, he met with a number of colleagues hardly trained at all. After some year's service, at the settlement of a peace for instance, he would set up in private practice. Alternatively, he might have chosen to go into private practice at once (which would not have made much difference in the quality of his professional colleagues). He would then write up his experiences, hoping to attract some interest from the few leading colleagues and from the general public. Yet getting a practice off the ground was not easy, neither in the provinces nor in London, without the necessary prestige of a hospital appointment. Free practice among the poor, in a dispensary or a specialised charity, thus proved a valuable beginning, and offered opportunities for treating many cases. Such opportunities could also be found in one of the naval hospitals. None of these institutions precluded their medical staff from private practice. Thus a professionally and socially satisfying career seemed possible, even if one had not studied at Oxford or Cambridge and was, therefore, not eligible as FRCP, or one did not hold an appointment at one of the great London Hospitals. A career was even more interesting for those who took an active part in the foundation and life of a new medical and/or literary-philosophical society.

This picture covers the beginnings of the careers of the London arithmetical observationists such as Millar, Black and Lettsom, of the naval surgeons Lind, Robertson, Blane and John Clark, and of the provincial men such as Percival, Haygarth, Alanson, Dobson and Falconer. There were exceptions: Rice Charleton in Bath for instance was an Oxonian. He had been a FRS for ten years before he was elected physician to the General Hospital there in 1757. However, the picture is fairly accurate even for the precursors of this movement (Clifton, Pringle, Monro), with the exception that they held degrees from Leyden, and for the later promoters of the method in the Army such as Rollo, McGrigor, Jackson, Chisholm, Ballingall and their associates.

As for doctors in general, none of the arithmetical observationists was born into the landed society. All started as relatively insignificant, or indeed „marginal“men. As pointed out recently by Inkster in a study on 'The social role of the medical community in Sheffield', provincial doctors around 1800 were marginal twice over, for they were both striving for individual status and members of a profession yet in the making.[20] But the men I have examined must have had strong ambitions to move upwards socially (besides their already mentioned desire to differentiate medicine from quackery). This is testified by the mere existence of their scientific works, by which they distinguished themselves form the majority of ordinary practitioners and of Army and Navy surgeons. The arithmetic observationist method required particular organisational skill and constancy of will simply to compile the raw data. Not surprisingly, some of its promoters were perhaps more important medical organisers than medical discoverers or scientists in the conventional use of these terms. In this respect the recognition of the opportunities that hospitals and military conditions afforded for clinical trials and experiments as early as the late 1750s, by Gooch,[21] Lind and Pringle, and which was continuously repeated afterwards, is noteworthy. In his Medical ethics (1803) Percival recommended the devising and subsequent trial of new remedies, and new methods in surgery, in public hospitals as being „for the public good“, provided they were scrupulously and conscientiously carried out, according to his plan of record-keeping and periodical analysis directed against the reliance on unconnected single cases.[22]

If one looks now at the careers of the early medical observationists, one sees that a number of them made their way up successfully. From the evidence presented I am tempted to speculate that the clear-cut presentation of the result of their work was not irrelevant for the promotion of military men like Robertson, Jackson, Guthrie, Hennen, and even Lind, although the latter became physician to Haslar before his explicit arithmetic observationist work. Blane, of course, was the first former naval surgeon, McGrigor the first active Army surgeon, ever to become knighted. With doctors who had stayed in civil life or who returned to it, this correlation is less evident. Lettsom was an F.R.S. and F.S.A. before he published his first dispensary report. Black and Millar remained relatively undistinguished physicians, yet they are mentioned in the D.N.B. which accords them, as it did Lettsom, successful practice in London. [Lettsom had also passed the L.R.C.P. in 1770, Black in 1787. Munk's Roll does not mention John Millar]. John Clark eventually found his way into the old Newcastle Infirmary. Percival and Haygarth, Alanson, Dobson and White equally rose to local importance. Cheyne became physician general to the forces in Ireland, a post of the highest authority in the country.

Another aspect of the marginality of the early arithmetic observationists is shown in their involvement in the lives of their own (medical) societies. This is ably illustrated by Millar's definition of the scope of the Medical Society of London. Lind, Robertson, Lettsom and black in London, Blane in Edinburgh and London, McGrigor in Edinburgh and Aberdeen, John Clarke in Newcastle, Percival and White in Manchester, London and Warrington, Alanson, Currie, Dobson and Haygarth at Warrington, took an active part in the foundation and/or early life of such (interrelated) circles. Black, Bland and Leake[23] gave private lectures in their houses. Lind and Rollo taught at their hospitals, others at their dispensaries. They all thus created a variety of diffusing, training and perfecting facilities outside the more established avenues for discussion and publication (Royal Society, Royal College of Physicians) and for training (Surgeons' Company).

Furthermore, being Scots, Quakers (Lettsom, Fowler) or Unitarians (Currie, Ferriar, Haygarth, Percival and Rigby) most of them were dissenters in one form or another. [John Clark belonged to the Episcopal Church of Scotland, thus being a dissenter by Scottish standards. He sent his children to a Unitarian school.[24]] They illustrate in medicine Walt's general statement that in the 18th century dissenters, by their very exclusion from established centres of influence and power, were encouraged to make a distinctive contribution to the nation's educational, scientific, industrial and commercial progress.[25]

In this section I have looked chiefly at the early arithmetic observationists as a quite homogeneous social group. Once the method was recognised and championed openly by the senior physicians of both the Army and Navy, its consistent use by ambitious and talented younger men became a means of advance, as John Hennen formulated it. The Sheffield study by Inkster (1977) suggests that this also continued to be the case for marginal men in civilian medicine. But the question whether, and when, the use of arithmetical observation, or as it was called in 1830, statistics, became a good means of professional and social advance for British civilian clinicians would require further study. Yet it leads me, finally, to inquire into the relevance of this arithmetic observationist infiltration.

E. The Significance

Asking about the relevance of a historical phenomenon immediately entails a series of further queries: Relevance for whom or what for? And in this thesis one might ask: relevance for the doctors themselves, their colleagues, their successors, their patients, or their discoveries? Relevance for a particular direction the subject took later on, and even, relevance for the physician of the present day? I have already hinted at speculative answers to most of these questions. Much research would be necessary to render them less tentative. For instance, the place of Louis and his pupils as innovators ought to be reconsidered in the light of these earlier British arithmetic observationists - and possibly of those of other countries, as might be inferred from recent studies on French surgery at the end of the 18th century, and on records of the weather-disease relationship,[26] as well as from the two surgical chapters in this thesis.

William Poulteney Alison (1790-1859) wrote in 1833 'A dissertation on the state of medical science'. He had started his professional career in 1815 as a physician to the newly founded New Town Dispensary at Edinburgh and had published the 'Quarterly Reports' of this institution in Bateman's style. From then he took a deep interest in the fevers of the poor.[27] He was to become a great reformer of Scottish Poor Law in the 1840s. By 1833, he held the chair of the Institute of Medicine (i.e. pathology and physiology) at Edinburgh. For these reasons he may be considered as a competent analyst. He indeed perceived the particular merit of the early comparative statistical inquiries into community diseases by Robertson, Percival, John Clark, Blane, McGrigor and Luscombe and of those by his own contemporaries Johnson and Bisset Hawkins in England, and Villermé in France. The latter three were thus seen within a long line of development rather than as the originators of a new line. This was also true in respect to the late improvements of the knowledge of fevers: Alison praised the clinical work on the discrimination of varieties of fevers by the Dublin physicians and by Bateman in London in the same sentence in which he mentioned that of Louis and others in France. And he did not particularly stress the latter's numerical method.[28] And, after all, is it not known that Louis condemned bloodletting on statistical grounds, but continued to use it?[29]

I am aware that I have described British hospital medicine between 1750 and 1830 somewhat one-sidedly by focusing on arithmetical observationists (though this criticism does not apply to my account of Army and Navy medicine). I shall now attempt to assess the impact their method made on the contemporary British medical world, both theoretically and practically.

As for the method as such, my evidence for the echo it created in the 18th century is limited. As mentioned in chapter three, I have not found direct reference to it in the lectures of Cullen or Gregory in Edinburgh or John Hunter in London. Important figures of the London establishment such as William Heberden father (1710-1801) and son (1767-1845)[30] did not, to my knowledge, mention or use it in their published writings, although they took an interest in vital statistics as manifested by the financing of the reprint of the Bills in the 1750s by the father, and by a book on the *Increase and decrease of diseases in London* (1801) by the son. The latter shared this interest with Robert Willan, who was the first to make a systematic study of health conditions in London, published from 1796.[31] (But Willan also was a „marginal man"). But I have not made a systematic study of the works of the physicians and surgeons of all London dispensaries, nor of the great London hospitals. I accepted the complaints about the absence of accurate record-keeping repeated by insiders of the latter such as Blane and Robertson in the 18th century, by Marcet around 1820 and by London hospital surgeons in the 1830s. This absence was furthermore confirmed by the Parliamentary inquiry of 1818 mentioned in chapter three. It was also criticised by the Lancet and the Edinburgh Journal in the 1810s and 1820s. From this absence one would be inclined to conclude that the physicians to these institutions were not particularly interested in arithmetical observation. Neither have I made a systematic enquiry into the works of the Fellows of the London College of Physicians during this period. Both groups, which were partly overlapping, may hardly be supposed systematically to have produced works on the lines of the arithmetic observationists who in turn did not refer to them in that context. But that there might have been occasional contributions or remarks on the method cannot be excluded from my analysis of the Medical Observations and Inquiries.

In general I could find limited assistance for my enquiry on the significance of the method from the secondary literature of the late 18th century. The Gentleman's Magazine, the Annual register and the contemporary editions of the Encyclopaedia Britannica are of no particular avail. I consulted also the Medical and philosophical Commentaries by a Society of Physicians of Edinburgh (1774-95), continued as Annals of Medicine (1796-1804), which were the most important review journals of British medical literature. They were published in several editions, simultaneously in London, Edinburgh and Dublin and were the forerunners of the Edinburgh Journal (1805-1855). Unfortunately the reviews in the two former periodicals were chiefly analytical. Yet from the selection of the books included, and from occasional remarks, one may get some impression of the echo these books created.

Lind's and Millar's books were not reviewed, but John Clark's were included twice (1774, 1780), the second time with some very flattering remarks.[32] Percival's and Lettsom's work on mortality in and around Manchester and at the Aldersgate Dispensary was reviewed equally well,[33] as were Dobson's on fixed air and William Black's on smallpox.[34] However, there were no specific remarks on their methodology, although a section of Dobson's numerical work was reprinted in the review. By contrast, Fowler's and Withering's works were the subject of repeated methodological comments. Upon reading Fowler's first book on tobacco, the reviewer thought that his manner of introducing this new medicine „may justly be considered as a discovery of very great utility". He agreed with the author that still more facts were necessary, but concluded that whatever further workers would find out, Fowler was „still entitled to much praise as a faithful and industrious observer". Withering's *Account of the foxglove* earned similar appreciation.[35] Both authors were again quoted in relation to Fowler's second and third *Medical reports*.[36] Fowler received a rare and favourable comment: „We cannot too highly applaud the industrious zeal with which he has endeavoured to render hospital practice subservient to medical improvement".[37]

Gilbert Blane's *Observations* received an even longer and very favourable comment concerning the necessity for mass observation. This review of seventy pages reprinted Blane's programmatic statements *in extenso*.[38]

The *Annals* also accepted original papers. There were analytic reviews of McLean, Chisholm, James Clark, Carmichael Smyth, Fowler, McGrigor and Haygarth. The latter was much praised for his calculating the probability of the contagiousness of typhus.[39] Fowler's work on arsenic was also singled out as an example for others to follow in Hamilton's *Duties of a regimental surgeon* (1787).[40]

The *London Medical Review* (1799-1802,1808-1812) included an analysis of the third edition of Blane's *Observations* (1799). His method was seen as having set an example worthy of imitation, and its principles were again fully reprinted. The rest of the valuable contents of the book was presumed by the reviewer to be familiar to most of his readers in the medical profession.[41]

It is tempting to conclude from this limited evidence that 18th century doctors must have been aware, and made aware, of arithmetical observation. With the exception of the opposition mentioned by Millar and Black themselves, it seems to have been appreciated in therapeutics as well as in nosography. This inference may be supported by consultation of the more explicit early 19th century review literature.

The short-lived *London Medical Review* thought Rollo's account of the arrangements and results of the Artillery Hospital at Woolwich valuable enough, that it „might be read with advantage by all persons concerned in the establishment or regulation of an infirmary". It was thus fully reprinted, inclusive of the tabular hospital report.[42]

The *Edinburgh Journal* as from 1805 continued the policy of the *Annals* but printed critical book reviews instead of mere analyses. It described James Currie's *Medical reports* as „one of the most valuable [books] which has ever been published... the style and manner should be imitated".[43] Haygarth's *Clinical histories* was seen as one of the too rare exceptions where a doctor, possessing extensive opportunities for observation, did not simply hurry from patient to patient, write fashionable prescriptions, receive his fees and forget about the patient and his disease. Haygarth had taken the trouble to record his observations and, having a sufficient number, to arrange and then reduce them to a tabular state, intelligible to others, and finally to generalise the facts, uninfluenced by hypothetical opinions.[44]

That Black's work did not lack impact either is revealed in this *Journal*'s comment on his short *Dissertation on insanity* (1810):

„We are glad to hear that he has not abandoned his plan, to exhibit the births of the human species, and the mortality of all ages over the globe; then the diseases and accidents by which they are swept away; and to reduce the whole, together with the remedies and modes of cure and prevention to arithmetical proof."[45]

On the other hand, Clutterbuck's *Inquiry into the seat and nature of fever*…(1807) that contained no statistics[46] failed „to excite a general conviction of its truth among his reader", despite the evidence collected and the spirit of investigation evinced. The reviewer felt that one would believe Clutterbuck, if one did not know about different interpretations: „We should be well pleased if he could decidedly show that fever is always connected with inflammation in any viscus".[47]

The lack of such precise evidence also induced the reviewer of an earlier book by Jackson (1808) to make some acid remarks. He questioned the author's claims of success stated

„in those general and equivocal terms, in which he deals largely... that we are altogether at a loss to discover the comparative advantages of the practice of his own hands....*Probably*! - is this the language of experience?...we doubt whether he has made the comparative experiment so often, as to ascertain the effect [of his method of cure]."[48]

It would seem that it had become unacceptable to defend a method as „generally successful without any discrimination of circumstances".[49] Even a new edition of Ferriar's first three volumes of *Medical histories* (1810), and the fourth volume in this series (1813), were criticised for lacking the additional evidence which the first editions had seemed to promise.[50] Consequently Mill's comparative statistics from two Dublin fever hospitals were reprinted in the review of his utility of blood-letting in fevers (1813) with the following comment „presuming....these are candid and correct statements, we may deem them potent arguments in favour of the advantages of the anti-phlogistic treatment of fever". With this method, Mills had adduced „very strong proof"of the superiority of bloodletting.[51] [Indeed he even impressed Bateman in London.[52]] Guthrie's answer to the question of the timing of amputation, and McGrigor's and Blane's works of 1813 and 1814 drew forth similar praises.[53]

In parallel to these reviews the *Edinburgh Journal* regularly published from 1807-1812 the complete annual statistical reports from the General Hospital near Nottingham.[In these six volumes they occupied over 170 pages, including tabular arrangements.] James Clarke (t1818), an Edinburgh M.D. and physician to this institution which was opened in 1782, arranged the case notes drawn up from every patient statistically according to diagnosis (according to Cullen's nosology), age-groups, sex, and months of the year. They were related to parallel tables of weather parameters. These statistics, more sophisticated versions of Lettsom's and Millar's, were, rounded off with a very thorough analysis, including autopsy reports. As had Lettsom and Millar, Clarke realised the little value of the overall figures of admissions, cures and deaths that were commonly published by hospitals, for the progress of medical science. He quoted Haygarth for his stand against the traditional practice of citing single cases. For him, too, the science of medicine was yet only in its infancy[54] and it was greatly to be regretted that more specific clinical reports such as his own had not been drawn up. True, this required much labour, yet for Clarke in 1807 it was obvious that „the advantages that immediately present themselves are so great, that the reporter is anxious to perform his undertaking with as much accuracy as possible“.[55]

From this evidence a more or less tacit acknowledgement of the utility - and even necessity - of numerical observations in clinical medicine can be recognised in the first decades of the 19th century. However, this did not preclude a critical attitude to the method from the beginnings, both by reviewers and by the promoters themselves. It was a valuable method, but it could also become treacherous and valueless if the data were observed arranged and interpreted in a single-minded or prejudiced fashion. Thus arithmetic observationism marked a shift away from reliance upon Authority, both scientific and social, towards the assertion of the individual himself. This quest for a new certainty in medicine and for a new meritocracy threw increased personal responsibility on the investigator; for, to use the method credibly, he was required, as he still is, to observe stringent moral standards both in the conduct of research and in the interpretation of results. This was indeed recognised in Percival’s *Medical ethics*.

These requirements are particularly illustrated in the treatment of fever and in my two surgical chapters, which also show the growing awareness that numbers alone were not sufficient, but that the circumstances behind them were as relevant as the mere comparative results. It is true that hospital reports tried to take into consideration environmental, personal and even anatomical parameters from their beginnings, perhaps in tribute to the Hippocratic tradition. Moreover, some private workers tried to match their groups as best they could in therapeutic trials. Yet with the availability of more and more such trials the importance of these additional parameters was seen in a new light, as numbers stood against numbers. In the midst of the struggle about bloodletting for the treatment of fever, during which statistics were widely used on both sides, the Edinburgh Journal, presuming their correctness and candour, wrote in 1813 that

„There is something painful and perplexing to the mind. For we are compelled to admit that there must be some... grievous misconception of the phenomena of the diseases in question, and of the operation of the medicines administered...., on the part of those who maintain the one or the other of these opposite opinions: and the practical consequences....appear to be of most fatal import. Or, if we admit of another alternative, the dilemma is still more painful, namely that neither of the methods of treatment gave had any material influence upon the progress of the diseases in question....“

This latter inference (which I described with respect to statistical writers on yellow fever, too) was explained away by the reviewer because of the powerful nature of the agents used by both parties in fevers. Thus, he humbly conceded that other explanations had to be found: was it perhaps that under different external or internal conditions of the animal economy, similar diseases did not require similar remedies? Significantly, this writer saw the way out of the maze by means of „extensive comparative experiments“.[56]

Thus, numerical observation had become an acceptable method. Its use so far had had theoretical consequences and had changed the outlook for the evaluation of therapies. I have illustrated, particularly in the surgical chapters but also in that on Army medicine, how from c.1820 doctors reacted with the use of more sophisticated arithmetical observationist techniques, to these feelings of perplexity and to the outspoken critic of their too undifferentiated a method by men who had never actually used it.

These considerations lead me now to enquire about the practical implications of arithmetical observationism. As Temkin and Ackerknecht have shown, motives in therapeutics are very complex and difficult to evaluate.[57] My answers, based as they are on what people wrote rather than on evidence of what they did, must be tentative and ambiguous; therefore I shall be brief.

The generally adopted therapy for continuous fevers is said to have changed at least twice during our period, i.e. when Boerhaave’s and Pringle’s anti-phlogistic treatment was replaced in the 1770s and 1780s by hygienic measures, bark and other stimulants; and when bloodletting and purging were reintroduced at the beginning of the century (to be attacked again by the 1820s). To what extent a practitioner was persuaded by the changing theoretical, patho-physiological explanation of these fevers or by the comparative numerical results of therapy, must remain open. (He might have been persuaded by both, of course, since the latter were often published to support the former). On the other hand, the 18th century empiricists had initially no particular opinion to defend, as exemplified by John Millar in 1769. The studies of the value of Peruvian bark for continuous fevers by Lind, Robertson, John Clark and Millar were quite empirical in this sense, as they went outspokenly against the overwhelming theoretical framework of Boerhaave. The same holds for the examination of „new“ drugs such as Fowler’s tobacco and arsenic, and Withering’s foxglove. Yet the theory of disease could still be influential, without there being any empirical evidence to support it: I may just recall that Doughty was impressed by, and changed his practice according to, an early work of Jackson, which had contained no numerical proofs. Neither did Clutterbuck’s book (1807), which reopened the period of bloodletting, contain any statistics. However, numerical results to verify the new inflammatory theory were afterwards sought.

This is also illustrated in the history of scurvy in the 18th century, and in the histories of lithotomy and amputation. The history of scurvy in the British Navy was probably shortened by the numerical method, as the verification of Macbride’s theory finally proved the ineffectiveness of the malt and the effectiveness of fruit juice on a large scale. Cheselden’s results of lateral lithotomy, a technique itself introduced and propagated on numerical grounds, probably won the technique many adherents, as did Friar Cosme’s suprapubic method, each according to the school and temperament of the individual surgeons. The same was probably true for the British improvements of amputation technique on the one hand, and for Bilguer’s „proof“of the inutility of this operation on the other hand.

Was the application of the numerical method by British military surgeons during the Napoleonic Wars relevant for the practice of amputations? From comparison with the French surgeons, whose work on amputation during the same wars was largely non-quantitative, I would be tempted to deny any influence. Both sides came to the same conclusion, which might therefore be ascribed to the needs of the military conditions during warfare. In that light, and according to Hennen’s testimony, Guthrie’s study (1815) would appear only as a post hoc confirmation and justification of the practices of hundreds of military surgeons during the preceding twenty years. Nonetheless the precise results as such were bound strongly to influence a future generation of military surgeons, as were the legendary successes of Larrey.

Indeed, my survey of the post-war period and of the 1830s showed, in amputation, lithotomy or fevers, that the method had a dynamic scientific potential of which the rising generation was well aware in Britain. In the words of a contemporary analyst,

„rational empiricism... has produced a most beneficial influence on the general state of medical practice. If it has, on some occasions, produced fluctuation of opinion, and in others indecision of inertness, it has tended to sweep away much error, and to purify the science from many of the antiquated doctrines and practices that still maintain their ground among our continental brethren.“[58]

I might add that rational empiricism had brought about testing, and testing not by a single extraordinary case. A therapy had to be justified by a numerical statement of success, rather than by mere authority. And standards for this evaluation had become recognised (along with their organisational requirements); these standards continued largely to be considered sufficient throughout the 19th century, as indicated by my introductory remarks on Kocher. Shryock was right in that diseases had to be known as distinct entities before numerical analysis could be applied profitably to evaluate respective therapies. But this knowledge was (and still is) a function of its time itself. Contemporaries emphasised that a number of diseases were well described and understood. And for the remaining diseases they advocated an arithmetic observationist approach, which effectively has since the end of the 18th century, continued to provide valuable results in certain situations.

Whereas according to Shryock accomplished probability mathematicians stimulated the rise of accomplished probability the *méthode numérique* in Paris, the evidence presented in this thesis would suggest that such an influence was minor in Britain when compared, for instance, to the social roots of arithmetic observationism. As indicated by Pearson there was a divorce between the two streams of thought which flowed from John Graunt: the mathematical theory based on probability, springing from the „life tables“, developed separately from the collection and interpretation, by very elementary processes, of mass observation. The reunion of these scientific and practical sides of statistics was an achievement of the 20th century only, namely the recognition that the science of statistics is the application of mathematics to the interpretation of mass observation.[59]

The 18th century development of the supremely important habit of observation, of careful, regular, and comprehensive recording, and of systematic interpretation with the means of simple arithmetic was not only important for the history of medical science. As it had several social roots, it also had social effects. To close this circle I may quote C. Kitson Clark’s Making of Victorian England (1968), where it is recognised that this type of 18th century study, which attempted to correlate disease with living conditions, „was a very important preparation to the scientific analysis of social conditions and their relation to disease upon which any effective attack upon the problem of public health must be based“.[60]

F. References to Chapter Eight

[1] Bostock 1833, pp.lxiii,lxix.
[2] Millar 1777,pp.4,7.
[3] Ackerknecht 1967, p.104;1973, p.105.
[4] Black 1788, pp.36,38.
[5] Robertson 1783, p.317.
[6] Lind 1763, p.2.
[7] Lind 1762, pp.141-143.
[8] Blane 1785, pp.201,202;1799,pp.582-583.
[9] Manning 1780, Vol.1, p.65.
[10] Lind 1762, pp.133-140.
[11] Pearson 1978, p.557.
[12] Lettsom 1774,pp.164-202.
[13] Morris 1751; 1759.
[14] Lettsom 1774, pp.343-344.
[15] Blane 1785, p.201.
[16] Brock 1973, p.17.
[17] Poynter 1969, p.57.
[18] Perkin 1972, pp.67-73.
[19] ibid., pp.17,38-56.
[20] Inkster 1977, p.128.
[21] Gooch 1758, p.v.
[22] Leake 1975, pp.76,88.
[23] Chaplin 1919, p.138.
[24] Personal communication by Dr. Charles Webster, Oxford.
[25] Watts 1978, p.4.
[26] Gelfand 1970; 1977; Peter 1975.
[27] Wilson 1978, pp.389-391.
[28] Alison 1833, pp.lxxv,c.
[29] Wrzosek 1931, p.437.
[30] Rolleston 1933.
[31] Chaplin 1919, p.75.
[32] Med.phil.Comment. 2; 9-19, (1774), 7; 177, (1780).
[33] ibid., 1;306-316, (1774), 2; 212,316-317, (1776), for Percival and 2; 95, (1774), 3; 280-289, (1775).
[34] ibid., 7; 31-43, (1780), for Dobson and 8; 141, (1781-1782) for Black.
[35] ibid., 10;;123,132, (1785) for Fowler and 10; 146 for Withering.
[36] ibid., 2nd decade, 1; 113-131, (1787), 10; 211-222,(1795).
[37] ibid.,10;212,(1795).
[38] ibid., 2; 17-73, (1788).
[39] Ann.Med., Lustrum 2;2; 51-78, (1803).
[40] Hamilton 1787, Vol.2, pp.104-105.
[41] Lond.med.Rev.Mag.2;431-438, (1800).
[42] ibid.,6;283-294, (1801).
[43] Edinb.med.surg.J.1;67,72,(1805).
[44] ibid., 479-480.
[45] ibid.,7;220,,(1811).
[46] Niebyl 1977, p.475.
[47] Edinb.med.surg.J.4;74,80-81, (1808).
[48] ibid.,5;119, (1809).
[49] ibid.,123.
[50] ibid.,7;226-231,(1811),10;240-244,(1814).
[51] ibid.,9;458-459 (1813).

[52] Niebyl 1977,p.475.
[53] Edinb.med.surg.J.12;222,233-240,(1816).
[54] ibid.,4;3,422,(1808).
[55] ibid.,3;309,(1807).
[56] ibid., 9;458-459 (1813).
[57] Temkin 1955; Ackerknecht 1962.
[58] Bostock 1833,p.lxix.
[59] Pearson 1978,p.126.
[60] Clark 1968,p.98.

BIBLIOGRAPHY

Abraham, J.J., (1933), Lettsom, his life, times, friends and descendants, London, Heinemann.

Ackerknecht, E.H., (1948), 'Anticontagionism between 1821 and 1867', Bull. Hist. Med., 22; 562-593.

-- -- -- (1953), 'Broussais, or a forgotten medical revolution', Bull. Hist. Med., 27: 320-343.

-- -- -- (1962), 'Aspects of the history of therapeutics', Bull. Hist. Med., 36; 389-419.

-- -- -- (1967), Medicine at the Paris Hospital 1794-1848, Baltimore, John Hopkins Press.

-- -- -- (1973), Therapeutics from the primitives to the 20th century, London and New York, Macmillan and Hafner.

-- -- -- (1976), 'Die Klinische Medizin und die Naturwissenschaften um 1800', Gesnerus, 33; 228-234.

Adair, J.M., (1787), A philosophical sketch of the natural history of the human body and mind..., Bath, Cruttwell.

Adami, J. G., (1922), Charles White of Manchester (1728-1813), Liverpool, University Press.

Aikin, J., (1771), _Thoughts on hospitals_, London, Johnson.

Alanson, E.A., (1782), _Practical observations on amputation and the after-treatment_, 2nd ed., London, Johnson.

Alcock, R., (1838), _Notes on the medical history and statistics of the British Legion in Spain_; comprising the results of gun-shot wounds, in relation to important questions in surgery, London, Churchill.

-- -- -- (1840), 'Observations on injuries of joints and their treatment', _Med.-chir. Trans. 23_; 243-322.

-- -- -- (1840-1841), 'Lectures on amputation and on the nature, progress, and termination of the injuries', _Lancet, i_; 105-111 and _passim_ up to _ii_, 849-858.

Alison, W.P., (1833), 'Dissertation of the state of medical science, from the termination of the eighteenth century to the present time', _in_: Forbes, J. _et al_. (eds.), _The cyclopaedia of practical medicine_, London, Sherwood _et al_., Vol. 1, pp. Lxxiii-cx.

Allan, R., (1808), _A treatise on the operation of lithotomy_, Edinburgh, for the author.

-- -- -- (1821-1824), _A system of pathological and operative surgery founded on anatomy_, Edinburgh and London Mc-Lachlan _et al_., Cradock _et al_.

Allison, R.S., (1943), _Sea diseases_: the story of a great natural experiment in preventive medicine in the Royal Navy. London, Bale.

Alzona, L., (1962), 'Un grande medico del Risorgimento: Giacomo Tommasini', _Pagine Stor. Med., 6_; 48-56.

Anderson, F., (1965), John Rollo's patient, _J. Hist. Med., 20_; 163-164.

Anderson, J., (1798), _A few facts and observations on the yellow fever of the West-Indies_...with the success attending the method of cure, Edinburgh and London, Mudie and Robinson.

Annesly, (Sir) J., (1828), _Researches into the causes, nature, and treatment of the more prevalent diseases of India and of warm climates generally_, London, Longman _et al_.

Anning, S.T., (1963), _The General Infirmary at Leeds_, Edinburgh and London, Livingstone.

-- -- -- (1969), 'Leeds house of recovery', _Med. Hist., 13_; 226-236.

Armstrong, G., (1771), _An essay on the diseases most fatal to infants_..., London, Cadell.

-- -- -- (1783), _An account of the diseases most incident to children from...birth till...puberty_, London, Cadell.

Armstrong, J., (1814), _Facts and observations relative to the fever commonly called puerperal_, London, Baldwin _et al_.

-- -- -- (1819), The same, (2nd ed.), _ibid_.

Artelt, W., (1958), 'Louis' amerikanische Schüler und die Krise der Therapie', _Sudhoffs Arch. Gesch. Med., 42_; 291-301.

Astruc, P., (1932), 'Le centenaire de la médecine d'observation', _Progr. Méd. (Paris), 9_; suppl. 73-79, 81-87.

Bagieu, J., (1756-57), _Examen de plusieurs parties de la chirurgie_, Paris, Delagnette.

Baker, (Sir) G., (1768), 'Essay regarding the cause of the endemical colic of Devonshire', _Med. Trans. roy. Coll. Phys. London, 1_; 175-256.

Baldinger, E.G., (1765), _Von den Krankheiten einer Armee_, Langensalza, Martini.

Ballingall, (Sir) G., (1852), _Outlines of military surgery_, 4th ed., Edinburgh, Black.

Bancroft, E.N., (1820), _An essay on the disease called yellow fever..._ partly delivered as the Gulstonian Lectures in the years 1806 and 1807, (Republication), Baltimore, Cushing and Dewelt.

Banner, J.M., (1843), 'A report of the cases of fractures of the larger bones of the extremities...', Edinb. med. surg. J., 59: 1-16.

Bariéty, M., (1972), 'Louis et la méthode numérique', Clio Med., 7; 177-183.

Bateman, T., (1818), A succint account of the contagious fever of this country, London, Longman.

Baseilhac, J., (1779), Nouvelle méthode d'extraire la pierre par dessus le pubis, Paris, D'Houry.

Baseilhac, P., (1804), De la taille latérale par le périnée, et celle de l'hypogastre ou haut appareil, Paris, Gabon.

Batty Shaw, A., (1968), 'The oldest medical societies in Great Britain', Med. Hist. 12; 232-244.

-- -- -- (1970), 'The Norwich School of Lithotomy', Med. Hist. 14; 221-259.

Beaglehde, J.C., (ed.), (1967-1969), The journals of Captain James Cook on his voyages of discovery, Cambridge Univ. Press.

Bégin, L.J., (1860), Etudes sur le service de santé militaire en France, Paris, Rozier.

Bell, B., (1) (1778), Treatise on the theory and management of ulcers.. Edinburgh, Elliot.

—— —— —— (1783-88), A system of surgery, Edinburgh, Elliot.

—— —— —— (1787-89), The same, 3rd ed., ibid.

—— —— —— (1801), The same, 7th ed., Edinburgh, Bell et al.

Bell, B. (2), (1868), The life, character and writings of Benjamin Bell, Edinburgh, Edmonston and Douglas.

Bell, (Sir), C., (1807), A system of operative surgery founded on the basis of anatomy, London, Longman et al.

—— —— —— (1814), The same, 2nd ed., ibid.

—— —— —— (1816), Surgical observations being a quarterly report of cases in surgery treated in the Middlesex Hospital and in private practice, London, Longman et al.

—— —— —— (1818), The same, Vol. 2, ibid.

Bell, J., (1815), The principles of surgery, London, Longman.

—— —— —— (1812), Discourses on the nature and cure of wounds, 3rd ed., Edinburgh, Bryce.

Bett, W.R., (1955), 'John Clark (1744-1805): Medical philantropist', Med. Press, 233; 473.

Bérard, N.N., (1786), Tableau méthodique et analytique des différentes manières de faire l'opération de la taille pour l'estraction de la pierre..., Paris, Polytypie.

Bickerton, T.H., (1936), Medical history of Liverpool, London, Murray.

Bilguer, J.U., (1764), A dissertation on the inutility of the amputations of limbs, London, Baldwin.

Billroth, T., (1869), Chirurgische Klinik Zürich 1860-1867..., Berlin, Hirschwald.

-- -- -- (1879), Chirurgische Klinik Wien 1871-1876..., Berlin, Hirschwald.

-- -- -- (1933), Historical studies on the nature and treatment of gunshot wounds from the fifteenth century to the present time, Transl. from the 1859 German original, New Haven, Connecticut, Nathan Smith Medical Club.

Blackadder, H.H., (1818), Observations on phagedaena gangraenosa, Edinburgh and London, Brown and Underwood.

Black, W., (1781), Observations medical and political on the small-pox and inoculation..., London, Johnson.

-- -- -- (1782), An historical sketch of medicine and surgery from their origin to the present time..., London, Johnson.

-- -- -- (1788), _A comparative view of the mortality of the human species at all ages_..., London, Dilly.

-- -- -- (1789), The same, 2nd ed., new title: _An arithmetical and medical analysis of the diseases and mortality of the human species_..., London, Dilly.

-- -- -- (1973), The same, reprint of the 1789 ed., Gregg International Publishers, Westmead, Farnborough, Hants.

Blanco, R.L., (1970), Henry Marshall, (1775-1851), and the health of the British army, _Med. Hist., 14;_ 260-276.

-- -- -- (1974), _Wellington's surgeon general: Sir James McGrigor_, Durham, N.C., Duke Univ. Press.

Bland, R., (1782), 'Midwifery reports of the Westminster General Dispensary..., summary, _Lond. med. J., 3_; 114-121.

Blane, (Sir) G., (1785), _Observations on the diseases incident to seamen_, London, Cooper.

-- -- -- (1799), The same, 3rd ed., London, Murray and Highley.

-- -- -- (1813 A), 'Observations on the comparative prevalence, mortality, and treatment of different diseases', _Med.-chir. Trans., 4_; 89-141.

-- -- -- (1813 B), 'Supplement', _ibid_, p. 466-477.

--- --- --- (1815), 'Statements on the comparative health of the British Navy from the year 1779 to the year 1814', Med.-chir. Trans., 6; 490-573.

--- --- --- (1816), 'Facts and observations respecting intermittent fevers', Med.-chir. Trans., 2nd ed., 3: 1-33.

--- --- --- (1819), 'A statement of facts tending to establish an estimate of the true value and present state of vaccination', Med.-chir. Trans., 10; 315-338.

--- --- --- (1820), 'Account of the epidemic spasmodic cholera..., in India, communicated in a letter from Frederick Corbyn, Esq., with communication on the same subject, by favour of the chairman...of the East India Company...', Med.-chir. Trans. 11; 110-164.

--- --- --- (1833), Select dissertations on several subjects of medical science, new ed., London, Nicol.

Bloch, H. (1969), 'Pierre Charles Alexander Louis. Influence on American medicine", N.Y. St. J. Med., 69; 3056-3059.

Bloom, H.J.G., W.W. Richardson, E.J.Harries, (1962), 'Natural history of untreated breast cancer (1805-1933), Brit. med. J., ii; 213-221.

Boggie, J., (1848), Observations on hospital gangrene with reference chiefly to the disease as it appeared in the British

Army..., Edinburgh, Sutherland and Knox.

Bollet, A.J., (1973), 'Pierre Louis, the numerical method and the foundation of quantitative medicine', *Am. J. med. Sci.*, *266*; 93-101.

Boott, F., (1833), *Memoir of the life and medical opinions of John Armstrong*, London, Baldwin and Cradock.

Bordenave, T., (1753), 'Précis de plusieurs observations sur les playes d'armes à feu en différentes parties', *Mém. Acad. roy. Chir. (Paris) 2*; 501-534.

Bostock, J., (1833), 'History of medicine', *in*: Forbes J. *et al* (eds.), *The cyclopaedia of practical medicine*, London, Sherwood *et al*., Vol, 1, pp. i-Lxxii.

Boucher, P.J., (1753A), 'Observations sur les playes d'armes à feu... première partie...' *Mém, Acad. roy. Chir. (Paris), 2*; 287-307.

-- -- -- (1753 B), The same, 'Deuxième partie', *ibid*., 461-483.

Bradley, L., (1971), *Small-pox inoculation. An eighteenth century mathematical controversy*, Nottingham, University Department of Adult Education.

Brock, M., (1973), *The great reform act*, London, Hutchinson Univ. Library.

Brock, R.C., (1952), The life and work of Sir Astley Cooper, Edinburgh and London Livingstone.

Brock, W.H., (1965), 'The life and work of William Prout', Med. Hist., 9; 101-126.

Brockbank, E.M., (1934), A medical statistician of a century and a half ago, Manchester, Norbury.

— — — (1950), John Ferriar, public health work, Tristan Shandy, other essays and verses..., London, Heinemann.

Brockbank, E.W., (1904), Sketches of the lives and work of the honorary medical staff of the Manchester Infirmary, Manchester Univ. Press.

Brocklesby, R., (1764), Oeconomical and medical observations from the year 1758 to the year 1763 inclusive, London, Becket.

Bro mfield (Sir). W., (1773), Surgical observations and cases, London, Cadell.

Bruppacher, R., (1967), Militärmedizin in der Auf klärung, Zürcher medizingeschichtliche Abhandlungen, N.R. Vol. 52, Zürich, Juris.

Bryce, J., (1796), An account of the yellow fever with a method of cure, Edinburgh and London, Creech and Robinson.

Buer, M.C., (1926), Health, wealth, and population in the early days of the industrial revolution, London, Routledge.

Bull, J.P., (1959), 'The historical development of clinical therapeutic trials', J. chron. Dis., 10; 218-248.

Bullough, V. and Bullough, B., (1971), 'The causes of the Scottish medical renaissance of the eighteenth century', Bull. Hist. Med., 45; 13-28.

Burnett, (Sir) W., (1816), A practical account of the mediterranean fever..., 2nd ed., London, Callow.

Butter, W., (1775), An account of the puerperal remittent fever, repr. in: Churchill, 1849, pp. 335-350.

Butterfield, L.H., (1946), 'Benjamin Rush: a physician as seen in his letters', Bull. Hist. Med., 20; 138-156.

Bynum, W.F., (1978A), 'Introduction' to: Medicine and industrialization in History, in: E.G. Forbes (ed.) Human implications of scientific advance, Proc. XVth int. Congr. Hist. Sci. Edinburgh 1977, Edinburgh, University Press, pp. 197-202,

-- -- -- (1978B), 'Hospital, disease and community: The London Fever Hospital, 1801-1850', in press.

Cabanis, P.J.G., (1798), Du degré de certitude de la médecine, Paris, Didot.

Cadoret, M., (1969), _Esquirol et la statistique médicale,_ M.D. thesis No. 663, Faculté de Médecine, Paris.

Callisen, A.C.P., (1830-1845), _Medicinisches Schriftsteller-Lexikon,_ Copenhagen, for the author.

The Cambridge modern history (1902-1911), ed. by A.W. Ward _et al_., Cambridge, Univ. Press.

The New Cambridge modern history (1957-1960), ed. by G.R. Potter _et al_., Cambridge, Univ. Press.

Cantlie (Sir) N., (1972), 'Inspector-General of Hospitals Robert Jackson (1750-1827), Army Medical Department', _Proc. roy. Soc. Med., 65_; 1123-1126.

-- -- -- (1974), _A history of the Army medical department,_ Edinburgh and London, Churchill and Livingstone.

Carpue, J.C., (1819), _A history of the high operation for the stone,_ London, Longman _et al_.

Cassedy, J.H., (1969), 'Metereology and medicine in colonial America: Beginnings of the experimental approach', _J. Hist. Med.,_ _24_; 193-194.

-- -- -- (1974), 'Medicine and the rise of statistics', _In:_ Debus, A.G. (ed.), _Medicine in seventeenth century England,_ Berkeley, Los Angeles, London, Univ. California Press.

Chaplin, A., (1919), Medicine in England during the reign of George III, London, by the author.

Charles, (Sir) J., (1961), Research and public health, London, Oxford Univ. Press.

Charleton, R., (1770), An inquiry into the efficacy of warm bathing in palsies, Oxford, Clarendon.

Cheselden, W., (1723), A treatise on the high operation for the stone..., London, Osborn.

-- -- -- (1732), The anatomy of the human body, 4th ed., London, Bettesworth and Hitch.

-- -- -- (1740), The same, 5th ed., London, Bowyer.

-- -- -- (1792), The same, 13th ed., London, Dodsley.

Chevalier, T., (1804), A treatise on gun-shot wounds..., London, Bagster.

Chevreau, A., (1912), Un grand chirurgien du XVIII^{e} siècle: Frère Côme, Mesnil-sur-l'Estrée, Didot.

Childs, St. J.R., (1970), 'Sir George Baker and the dry belly-ache', Bull. Hist. Med., 44; 213-240.

Chisholm, C., (1795), An essay on the malignant pestilential fever introduced into the West India Islands from Boulam on the coast of Guinea, London, Dilly.

-- -- -- (1801), The same, 2nd ed., London, Mawman.

-- -- -- (1822), A manual on the climate and diseases of tropical countries..., London, Burgess and Hill.

Churchill, F., (ed.), (1849), Essays on the puerperal fever and other diseases selected from the writings of British authors previous to the close of the eighteenth century, London, Sydenham Society.

Civiale, J., (1827), De la lithotritie, Paris, Béchet.

Clark, G.K., (1968), The making of Victorian England, London, Methuen.

Clark, James, (1797), A treatise on the yellow fever as it appeared in the Island of Dominica..., London, Murray.

Clark, John, (1773), Observations on the diseases in long voyages to hot countries..., London, Wilson.

-- -- -- (1792), The same, 2nd ed. (...and on the same diseases as they appear in Great Britain), London, Murray.

-- -- -- (1780), Observations on fever, especially those of the continued type, London, Cadell.

-- -- -- (1802), A collection of papers, intended to promote an institution for the cure and prevention of fevers, Newcastle, Hodgson.

Clark, T., (1801), Observations on the nature and cure of fevers, and of diseases of the West and East Indies and of America, Edinburgh, Bell and Bradfute.

Clarke, James, (1807-1812), 'Medical report from the General Hospital near Nottingham', Edinb. med. surg. J., 3; 309-322, 4; 1-20, 265-285, 422-440, 5; 188-198, 257-277, 6; 1-14, 261-287, 7; 129-146, 8; 138-150.

Clarke, John, (1793), 'Practical essays on the management of pregnancy and labour and on the inflammatory and febrile diseases in lying-in women', in: Churchill, 1849, pp. 363-443.

Clarke, Joseph, (1788) 'Observations on some causes of the excess of mortality of males above that of females...', summary, Lond. med. J., 9; 179-200.

-- -- -- (1790), 'Observations on the puerperal fever...', in: Churchill, 1849, pp. 352-362.

-- -- -- (1817), 'Abstract of a registry kept for some years in the lying-in Hospital of Dublin, Trans. Ass. Fellows Lic. King's Queen's Coll. Phys. Irel., 1; 367-402.

Cleghorn, G., (1751), Observations on the epidemical diseases in Minorca from the year 1744 to 1749, London, Wilson.

Clifton, F., (1731), Tabular observations recommended as the plainest and surest way of practising and improving physick, London, Brindley.

--- --- --- (1732), The state of physick, ancient and modern, briefly considered, London, Nourse.

Cohen, (1st Baron of Birkenhead), H., (1972), 'The Liverpool medical school and its physicians', Med. Hist., 16; 310-320.

Coley, N.G., (1968), 'Alexander Marcet (1770-1822), physician and animal chemist', Med. Hist., 12; 394-402.

Collins, E.T., (1929), The history and traditions of the Moorfields Eye Hospital, London, Lewis.

Collins, R., (1835), A practical treatise on midwifery, containing the results of sixteen thousand six hundred and fifty four births..., London, Longman.

Colot, F., (1727), Traité de l'opération de la taille, Paris, Vincent.

Cooper, (Sir) A., (1823), A treatise on dislocations and on fractures of the joints, London, Longman.

--- --- --- (1825), The lectures of Sir Astley Cooper, Bart. FRS... on the principles and practice of surgery. London, Underwood.

-- -- -- (1832), Lectures on the principles and practice of surgery as delivered in the theatre of St. Thomas's hospital, 3rd ed., London, Westley.

Cooper, S., (1807), The first lines of the practice of surgery, London, Phillips.

-- -- -- (1819), The same, 4th ed., London, Longman et al.

-- -- -- (1826), The same, 5th ed., ibid.

-- -- -- (1818), Dictionary of practical surgery, 3rd ed., London, Longman et al.

-- -- -- (1822), The same, 4th ed., ibid.

-- -- -- (1861-72), The same, new ed. by S.A.Lane et al. London, ibid.,

Cope, Z., (1953), William Cheselden 1688-1752, Edinburgh and London, Livingstone.

-- -- -- (1964), 'The history of the dispensary movement', in: Poynter, 1964, pp. 73-76.

-- -- -- (1969), 'The influence of the free dispensaries upon medical education in Britain', Hist. Med., 13; 29-36.

Cranefield, P.F., (1974), The way in and the way out. François Magendie, Charles Bell and the roots of the small nerves, Mount Kisco, New York, Futura Publishing.

Crellin, J.K., (1971), 'William Cullen: his calibre as a teacher, and an unpublished introduction to his _Treatise of the materia medica_, London, 1773, _Med. Hist., 15_; 79-87.

-- -- -- (1974), 'Dr. James's fever powder', _Trans. Brit. Soc. Hist. Pharm., 1_; 136-143.

Crichton, J., (1828), 'Observations on the operation of lithotomy', _Edinb. med. surg. J., 29_; 225-236.

Crosse, J.G., (1822), 'An eulogy upon Edward Rigby', _in_: Rigby, 1822, pp. XXV-LXI.

-- -- -- (1835), _A treatise on the formation, constituents and extraction of the urinary calculus_, London, Churchill.

-- -- -- (1841), The same, 2nd ed., _ibid_.

Crosse, V.M., (1968), _A surgeon in the early nineteenth century. The life and times of John Green Crosse, 1790-1850_. Edinburgh, Livingstone.

Crowe, K.E., (1973), 'The Walcheren expedition and the new Army Medical Board: a reconsideration', _Engl. hist. Review, 88_; 770-785.

Crummer, L.R., (1922), 'Robert Jackson, M.D., late inspector general of army hospitals', reprint from _Mili Surg_., Febr. 1922.

-- -- -- (1928), 'Prefactory essay to an introduction to the study of physic by William Hebeden M. D', Ann. Med. Hist., 10; 226-241.

Cullen, M.J., (1975), The statistical movement in early Victorian Britain, Has ocks, Sussex and New York, Harvester and Barnes and Noble.

Cullen, W., (1786), Institutions de médecine-pratique, traduites par M. Pinel, Paris, Duplain.

Cullingworth, C. J., (1904), Charles White, London, Glaisher.

Currie, J., (1792), 'Of tetanus and of convulsive disorders', Mem. Med. Soc. Lond., 3; 147-175.

-- -- -- (1797), Medical reports on the effects of water, cold and warm as a remedy in fever and febrile diseases, Vol. 1, Liverpool and London, M'Creery and Cadell.

-- -- -- (1804), The same, Vol. 2, ibid.

Currie, W.W., (1831), Memoir of the life, writings, and correspondence of James Currie, M.D., FRS., London, Longman.

Cutter, J.S. and H.R. Viets, (1964), A short history of midwifery, Philadelphia and London, Saunders.

Dainton, C., (1961), The story of England's hospital, London, Museum Press.

Danvin, B., (1831), De la méthode numérique et de ses avantages dans l'étude de la médecine, thesis, Paris.

Davis, J.B., (1821), Annals historical and medical, during the first four years, of the universal dispensary for children... founded in 1816, London, Simpkin and Marshall.

Delaunay, P., (1953), 'Les doctrines médicales au début du XIX^{e} siècle - Louis et la méthode numérique', in: E.A. Underwood (ed.), Science, medicine and history. Essays on the evolution of scientific thought and medical practice, London, New York, Toronto, Oxford Univ. Press, Vol, 2, pp. 321-330.

Dennis, F.S., (1895), System of surgery, Philadelphia, Lea.

Deschamps, J.F., (1796), Traité historique et dogmatique de la taille, Paris, for the author.

Desruelles, H.M.J., (1830), Memoir of the treatment of venereal diseases without mercury, Philadelphia, Carey and Lea.

Dickinson, N., (1819), Observations on the inflammatory endemic incidental to strangers in the West Indies, commonly called the Yellow Fever, London, Callow et al.

Dickson, D.J.H., (1816), 'Observations on the utility of blood letting, and purgatives in a fever which prevailed in the Russian fleet', Edinb. med. surg. J., 12; 158-176.

Dictionary of national biography, (1908), ed. by L. Stephen and S. Lee, London, Smith, Elder et al.

Dictionary of scientific biography, (1970-1976), ed. by C.C. Gillispie et al, New York, Scribner's sons.

Dlugatz, P.M., (1968), Astley Cooper's contribution to the knowledge of dislocations and fractures, M.D. thesis, Zürich, Juris.

Dobson, J., (1972), 'Percival Pott', Ann.roy. Coll. Surg. Engl., 50; 54-65.

-- -- -- (1977), 'Matthew Dobson', in: Ross, J.A. (ed.) Collected papers concerning Liverpool medical history, Liverpool, part 2, pp. 1-13.

Dobson, M., (1779), A medical commentary on fixed air, Chester, Monk.

Doughty, E., (1816), Observations into the nature and treatment of the yellow, or bulam fever in Jamaica and at Cadiz, London, Philantropic Society.

Douglas, J., (1722), 'An account of the new method of cutting for the stone', Phil. Trans.roy.Soc., 32; 83-88.

Dudley, (Sir) S.F., (1953), 'The Lind-tradition in the Royal Naval Medical Service', in: C.P. Stewart and D. Guthrie (ed.), Lind's treatise on scurvy, Edinburgh Univ. Press, pp. 369-386.

Dukes, C.E., (1960), 'London medical societies in the eighteenth century', Proc. roy. Soc. Med., 53; 699-706.

-- -- -- (1961), 'Dr. James Sims (1741-1820): a new appraisal', Med. Hist., 5; 375-383.

Duncan, A., (1781), Medical Cases selected from the Public Dispensary at Edinburgh, 2nd ed., Edinburgh, Elliot.

Dupuytren, G., (1812), Lithotomie, Paris, Lebegne.

Earle, (Sir) J., (1793), Practical observations on the operation for the stone, London, Johnson.

Earle, H., (1820), 'On renal calculi', Med.-Chir. Trans., 11; 211-231.

Enders, J.F., (1964), 'Francis Home and his experimental approach to medicine', Bull. Hist. Med., 38 101-112.

Fasbender, H., (1906), Geschichte der Geburtshülfe, Jena, Fischer.

Falconer, W., (1787), An account of the efficacy of the aqua mephitica alcalina, 2nd ed., London, Cadell.

-- -- -- (1790), A practical dissertation on the medicinal effects of the Bath waters, London and Bath, Robinson and Cruttwell.

-- -- -- (1795), An account of the use, application and success of the Bath waters in rheumatic cases, Bath and London, Meyler and Robinson.

-- -- -- (1805), A dissertation on ischias or the disease of the hip-joint, London, Cadell.

Faure, N.N., (1753), Mémoire sur l'amputation, in: Boucher, 1753 (B).

-- -- -- (1759), Mémoire sur l'amputation , Prix Acad. roy. Chir. (Paris), 3; 489-520.

Feinstein, A.R., (1967), Clinical judgment, Baltimore, Williams and Wilkins.

Fellowes, (Sir) J., (1815), Reports of the pestilential disorder of Andalusia which appeared at Cadiz in the years 1800, 1804, 1810, and 1813, London, Longman et al.

Fenwick, S., 1847-1848, 'Statistical researches on amputations' Monthly J. med. Sci., 8; 236-254, 333-343, 404-415, 482-495, 583-587.

Fergusson, W., (1813), 'Observations on the venereal disease in Portugal', Med.-chir. Trans., 4; 1-16.

-- -- -- (1817), 'An inquiry into the origin and nature of the yellow fever', Med.-chir. Trans., 8; 108-172.

Ferriar, J., (1792), Medical histories and reflexions Vol. 1, quoted from a later ed., 1810, London, Cadell and Davies.

-- -- -- (1795), The same, Vol. 2, quoted from a later ed. 1810, _ibid_.

-- -- -- (1798), The same, Vol. 3, quoted from a later ed. 1810, _ibid_.

-- -- -- (1813), The same, Vol. 4, London, Cadell and Davies.

Forbes, (Sir) J., (1818), 'Medical report on the state of health in His Majesty's ship Venerable in the years 1814 and 1815', _Med.-chir. J. and Review, 5_; 93-106.

Fordyce, G., (1793), 'An attempt to improve the evidence of medicine', _Trans. Soc. Improvement Med.-Chir. Knowledge, 1_; 243-293.

Fowler, T., (1785) _Medical reports on the effects of tobacco in the cure of dropsies and dysenteries_..., quoted from the 2nd ed., London, for the author, 1788.

-- -- -- (1786), _Medical reports on the effects of arsenic in the cure of agues, remittent fevers, and periodic head-aches_, London, Johnson.

-- -- -- (1795), _Medical reports on the effects of blood-letting, sudorifics, and blistering in the cure of the acute and chronic rheumatism_, London, Johnson.

Fulton, J.F., (1933), 'The Warrington Academy and its influence upon medicine and science', Bull. Inst. Hist. Med. Johns Hopkins Univ. 1; 50-80.

-- -- -- (1953), 'The place of William Withering in scientific medicine', J. Hist. Med., 8; 1-15.

Fuster, J.J.N., (1832), De l'application du calcul à la thérepeutique, Gaz. Méd. (Paris), i; 38-43.

Gadient, A., (1963), Die Anfänge der Urologie als Spezialfach in Paris (1800-1850), Zürcher medizingeschichtliche Abhandlungen, N.R., Vol. 13, Zürich, Juris.

Gallot-Lavallé, P., (1913), Jean Colombier, un hygieniste au XVIII[e] siècle, Paris, Jouve.

Garrison, F.H., (1922), Notes on the history of military medicine, Washington, Association of Military surgeons.

-- -- -- (1924), An introduction to the history of medicine, 3rd ed., Philadelphia and London, Saunders.

Gask, G.E., (1950), 'John Hunter in the campaign in Portugal, 1762-3', in: Essays in the history of medicine, London, Butterworth, pp. 116-144.

Gay, F.P., (1939), 'Medical logic', Bull. Inst. Hist. Med. Johns Hopkins Univ., 7; 6-27.

Gavarret, L.D.J., (1840), Principes généraux de statistique médicale, Paris, Bechet and Labé.

Gelfand, T., (1970), 'Empiricism in eighteen-century French surgery', Bull. Hist. Med., 44; 40-53.

-- -- -- (1973 A), The training of surgeons in eighteenth century Paris and its influence on medical education, Ph. D. thesis, Baltimore, M.D. Univ. Micro-films.

-- -- -- (1973 B), 'The Hospice of the Paris College of Surgery 1774-1793', Bull. Hist. Med., 47; 375-393.

-- -- -- (1977), 'A clinical ideal: Paris 1789', Bull. Hist. Med., 51; 397-411.

George, D., (1925), London life in the XVIIIth century, London, Kegan Paul, et al.

Gibbon, J., (1922), 'London surgery in the early part of the nineteenth century', Ann. med. Hist., 4; 80-85.

Gillespie, L., (1800), Observations on the diseases which prevailed on board of His Majesty's squadron on the Leeward Island station between November 1794 and April 1796, London, Cuthell.

Glass, D.V., (1973), 'Introduction', in Black 1973, no pagination.

Gooch, B., (1758), Cases and practical remarks in surgery, London, Wilson and Durham.

-- -- -- (1767), The same, 2nd ed., Norwich, Chase.

-- -- -- (1773), Medical and chirurgical observations as an appendix to a former publication, London, Robinson.

Gordon, A., (1795), 'A treatise on the puerperal fever of Aberdeen', in: Churchill, 1849, pp. 445-500.

Gordon-Taylor, (Sir) G., (1958), Sir Charles Bell. His life and times, Edinburgh and London, Livingstone.

Granville, A.B., (1819), A report of the practice of midwifery at the Westminster General Dispensary during 1818, London, Burgess and Hill.

Gray, J., (1952), History of the Royal Medical Society, Edinburgh, University Press.

Greenwood, M., (1936), 'Louis and the numerical method', In: The medical dictator and other biographical studies, London, Williams and Norgate, pp. 123-142.

-- -- -- (1942), British loss of life in the wars of 1794-1815 and in 1914-1918, J. roy. stat. Soc., 105; 1-11.

-- -- -- (1948 A), Medical statistics from Graunt to Farr, Cambridge Univ. Press.

-- -- -- (1948 B), Some British pioneers of social medicine, London, New York, Toronto, Oxford Univ. Press.

Gregory, J., (1805), Lectures on the duties and qualifications of a physician 1772, revised and corrected by James Gregory, Edinburgh and London, Creech and Cadell.

Gunn, A., (1964), 'Maternity hospitals', in: Poynter, 1964, pp. 77-101.

Guthrie, G.J., (1815), On gun-shot wounds of the extremities, London, Longman.

-- -- -- (1817), 'Observations on the treatment of the venereal disease without mercury', Med.-chir, Trans., 8; 550-581, quoted in Desruelles, 1830.

-- -- -- (1820), Observations on gun-shot wounds and on injuries of the nerves, London, Burgess and Hill.

-- -- -- (1838), Clinical lectures on compound fractures of the extremities..., London, Churchill.

-- -- -- (1853), Commentaries on the surgery of the war in Portugal, Spain, France, and the Netherlands..., London, Renshaw.

Hall, M., (1830), Researches principally relative to the morbid and curative effects of loss of blood, London, Seeley et al.

Hamilton, J., (1805), Observations on the utility and administration of purgative medicine in several diseases, Edinburgh, Stewart.

Hamilton, R., (1787), *The duties of a regimental surgeon considered*, London, Johnson *et al*.

Hammick, (Sir) S.L., (1830), *Practical remarks on amputation, fractures and strictures of the urethra*, London, Longman *et al*.

Hartston, W., (1963), 'Medical dispensaries in eighteenth century London', *Proc. roy. Soc. Med., 56;* 753-758.

Hawkesworth, J., (1785), *An account of the voyages undertaken* ...*for making discoveries in the southern hemisphere*...*by Commodore Byron, Captain Wallis, Captain Carteret, and Captain Cook*..., 3rd ed., London, Strahan *et al.*

Hawkins, F.B., (1829), *Elements of medical statistics*, London, Longman, *et al*.

Haygarth, J., (1784), *An inquiry how to prevent the small-pox*, Chester and London, Monk and Johnson.

-- -- -- (1800), *Of the imagination as a cause and as a cure of disorders of the body*..., Bath and London, Cruttwell and Cadell.

-- -- -- (1801), *A letter to Dr. Percival on the prevention of infectious fevers*..., London, Cadell and Davies.

-- -- -- (1805), *A clinical history of diseases* *I.* A clinical history of the acute rheumatism, or rheumatick fever, *II.* A clinical history of the nodosity of the joints, London, Cadell and Davies

-- -- -- (1813), The same, 2nd ed., _ibid._

Heberden, W. (the elder), (1772), 'On a disorder of the breast', _Med. Trans. roy. Coll. Phys. Lond. 2_; 59-67.

-- -- -- (1802), _Commentaries on the history and cure of diseases_, London, Payne.

Hennen, J., (1818 A), 'Observations on the cure of syphilis without mercury...', _Edinb. med. surg. J., 14_; 201-208, 328-335, 356.

-- -- -- (1818 B), _Observations on some important points in the practice of military surgery_..., Edinburgh, Constable.

-- -- -- (1829), _Principles of military surgery_, 3rd ed., London, Wilson.

Hey, W., (1815), _Treatise on the puerperal fever_, London, Longman.

Hillary, W., (1759), _Observations on the changes of the air and the concomittant epidemical diseases in the island of Barbados_, London, Hitch and Hawes.

-- -- -- (1761), _An inquiry into the means of improving medical knowledge_, by examining all those methods which have hindered, or increased its improvement in all past ages..., London, Hitch and Hawes.

Hirsch, A. (ed.), (1929), _Biographisches Lexikon der hervorragenden Ärzte aller Zeiten und Völker_, 2nd ed., Berlin and Vienna, Urban and Schwarzkopf.

Hirschberg, A.J., (1908), 'Geschichte der Augenkeilkunde', _in_: Greafe-Saemisch, _Handbuch der gesamten Augenheilkunde_, Vol. 13, 2nd ed., Leipzig, Engelmann.

-- -- -- (1912), _Frankreichs Augenärzte_, Leipzig, Engelmann.

Highmore, A., (1814), _Pietas Londiniensis_. The history, design and present state of the various public charities in and near London, London, Cradock and Joy.

Home, F., (1759), _Medical facts and observations_, London, Millar.

Howard, J., (1791), _An account of the principal lazarettos in Europe_, 2nd ed., London, Johnson _et al_.

Howe, G.M., (1972), _Man, environment and disease in Britain_, New York and Newton Abbot, Barnes and Noble, David and Charles.

-- -- -- (1977), _A world geography of human diseases_, London, New York, San Francisco, Academic Press.

Huard, P. and Vetter, T., (1967), 'Le docteur Jean Civiale et la lithotritie (1792-1867)', _Episteme, 1_; 239-254.

Hughes, R.E., (1975), 'James Lind and the cure of scurvy: an experimental approach', Med. Hist., 19; 342-351.

Hulme, N., (1772), A treatise on the puerperal fever, London, Cadell, also in: Churchill, 1849, pp. 61-116.

Hume, E.E., (1942), 'Francis Home, M.D., 1719-1813...', Bull. Hist. Med., 11; 48-68.

Hume, G.H., (1906), The history of the Newcastle Infirmary, Newcastle-upon-Tyne, Reid.

Hume, J., (1778), 'An account of the true bilious, or yellow fever; and of remitting and intermitting fevers of the West Indies', in: Letters and Essays on... the West Indies to Dr. Monro, London, Murray.

Hume, W.E., (1951), The Infirmary of Newcastle-upon-Tyne 1751-1951, Newcastle-upon-Tyne, Reid.

Hunt, T., (1972), The Medical Society of London 1773-1973, London, Heinemann.

Hunter, John., (1794), A treatise on the blood, inflammation and gun-shot wounds, London, Nicol.

Hunter, J. (of Jamaica), (1788), Observations on the diseases of the Army in Jamaica and on the best means of preserving the health in hot climates, London, Nicol.

Hunter, R. and Mac Alpine, I., (1965), 'William Pargeter and the Medical Society of Oxford 1780-83', <u>Med. Hist., 9</u>; 181-183.

Hutchison, A.C., (1816), <u>Some practical observations in surgery, embracing the subjects of amputation</u>..., London, Callow.

-- -- -- (1826), The same, 2nd ed., London, Underwood.

-- -- -- (1817), <u>Some further observations on the subject of the proper period for amputating in gun-shot wounds</u>, London, Callow.

-- -- -- (1818), 'On the comparative infrequency of urinary calculi among sea-faring people', <u>Med.-chir. Trans., 9</u>; 443-471.

-- -- -- (1830 A), 'A further inquiry into the comparative infrequency of calculous diseases among sea-faring people', <u>Med.-chir. Trans., 16</u>; 94-120.

-- -- -- (1830 B), 'Some observations on the frequency of calculous diseases in Scotland', <u>Med.-chir. Trans., 16</u>; 120-127.

Inkster, I., (1977), 'Marginal men: aspects of the social role of the medical community in Sheffield 1790-1850', <u>in</u>: J. Woodward and D. Richards (eds.), <u>Health care and</u>

popular medicine in nineteenth century England, London Croom Helm.

Inman, T., (1844), 'Tables of the mortality after operations', Lancet, ii, 39-40.

Jackson, R., (1791), A treatise on the fevers of Jamaica with some observations on the intermitting fever of America, London, Murray.

-- -- -- (1798), An outline of the history and cure of fever... of the West Indies..., Edinburgh and London, Mundell and Longman.

-- -- -- (1808), An exposition of the practice of affusing cold water on the surface of the body as a remedy for the cure of fever, Edinburgh, Martin.

-- -- -- (1817), 'Comparative returns of sick of the army serving in the Windward and Leeward Islands and Colonies, from 1803 to 1814 included', Trans. med. soc. Lond., 1; 281-296.

-- -- -- (1821), Remarks on the epidemic yellow fever which has appeared at intervals at the south coast of Spain..., London, Underwood.

J. amer. med. Ass., (1968), 'Matthew Dobson (1735-1784), clinical investigator of diabetes mellitus', Editorial, 205; 698.

James, R., (1764), A dissertation on fevers and inflammatory distempers...to which is added an account of the success with which this medicine has been given in the small-pox, yellow fever...and rheumatism, 6th ed., London, Newbery.

-- -- -- (1770), The same, 7th ed., ibid.

James, R.R., (1916), 'Two celebrated salopian surgeons', Trans. Shropshire arch. Soc., reprint.

Jarcho, S., (1957), 'John Mitchell, Benjamin Rush and yellow fever', Bull. Hist. Med., 31; 132-136.

Johnson, J., (1813), The influence of tropical climates, more especially the climate of India, on European constitutions, London, Stockdale.

-- -- -- (1827), The same, 4th ed., London, Underwood.

Kaiser, W., (1970), 'Johann Ulrich Bilguer 1720-1796 und die medizinische Fakultät Halle', Gesnerus, 27; 85-95.

Kasich, A.M., (1946), 'William Prout and the discovery of hydrochloric acid in the gastric juice', Bull. Hist. Med., 20; 340-358.

Keith, W., (1844A),'Hospital statistics of stone in the bladder..., in the Royal Infirmary of Aberdeen..., (1838-1843), Edinb. med. surg. J., Vol. 61; pp. 123-129.

Kershaw, R., (1913), 'British ear and throat clinics historically considered', J. Laryng., 28; 421-427.

King, L.S., (1961), 'The blood-letting controversy: a study in the scientific method', Bull. Hist. Med., 35; 1-13.

-- -- -- (1971), The medical world of the eighteenth century, Huntingdon, N.Y., Krieger.

-- -- -- (1976), 'Evidence and its evaluation in eighteenth century medicine', Bull. Hist. Med., 50; 174-190.

King, T., (1832), Lithotrity and lithotomy compared, being an analytical examination of the present methods of treating stones in the bladder, London, Longman.

Kirkland, T., (1767), An essay towards an improvement in the cure of those diseases which are the causes of fever, London, Dodsley.

-- -- -- (1770), Observations upon Mr. Pott's general remarks on fractures, London, Becket and De Hondt.

-- -- -- (1774), A treatise on childbed-fevers, and on the methods of preventing them, London, Baldwin, repr. in: Churchill 1849, pp. 281-334.

-- -- -- (1780), Thoughts on amputation..., London, Dawson.

-- -- -- (1783), An inquiry into the present state of medical surgery, London, Dodsley.

Kirkpatrick, T.P.C., (1913), The book of the Rotunda Hospital, London, Adlard.

Klein, C., (1801), Chirurgische Bemerkungen, Stuttgart, Loeslund.

-- -- -- (1816-1819), Practische Ansichten der bedeutendsten chirurgischen Operationen auf eigene Erfahrungen gegründet, Stuttgart, Hasselbrink.

Koelbing, H.M., (1967), 'Die "Ärztliche Ethik" des Thomas Percival', Schweiz. med. Wschr., 97; 713-716.

-- -- -- (1978), 'Thomas Kirkland (1722-1798), und seine "Untersuchung über den gegenwärtigen Zustand der Medizinal-Chirurgie"', Schweiz. Rundschau Med. (Praxis), 67; 353-356.

Langenbeck, C.J.M., (1802), Eine einfache und sichere Methode des Steinschnittes, Würzburg, Stahel.

Laslett, P., (1971), The world we have lost, London, Methuen, 2nd ed.

-- -- -- (1973), 'Introduction' to J. Graunt, Natural and political observations made upon the bills of mortality (1662), reprinted by Gregg International Publishers Westmead, Farnborough, Hants.

Lauer, H.H., (1966), 'Zahl und Medizin', Janus, 53; 161-193.

Lawrence, C., 'Philosophy and Medicine in the Scottish Enlightment', in preparation.

Lawrie, J.A., (1841), 'On the results of amputations', Lond. med. Gaz., N.S., 1; 394-405.

Laws, M.E.S., (1955), 'Medicine at Montevideo 1807', Lancet, ii; 610-611.

Leake, C.D., (ed.), (1975), Percival's medical ethics, Huntington, N.Y., Krieger.

Leake, J., (1772), Practical observations on the child-bed fever, repr. in: Churchill 1849, pp. 117-203.

Le Dran, H.F., (1730), Parallèle des différentes manières de tirer la pierre hors de la vessie, Paris, Osmont.

-- -- -- (1731), Observations de chirurgie..., Paris, Osmont.

-- -- -- (1737), Traité ou reflexions tirées de la pratique sur les playes d'armes à feu, Paris, Osmont.

-- -- -- (1774), Traité des opérations de chirurgie, new ed., Bruxelles, Veuve Vase.

Lemprière, W., (1799), Practical observations on the diseases of the army in Jamaica as they occured between 1792 and 1797..., London, Longman and Rees.

-- -- -- (1812 ?), Report on the medicinal effects of an aluminous chalybdate spring...in the Isle of Wight, Newport, Musson and Taylor.

Lesky, E., (1973), 'Gerard van Swieten. Auftrag und Erfüllung', in: E. Lesky and A. Wandraska (eds.), Gerard van Swieten und seine Zeit, Wien, Köln, Graz, Böhlaus.

-- -- -- (1976), The Vienna Medical School of the 19th century, Baltimore and London, John Hopkins Univ. Press.

-- -- -- (n.d.), 'Theodor Billroth 1829-1894', in: R. Dumesnil and H. Schadewaldt, (eds.), Die berühmten Ärzte, 2nd ed., Köln, Aulis; pp. 327-329.

Lettsom, J.C., (1772), On the general treatment and cure of fevers, London, Cornish.

-- -- -- (1774), Medical memoirs of the General Dispensary in London, London, Dilly.

-- -- -- (1775), On the improvement of medicine in London on the basis of public good, London, Phillips.

Lewis, M.A., (1960), Social history of the Royal Navy, 1793-1815, London, Allen and Unwin.

Lewis, (Sir) A., (1967), 'Philippe Pinel and the English', _in_: _The state of psychiatry_, London, Routledge and Kegan Paul, pp. 9-17.

Lind, J., (1757), _A treatise on the scurvy_, 2nd ed., London, Millar.

-- -- -- (1772), The same, 3rd ed., London, Crowder. _et al_.

-- -- -- (1953), The same, ed. by C.P. Stewart and D. Guthrie, repr. Edinburgh, Univ. Press.

-- -- -- (1762), _An essay on the most effectual means of preserving the health of seamen, in the Royal Navy_, 2nd ed., London, Wilson.

-- -- -- (1763), _Two papers on fevers and infection_, London, Wilson.

-- -- -- (1768), _An essay on diseases incidental to Europeans in hot climates_, London, Beckett and de Hondt.

Liston, R., (1828), List of cases in which the operation of lithotomy was performed, _Edinb. med. surg. J., 29_; 236-238.

Livesley, B., (1975), 'The resolution of the Heberden-Parr controversy', _Med. Hist., 19_; 158-171.

Lloyd, C., (1965), 'Sir Gilbert Blane', _in_: C. Lloyd (ed.), _The health of seamen_, London, Navy Records Society, pp. 132-134.

Lloyd C. and Coulter, J.L.S., (1961), Medicine and the Navy, Edinburgh, Livingstone.

Louis, A., (1757), 'Rapport des expériences faites par l'Académie, sur différentes méthodes de tailler', Mém. Acad. roy. Chir., (Paris), 3; 623-656.

Louis, P.C.A., (1825), Recherches anatomico-pathologiques sur la phtisie, Paris, Gabon.

-- -- -- (1828), 'Recherches sur les effets de la saignée dans quelques maladies inflammatoires', Arch. gén. Méd., 18; 321-326.

-- -- -- (1835), Recherches sur les effets de la saignée dans quelques maladies inflammatoires et sur l'action de l'émétique et des vésicatoires dans la pneumonie, Paris, Baillière.

-- -- -- (1837), 'De l'examen des malades et de la recherche des faits généraux', Mém. Soc. méd. Obs., (Paris), 1; 1-63.

Lucas, J., (1776), 'On the amputation of the ankle with a flap', Med. Obs. Inq., 5; 323-328.

Luscombe, E.T., (1820), Practical observations on the means of preserving the health of soldiers..., Edinburgh, Constable.

Lyon, W., (1844), 'On the statistics and treatment of fractures during two years service as surgeon to the Glasgow Royal Infirmary', Monthly J. med. Sci., 4; 16-32, 89-100.

Lyons, J.B., (1961-62), The letters of Sylvester O'Halloran (1), N. Munster antiq. J., 8; 168-181.

-- -- -- (1962-63), The letters of Sylvester O'Halloran (2), N. Munster antiq. J., 9; 25-50.

-- -- -- (1963), Sylvester O'Halloran (1728-1807), Irish J. med. Sci., 217-232, 279-288.

Macalpine, I. and Hunter, R., (1969), George III and the mad-business. London, Allen Lane, Penguin Press.

Macbride, D., (1764), Experimental essays, London, Millar.

-- -- -- (1767A), The same, 2nd ed., Dublin, Ewing.

-- -- -- (1767B), An historical account of a new method of treating the scurvy at sea..., London, Millar and Cadell.

-- -- -- (1772), A methodical introduction to the theory and practice of physic, London, Strahan et al.

-- -- -- (1787), The same, transl, of 2nd ed., Introduction methodique a la theorie et la pratique de le medecine Paris, Duplain.

Mc Conaghey, R.M.S., (1967), 'Sir George Baker and the Devonshire colic', Med. Hist., 11; 345-360.

-- -- -- (1969), John Huxham, Med. Hist., 13; 280-287.

Mc Grigor, (Sir) J., (1802), 'Account of diseases of the 88th Regiment, during their passage to India, and at Bombay, from December 1798 till June 1800', Ann. Med., Lustrum 2, 1; 353-369.

-- -- -- (1804), Medical sketches of the expedition to Egypt from India, London, Murray.

-- -- -- (1810), 'Observations on the fever which appeared in the army from Spain on their return to this country in January 1809', Edinb. med. surg. J., 6; 19-32.

-- -- -- (1815), 'Sketch of the medical history of the British Armies in the Peninsula of Spain and Portugal, during the late campaigns', Med.-chir. Trans., 6; 381-489.

-- -- -- (1861), The autobiography and services of Sir James Mc Grigor, Bart., London, Longman et al.

Mc Lean (Sir) C., (1817-1818), Results of an investigation respecting epidemic and pestilential diseases..., London, Underwood.

-- -- -- (1818), Practical illustrations of the progress of medical improvement for the last thirty years, London, for the author.

Machardy, F.N., (1841), The double-flap and circular amputations contrasted, London, Simpkin et al.

Malgaigne, J.F., (1842), Études statistiques sur les résultats des amputations dans les hôpitaux de Paris, Paris, Locquin.

Manley, G., (1952), 'The weather and diseases: some eighteenth-century contributions to observational metereology', Notes Rec. R. Soc. London., 9; 300-307.

Mann, G., (1958), 'Ernst Gottfried Baldinger, und sein Magazin für Ärzte', Sudhoffs Arch. Gesch. Med., 42; 312-318.

Manning, H., (1780), Modern improvements in the practice of physic and surgery, London, Robinson and Murray.

Marcet, A., (1817), An essay on the chemical history and medical treatment of calculous disorders, London, Longman et al.

-- -- -- (1819), The same, 2nd ed., ibid.

Marmion, T., (1949), 'A forgotten chapter in the history of medical statistics', Med. ill. (Lond.), 3; 266-270.

Marshall, H., (1833), 'Contribution to statistics of the army, with some observations on military medical returns', Edinb. med. surg. J., 40; 36-44.

Martineau, P., (1821), 'On lithotomy', Med.-chir. Trans., 11; 402-413.

Maty, M., (1769), 'An essay on the advantages of very early inoculation', Med. Obs. Inq., 3; 2nd ed.; 287-307.

Merriman, S., (1816), 'Cases of premature labour artifically induced in women with distorted pelvis', Med.-chir. Trans. 3; 2nd ed., 123-145.

-- -- -- (1819), 'Cases of tumours within the pelvis impeding parturition; with remarks', Med.-chir. Trans., 10; 50-76.

-- -- -- (1820), A synopsis of the various kinds of difficult parturition, with practical remarks on the management of labour, 3rd ed., London, Callow.

Méry, J., (1700), Observations sur la manière de tailler dans les deux sexes pour l'extraction de la pierre, practiquée par Frère Jaques, Paris, Boudot.

Millar, J., (1769), Observations on the asthma, and on the hooping cough, London, Cadel et al.

-- -- -- (1770), Observations on the prevailing diseases in Great Britain, London, Cadell.

-- -- -- (1798), The same, 2nd ed., London, for the author.

-- -- -- (1774), Observations on antimony, London, Johnson et al.

-- -- -- (1776), A discourse on the duty of physicians, London, Johnson.

-- -- -- (1777), Observations on the practice in the Medical Department of the Westminster General Dispensary..., London, by order of the Governors.

-- -- -- (1778-1779), Observations on the management of diseases in Army and Navy, London, for the author.

-- -- -- (1783), Observations on the management of diseases in the Army and Navy...in reply to Doctor Monro, London, for the author.

-- -- -- (1798), Observations on the conduct of the war, in an appeal to the people of Great Britain..., London, for the author.

-- -- -- (1802-1803 ?), Observations on the change of public opinion in religion, politics and medicine..., London, Barfield.

Miller, A., (1831), An inquiry into the average mortality in lithotomy cases, with few remarks on the operation of lithotrity, Edinburgh, Shortreed.

Miller, G., (1957), The adoption of inoculation for smallpox in England and France, Philadelphia, Univ. Pennsylvania Press.

-- -- -- (1962), 'Airs, waters, and places in history', J. Hist. Med., 17; 129-140.

Millingen, J.G., (1819), The army medical officer's manual upon active service..., London, Burgess and Hill.

-- -- -- (1837), Curiosities of medical experience, London, Bentley.

Milt, B., (1956), 'Empirie und das statistisch fundierte biologisch-medizinische Denken in der Geschichte', Gesnerus, 13; 1-28.

Mingay, G.E., (1963), English landed society in the eighteenth century, London and Toronto, Routledge and Kegan Paul, Univ. Toronto Press.

Mitaritonna, O., (1968), 'Applicazioni dei metodi quantitativi nelle varie discipline della scienza medica negli ultimi cento anni', in: Int. Congr. Hist. Med., 1968, Vol. 1; pp. 615-622.

Monro, A., primus. (1737), 'Remarks on the amputations of the larger extremities', Med. Ess. Obs., 4; 321-347.

-- -- -- (1781), The works of Alexander Monro, Edinburgh and London, Elliot and Robinson.

Monro, A., secundus, (1776-1784), Lectures on surgery..., transcribed from the copy...as taken down in shorthand by W. Thorburn, Royal College of Surgeons of England, Manuscript No. 42a 63.

Monro. D., (1764), Account of the diseases which were most frequent in the British military hospitals in Germany, London, Millar et al.

-- -- -- (1771 A), 'Cases of aneurysms, with remarks', Ess. phys. lit. Soc. Edinb., 3; 178-291.

-- -- -- (1771 B), 'Of the use of mercury in convulsive disorders', Ess. phys. lit. Soc. Edinb., 3; 551-556.

-- -- -- (1780), Observations on the means of preserving the health of soldiers and of conducting military hospitals, London, Murray, et al.

Morand, S.F., (1728), Traité de la taille au haut appareil..., Paris, Cavelier.

-- -- -- (1731), 'Recherches sur l'opération de la taille par l'appareil latéral', Hist. Acad. roy. Sci., 1731; 144-159.

-- -- -- (1768-1772), Opuscules de chirurgie, Paris, Desprez.

Morris, C., (1751), Observations on the past growth and present state of the City of London, to which are annexed a complete table of christenings and burials...1601-1750 - together with a table of the numbers which have annually died of each disease from 1675 to the present time, London.

-- -- -- (1759), The same, ed. by J.P. and T. Birch.

Morton, L.T., (1954), Garrison and Morton's medical bibliography, 2nd ed., London, Morton.

Moseley, B., (1789), A treatise on diseases, on military operations and on the climate of the West-Indies, London, Cadell.

Muellener, E.R., (1964), 'Genfer Medizinalstatistik und Hygiene in der ersten Hälfte des 19 Jahrhunderts: André-Louis Gosse (1791-1873), Jacob-Marc d'Espine (1805-1860) und Henri-Clermond Lombard (1803-1895)', Gesnerus, 21; 154-192, ~~1964~~.

-- -- -- (1966), 'Zur methodischen therapeutisch - klinischen Forschung der "Ecole de Paris" (1800-1850)', Gesnerus, 23; 122-131.

-- -- -- (1967), 'Pierre-Charles-Alexandre Louis' (1787-1872) Genfer Schüler und die "Methode numérique"', Gesnerus, 24; 46-74, 1967.

Müller, H., (1968), 'Johann Ulrich Bilguer (geb. 1720 in Chur, gest. 1796 in Berlin)', Gesnerus, 25; 116-120.

Munk, W., (1878), The roll of the Royal College of Physicians of London, 2nd ed., London, by the College.

Murray, T.A., (1801), Remarks on the situation of the poor in the metropolis as contributing to the progress of contagious disease...,with a plan for the institution of houses of recovery, London, Noble.

Murrell, S., (1949), Physician extraordinary, (Sir Gilbert Blane), London, Hodder and Stoughton.

Neale, H. StJ., (1804), Chirurgical institutes, drawn from practice, on the knowledge and treatment of gun-shot wounds..., London, Jones.

Neuburger, M., (1943), The doctrine of the healing powers of nature throughout the course of time, New York, (Xerocopy).

-- -- -- (1950), 'Francis Clifton and William Black. Eighteenth century critical historians of medicine', J. Hist. Med., 5; 44-49.

Neve, M., 'The medical profession and the Bath waters in the eighteenth century', in preparation.

Newman, (Sir) G., (1932), The rise of preventive medicine, Oxford, Univ. Press.

Niebyl. P.C., (1977), 'The English bloodletting revolution, or modern medicine before 1850', Bull. Hist. Med., 51; 464-483.

O'Halloran, S., (1765), A complete treatise on gangrene and sphacelus with a new method of amputation, London, and Dublin, Vaillant and Wilson.

O'Halloran, T., (1824), Aperçu succint de la fièvre jaune telle qu' elle a régné dans l'Andalousie en 1820, French translation, Paris, Chevot et al.

Oldham, J.B., (1977), 'The dawn of surgery in Liverpool', in: Ross, J.A. (ed.), Collected papers concerning Liverpool medical history, Liverpool, part 2, pp. 14-27.

Osler, W., (1908), 'The influence of Louis on American medicine', in: An Alabama student and other biographical essays, Oxford, Univ. Press, pp. 189-210.

Park, H., (1783), An account of a new method of treating diseases of the joints..., in a letter to Mr. Percival Pott, London, Johnson, 1783. (Date erroneously printed as 1753).

Park, H. and Moreau, H., (1806), Cases of the excision of carious joints, Glasgow, Univ. Press.

Parker, E., (1841), 'Statistical account of the amputations performed at the Northern Hospital, Liverpool', Lond. med. Gaz., N.S. 2; 269-272.

Parkes, E.A., (1862), Memoir of Sir John Forbes, London, Savill and Edwards.

Parkinson, J.W.K. (ed.), (1833), Hunterian reminiscences: being the substance of a course of lectures on the principles and practice of surgery delivered by the late Mr. John Hunter in the year 1785, London, Sherwood.

Parsons, G.P., (1978), 'The British medical profession and contagion theory: puerperal fever as a case study, 1830-1860', Med. Hist., 22; 138-150.

Peachey, G.C., (1914), 'William Bromfield, 1713-1792', Proc. roy. Soc. Med., 8; 103-126.

-- -- -- (1916), The same, Janus, 21; 27-50.

Pearson, K., (1978), The history of statistics in the 17th and 18th centuries against the changing background of intellectual, scientific and religious thought, London, Griffin.

Peck, T.W. and K.D. Wilkinson, (1950), William Withering of Birmingham, Bristol, Wright.

Pelling, M., (1978), Cholera, fever and English medicine 1825-1865, Oxford Univ. Press.

Percival, T., (1767), Essays medical and experimental,1st series, quoted from 2nd ed., London, Johnson 1772.

-- -- -- (1773), The same, _2nd series_, London, Johnson.

-- -- -- (1827), Medical ethics, 2nd ed., London, Jackson, 1827.

Perkin, H., (1972), _The origins of modern English society 1780-1880_, London, Routledge and Kegan Paul.

Peter, J.P., (1975), 'Disease and the sick at the end of the eighteenth century', _in_: R. Forster and O. Ranum (eds.), _Biology of man in history_, Baltimore and London, John Hopkins Univ. Press, pp. 81-124.

Peterkin, A. and Johnston, W., (1968), _Commissioned officers in the medical services of the British Army 1660-1960_, Vol, 1, London, Wellcome Historical Medical Library.

Petersen, J., (1877), Hauptmomente in der geschichtlichen Entwickelung der medicinischen Therapie, Kopenhagen, Höst.

Pettigrew, T.J., (1817), _Memoirs of the life and writings of the late John Coakeley Lettsom_...with a selection from his correspondence, London, Longman _et al_.

Phillips, B., (1832), _A treatise on the urethra_, London, Longman.

-- -- -- (1837-1838), 'Observations on the results of amputations in this and other countries', summary, _Lancet, i_; 269.

-- -- -- (1838), 'Result of amputation in different countries', _Lond. med. Gaz._, N.S., _2_; 457-463.

Pinel, P., (1790-91), Abrégé des Transactions Philosophiques de la Société Royale de Londres, parts 6 and 7, Paris.

-- -- -- (1806), Treatise on insanity, Sheffield, Todd.

Piquemal, J., (1974), 'Succès et décadence de la méthode numérique en France à l'époque de Pierre-Charles-Alexandre Louis', Méd. de Fr., (250), 11-60.

Porter, A., (1963), 'Thomas Trotter, M.D., naval physician', Med. Hist., 7; 155-164.

Posner, E., (1975), 'William Withering versus the Darwins', Hist. Med., 6; (1) 51-57; (2) 54.

Pott, P., (1763) A treatise on ruptures, London, Hawes et al.

-- -- -- (1769), Some few general remarks on fractures and dislocations, London, Hawes et al.

-- -- -- (1771), The chirurgical works, London, Hawes et al.

-- -- -- (1779), Remarks on that kind of palsy...to which are added observations on the necessity of amputation in certain cases and circumstances, London, Johnson.

-- -- -- (1790), The chirurgical works of Percival Pott, London, Johnson.

Potter, J.P., (1841 A), 'Results of amputations at University College Hospital, statistically arranged', Med.-chir. Trans., 24; 155-176.

-- -- -- (1841 B), The same, summary, *Lancet, ii*; 345-348.

Power, (Sir) D., (1934), 'The history of amputation of the breast to 1904', reprint, *Lpool med.-chir. J.*, *42*.

-- -- -- (1936), 'Ipsissima verba XI. Two Liverpool surgeons, 1. Henry Park, who excised joints in 1781', *Brit. J. Surg., 24*; 205-207.

-- -- -- (1937), 'Ipsissme verba XII. Two Liverpool surgeons. 2. Edward Alanson, a pioneer in hospital sanitation', *Brit. J. Surg., 24*; 421-424.

-- -- -- (1939), (ed.), *British medical societies*, London, Medical Press.

Poynter, F.N.L., (ed.), (1964), *The evolution of hospitals in Britain*, London, Pitman.

Poynter, J.R., (1969), *Society and pauperism. English ideas on poor relief, 1795-1834*, London and Toronto, Routledge and Univ. Toronto Press.

Pringle, (Sir) J., (1742), 'Vinum antimonii ceratum, a specific medicine in the dysentery', *Med. Ess. Obs.,5*; 194-221.

-- -- -- (1750), *Observations on the nature and cure of hospital jayl fevers...*, London, Millar and Wilson.

--- --- --- (1752), Observations on the diseases of the army in camp and garrison..., London, Millar and Wilson.

--- --- --- (1776), A discourse upon some late improvements of the means for preserving the health of mariners..., London, Royal Society.

Probst, C., (1972), Der Weg des ärztlichen Erkennens am Krankenbett. Hermann Boerhaave und die ältere Wiener medizinische Schule..., Sudhoffs Arch. Gesch. Med., Beiheft 15, Wiesbaden, Steiner.

Prout, W.M., (1821 A), An inquiry into the nature and treatment of gravel, calculus and other diseases connected with a deranged operation of the urinary organs, London, Baldwin et al.

--- --- --- (1821 B), Reply to Dr. Yelloly's remarks on the estimate of mortality from the operation of lithotomy, Ann. Phil. N.S. 6; 426-427.

Pym, (Sir) W., (1848), Observations upon Bulam, vomito-negro or yellow fever, 2nd ed., London, Churchill.

Quarrier, D., (1817), 'Report of the state of the wounded on board the Leander in the action before Algiers', Med.-chir. Trans., 8; 1-11.

Quetelet, L.A.J., (1835), Sur l'homme et le développement de ses facultés ou essai de physique sociale, Paris, Bachelier.

Radcliffe, W., (1976), 'The Colchester Medical Society, 1774', Med. Hist., 20; 394-401.

Ranby, J., (1744), The method of treating gun-shot wounds, London, Knapton.

Razzell, P., (1977), The conquest of smallpox, Firle (Sussex), Caliban Books.

Reide, T.D., (1793), A view of the diseases of the army in Great Britain, America, the West Indies and on board of King's ships..., from the beginning of the late war to the present time, together with monthly and annual returns of the sick..., London, Johnson.

Relph, J., (1794), An inquiry into the medical efficacy of a new species of Peruvian bark, London, Phillips.

Renwick, W., (1792), An inquiry into the nature and causes of sickness in ships of war, London, Evans.

Rhodes, P., (1977), Doctor John Leake's Hospital, London, Davis-Poynter.

Rigby, E., (1783), An essay on the use of the red peruvian bark in the cure of intermittents, London, Johnson.

-- -- -- (1822), An essay on the uterine haemmorrhage which precedes the delivery of the full-grown foetus, 6th ed., London, Hunter.

Risse, G.B., (1970), 'The Brownian system of medicine. Its theoretical and practical implications', Clio med., 5; 45-51.

-- -- -- (1971), 'The quest for certainty in medicine: John Brown's system of medicine in France', Bull, Hist. Med., 45; 1-12.

-- -- -- (1974), 'Doctor William Cullen, physician, Edinburgh', Bull. Hist. Med., 48; 338-351.

Robertson, R., (1777), A physical journal kept on board His Majesty's Ship Rainbow..., London, Dilly.

-- -- -- (1783), Observations on the jail, hospital or ship fever, London, Murray.

-- -- -- (1789), The same, 2nd ed., London, for the author.

-- -- -- (1790), An essay on fevers, London, for the author.

-- -- -- (1804), Observations on the diseases incident to seamen, 2nd ed., London, for the author.

-- -- -- (1810-1811), Synopsis morborum. A summary view of observations on the principal diseases incident to seamen, London, for the author.

Roddis, L.H., (1936), 'William Withering and the introduction of digitalis into medical practice', Ann. med. Hist., 8; 185-201.

-- -- -- (1950), _James Lind, founder of nautical medicine_, New York, Schuman.

Rolleston, (Sir) H.D., (1915), 'James Lind, pioneer of naval hygiene', _reprint, J. roy. Nav. Med. Serv., 1_

-- -- -- (1916), 'Sir Gilbert Blane, M.D., F.R.S., 1749-1834. An administrator of naval medicine and hygiene', reprint, _J. roy. Nav. Med. Serv., 2_.

-- -- -- (1919), 'Thomas Trotter, M.D. (1760/1832), _in:_ W. Osler (ded.), _Contributions to medical and biological research_, New York, Hoeber, Vol. 1, pp. 153-165.

-- -- -- (1930), 'Medical friendships, clubs and societies', _Ann. med. Hist_. N.S., _2_; 249-266.

-- -- -- (1931), 'History of cinchona and its therapeutics', _Ann. Sci., 3_; 261-270.

Rollo, J., (1780), _Observations on the diseases which appeared in the army on St. Lucia_..., Barbados, Orderson for the author.

-- -- -- (1781), The same, London, Dilly.

-- -- -- (1786), _Observations on the acute dysentery_, London, Dilly.

-- -- -- (1801), A short account of the Royal Artillery Hospital at Woolwich, London, Mawman, 1801.

-- -- -- (1804), Medical report of cases of inoculation and re-inoculation with variolous and vaccine matter, London, Mawman et al.

Rook, A., (1960), 'General practice 1793-1803: the Transactions of a Huntingdonshire Medical Society', Med. Hist., 4; 236-252, 330-347.

Rose, T., (1817), 'Observations on the treatment of syphilis...' Med.-chir. Trans., 8; 349-426.

Rosen, G., (1955), 'Problems in the applications of statistical analysis to questions of health 1700-1880', Bull. Hist. Med., 29; 27-45.

-- -- -- (1958), History of Public Health, New York, M.D. Publications.

Rowley, W., (1788), The causes of the great number of deaths amongst adults and children, quoted from 2nd ed. London, Newbery, 1793.

-- -- -- (1793), Dr. Rowley's rational practice of physic in four volumes, London, for the author.

-- -- -- (1804), A treatise on putrid, malignant, infectious fevers and how they ought to be treated..., London, Barfield.

Rumsey, J., (1826), Some account of the life and character of the late Thomas Bateman, London, Longman.

Sabatier, R.B.,(1796), De la médecine opératoire..., Paris, Didot.

--- --- --- (1824), The same, new ed. by L.J. Sanson and L.J. Bégin, Paris, Béchet.

Saucerotte, N., (1801), Mélanges de chirurgie, Paris, Gay.

Saunders, W., (1782), Observations on the superior efficacy of the red Peruvian bark in the cure of agues and other fevers, 2nd ed., London, Johnson.

Selwyn S., (1966), 'Sir John Pringle: Hospital reformer, moral philosopher and pioneer of antiseptics', Med. Hist., 10; 266-274.

Serre, M., (1830), Traité de la réunion immédiate, Paris, Gabon.

Seydel, H., (1976), Statistik in der Medizin. Ein Entwurf zu ihrer Geschichte, Kieler Beitr. Gesch. Med. Pharm. Vol. 15, Neumünster, Wachholtz.

Sheehan, D., (1942), 'Charles White, eighteenth century surgeon', Ann. med. Hist., 3rd ser. 4; 136-146.

Shelley, H.S., (1958), 'Cutting for the stone', J. Hist. Med., 13; 50-67.

Shepherd, J.A., (1965), Spencer Wells, London and Edinburgh, Livingstone.

Shrewsbury, J.F.D., (1970), A history of bubonic plague in the British Isles, Cambridge, Univ. Press.

Shryock, R.H., (1948), The development of modern medicine, London, Gollancz.

-- -- -- (1961), 'The history of quantification in medical science', Isis, 52; 215-237.

Sigerist, H.E., (1936), 'Introduction' to 'On percussion of the chest being a translation of Auenbrugger's original treatise by John Forbes', Bull Inst. Hist. Med., Johns Hopkins Univ., 4; 373-403.

Sims, J., (1773), Observations on epidemic disorders with remarks on nervous and malignant fevers, London, Johnson.

-- -- -- (1774), A discourse on the best method of prosecuting medical enquiries..., London, Hart.

Singer, D.W., (1949-1950), 'Sir John Pringle and his circle', Ann. Sci., 6; 127-180, 229-261.

Singer, C. and Underwood, E.A., (1962), A short history of medicine, 2nd ed., Oxford, Clarendon Press.

Smith, A., (1847), 'David Macbride, M.D.', reprint, Dublin quart. J. med. Sci.

Smith, A., (1821-1823), 'Report of cases treated at the Ophthalmic Hospital, Chatham, communicated by Professor Thomson', Edinb. med. surg. J., 17; 349-375, 495-509, 18; 509-519, 19; 13-26.

Smith, D.C., (1976), 'Quinine and fever: the development of the effective dosage', J. Hist. Med., 31; 343-367.

Smith, R., (1820), 'A statistical inquiry into the frequency of stone in the bladder in Great Britain and Ireland, Med.-chir. Trans., 11; 1-53.

Smyth, J.C., (1796), An account of the experiment made at the desire of the Lords Commissioners of the Admiralty..., London, Johnson.

-- -- -- (1799), The effect of the nitrous vapour in preventing and destroying contagion..., London, Johnson.

Sorsby, A., (1936), 'Defunct London eye hospitals', Brit. J. Ophthalm., 20; 77-98.

-- -- -- (1946), 'Nineteenth century provincial eye hospitals', Brit. J. Ophthalm., 30; 501-546.

Spencer, H.R., (1927), History of British midwifery from 1650 to 1800, London, Bale.

Sprigge, S.S., (1897), The life and times of Thomas Wakley, London, Longmans.

Sutton, T., (1806), A practical account of a remittent fever frequently occuring among the troops in this climate, Canterbury and London, Simmons and Robinson.

Swieten, van G., (1776), The diseases incident to armies with the method of cure...to which is added the nature and treatment, of gun-shot wounds. By John Ranby, Philadelphia, Bell.

Syme, J., (1830), Lithotomy at Edinburgh surgical hospital, Edinb. med. surg. J., 33; 240-241.

Schadewaldt, H., (1955), 'Der Schiffsarzt', Ciba Z., 7; 2502-2536.

Schipperges, H., (1971), 'Zur Einführung statistischer Methoden in Medizin und Biologie', Die Kapsel, (Hauszeitschift Scherer GmbH, Eberbach/Baden) 27; 1115-1125.

Schmitz-Cliever, E., (1964), 'Der Bruch und Steinschneider Jacques Beaulieu ("Frère Jacques") in der Reichsstadt Aachen (1698)', Sudhoffs Arch. Gesch. Med., 48; 18-26.

Schofield, R.E., (1963), The Lunar Society of Birmingham, Oxford, Clarendon Press.

Stancliffe, F.S., (1964), The Manchester Eye Hospital, 1814-1964. Manchester, Univ. Press.

Stanger, C., (1802), Remarks on the necessity and means of suppressing contagious fever in the metropolis, London, Phillips.

Steiner, W.R., (1939), 'Some distinguished American medical students of Pierre-Charles-Alexander Louis of Paris', Bull. Inst. Hist. Med. Johns Hopkins Univ., 7; 783-793.

-- -- -- (1940), 'Dr. Pierre Charles Alexander Louis. A distinguished Parisian teacher of American medical students', Ann. med. Hist., 3rd Ser., 2; 451-458.

Stevenson, L.G., (1964), 'John Hunter. Surgeon general 1790-1793', J. Hist. Med., 19; 239-254.

Stewart. C.P., (1953), The third edition of the Treatise, in: C.P. Stewart and D. Guthrie (eds.) Lind's treatise on scurvy, Edinburgh Univ. Press, pp. 361-368.

Temkin, O., (1951), 'The role of surgery in the rise of modern medical thought', Bull Hist. Med., 25; 248-259.

-- -- -- (1955), 'Therapeutic trends and the treatment of syphilis', Bull. Hist. Med., 29; 309-316.

Theden, J.C.A., (1778), Unterricht für die Unterwundärzte bey Armeen, 2nd ed., Berlin, Nicolai.

Thomson, F., (1790), An essay on the scurvey...with some observations on fevers, London, for the author.

Thomson, J., (1808), Observations on lithotomy, Edinburgh and London, Blackwood and Longman.

-- -- -- (1813), Lectures on inflammation, Edinburgh and London, Blackwood and Cadell.

-- -- -- (1816), Report of observations made in the British military hospitals in Belgium after the battle of Waterloo, Edinburgh and London, Blackwood and Cadell.

-- -- -- (1818 A), 'Observations on the treatment of syphilis without mercury', Edinb. med. surg. J., 14: 84-91.

-- -- -- (1818 B), 'Report of cases of gonorrhoea in the Hospital of the Castle of Edinburgh', Edinb. med. surg. J., 14; 263-264.

-- -- -- (1843-1844), 'Statistics of eight of the principal civil hospitals in Scotland...', Edinb. med. surg. J., 60; 341-362, 61; 75-103.

Thomson-Walker, (Sir) J., (1935), 'John Green Crosse of Norwich', Proc. roy. Soc. Med., 28; 391-398.

Toellner, R., (1965), C.C. v. Klein. Ein Wegbereiter wissenschaftlicher Chirurgie in Würtemberg, Stuttgart, Fischer, 1965.

Tolet, F., (1708), Traité de la lithotomie, 5th ed., Paris, Muguet.

Treyeran, J.A., (1802), Parallèle des diverses méthodes proposées pour l'extraction des calculs vésicaux, et description d'un nouveau procédé préférable à tous ceux usités jusqu'à ce jour, Paris, Richard et al.

Triolo, V.A., (1969), 'The Institution for investigating the nature and cure of cancer', Med. Hist., 13; 11-28.

Trotter, T., (1786), Observations on the scurvy, Edinburgh, Elliot et al.

-- -- -- (1792), The same, 2nd ed., London, Longman.

-- -- -- (1797), Medicina nautica. An essay on the diseases of seamen, London, Cadell.

Turnbull, W., (1806), The Naval Surgeon, London, Phillips.

Turner, A.L., (1929), The Royal Infirmary of Edinburgh. Bicentenary Year 1729-1929, Edinburgh, Oliver and Boyd.

Turner, W., (1957), The Warrington Academy, Warrington, Library and Museum Committee.

Underwood, E.A., (1951), 'The history of the quantitative approach to medicine', Brit. med. Bull., 7; 265-274.

-- -- -- (1977), Boerhaave's men at Leyden and afterwards, Edinburgh, University Press.

Velpeau, A., (1839), Nouveaux éléments de médecine opératoire, 2nd ed., Paris, Baillière.

Vetch, J., (1807), An account of the ophthalmia which has appeared in England since the return of the British Army from Egypt, London, Longman.

-- -- -- (1818), Observations relative to the treatment by Sir William Adams of the ophthalmia cases of the army, London, Callow.

Viseltear, A.J., (1968), 'Joanna Stephens and the eighteenth century lithontriptics; a misplaced chapter in the history of therapeutics', Bull. Hist. Med., 42; 199-220.

Vess, D.M., (1975), Medical revolution in France, 1789-1796, Gainsville, Univ. Presses of Florida.

Vogeler, K., (1929), 'Johann Ulrich Bilguer und der Streit um die Amputation im 18. Jahrhundert', Münch. med. Wschr., 76; 750-754.

Wade, P.J., (1791), Select evidences of a successful method of treating fever and dysentery in Bengal, London, Murray.

Walls, E.W., (1964), 'John Bell, 1763-1820', Med. Hist., 8; 63-69.

Wangensteen, O.H., J. Smith and S.D. Wangensteen, (1967), 'Some highlights in the history of amputation reflecting lessons in wound-healing', Bull. Hist. Med., 41; 97-131.

Wangensteen, O.H., S.D. Wangensteen and J. Wiita, (1969), 'Lithotomy and lithotomists: progress in wound management from Franco to Lister', Surgery, 66; 929-952.

Wangensteen, O.H., S.D. Wangensteen and C.F. Klinger, (1972), 'Surgical clea nliness, hospital salubrity and surgical statistics, historically considered', Surgery, 71; 477-493.

Waldron, H.A., (1970), 'The Devonshire Colic', J. Hist. Med., 25; 383-413.

-- -- -- (1972), 'Sir George Baker and lead poisoning', in: Int. Congr. Hist. Med. London 1972, Vol. 2, pp. 1286-1290.

Watt, (Sir) J., (1978), 'The medical effects of Thomas Cook's voyages', in: Cook and his times, Proc. Bicentennial Conference, Simon Frazer Univ. Vancouver, in press.

Watts, M.R., (1978), The dissenters from the reformation to the French revolution, Oxford Clarendon.

Weaver, G.H., (1928), 'John Haygarth, clinician, investigator, apostle of sanitation 1740-1827', Bull Soc. Med. Hist. (Chicago), 4; 156-200.

Webster, C., (1977), 'The crisis of the hospitals during the industrial revolution', in: E.G. Forbes (ed.), Human implications of scientific advance, Proc. Int. Congr. Hist. Sci. Edinburgh 1977, pp. 214-223.

Weldon, W., (1794), Observations physiological and chirurgical on compound fractures, London, Crosby.

White, C., (1770), Cases in surgery, London, Johnston.

-- -- -- (1771), 'An account of a new method of amputating the leg...', Med. Obs. Inq., 4; 168-177.

-- -- -- (1773), Treatise on the management of pregnant and lying-in women...extracts on puerperal fever, repr. in Churchill 1849, pp. 205-280.

Wilson, J., (1812), 'Report of the treatment adopted for the cure of contagious fever among the seamen...that were sent ashore to the Royal Hospital at Plymouth', Edinb. med. surg. J., 8; 403.

Withering, W., (1785), An account of the foxglove and some of its medical uses..., Brimingham, Robinson.

Woodward, J.H., (1973), 'The British voluntary hospital movement - success or disaster?' Ann.Cis. Hist. soc. (4); 233-254.

-- -- -- (1974), To do the sick no harm. A study of the British voluntary hospital system to 1875, Londoh, Routledge and Kegan Paul.

Wright, W., (1797), 'Report concerning the diseases most common among the troops in the West Indies', Ann. Med. 2; 345-372.

Wright, W., (1828), Memoir of William Wright, M.D., F.R.S. and F.R.S.Ed., London and Edinburgh, Cadell and Blackwood.

Wright, St. Clair, R.E., (1964), Doctors Monro. A medical saga, London, Wellcome Historical Medical Library.

Wright Thomson, A.M., (1963), The Glasgow Eye Infirmary, 1824-1962, Glasgow, Smith.

Wrzosek, A., (1930), 'Les mérites de Louis et de Dietl en ce qui concerne la suppression dans la thérapeutique de l' abus des saignées', in: Int. Congr. Hist. Med.Rome 1930; pp. 436-440.

Wyatt, H.V., (1976), 'James Lind and the prevention of scurvy', Med. Hist., 20; 433-438.

Wyke, T.J., (1975), 'The Manchester and Salford Lock Hospital, 1818-1917', Med. Hist., 19; 73-86.

Yelloly, J., (1821), 'Observations on Dr. Prout's estimate of the mortality from the operation of lithotomy', _Ann. Phil._ N.S., _6_; 363.

-- -- -- (1829 A), 'Remarks on the tendency to calculous diseases with observations on the nature of urinary concretions', _Phil. Trans. roy. Soc., 119_; 55-81.

-- -- -- (1829 B), 'Observations on the statement made by Dr. Douglas of Cheselden's improved lateral operation...', _Med.-chir. Trans., 15_; 339-368.

-- -- -- (1830), 'Sequel' to Yelloly 1829(A), _Phil. Trans. roy. Soc., 120_; 415-428.

Zumstein, B., (1968), _Anton Stoerck (1731-1803) und seine therapeutischen Versuche_, M.D. thesis, Zürich, Juris.

ADDITIONAL BIBLIOGRAPHY

Admiralty of Great Britain, (1808), _Regulations and instructions relating to His Majesty's Service at sea_, London, pr. by Winchester.

Ballingall, (Sir) G., (1823), _Practical observations on fever, dysentery and liver complaints_, 2nd ed., Edinburgh, Black.

-- -- -- (1827A), _Review of some of the cases which have lately occured in the Royal Infirmary of Edinburgh_, Edinburgh, Balfour.

-- -- -- (1827B), Syllabus of the course of lectures on military surgery, Edinburgh, s.n.

Black, W., (1810), A dissertation on insanity; illustrated with tables and extracted from between two and three thousand cases in Bedlam, London, for the suthor.

Boerhaave, H., (1744), Aphorismi de cognoscendis et curandis morbis..., 6th ed., Edinburgh, Drummond.

Brickerton, T.H., (1936), Medical history of Liverpool, London, Murray.

Burnett, (Sir)W., (1831), An account of a contagious fever...amongst the...prisoners of War at Chatham..., London, Burgess and Hill.

Cartwright, F.F., (1967), The development of modern surgery, London, Barker.

Clutterbuck, H., (1807), An inquiry into the seat and nature of fever;..., London, Boosey.

Comrie, J.D., (1932), History of Scottish Medicine, London, Wellcome and Baillière

Cornelius, E.H., (1968), 'Some past presidents of the College and their contributions to the Library and Museum', Ann. roy. Coll. Surg. Engl., 43; 39-50;

Coutts, J., (1909), A history of the University of Glasgow, Glasgow, Maclehose.

Crowe, K.E., Medical arrangements of Wellington's Army during the Peninsular War 1809-1814, Ph. D. thesis, Univ. London, in preparation.

Davis, J.L., (1952), Westminster Hospital, London, Murray.

Heberden, W., (1801), Observations on the increase and decrease of different diseases and particularly the plague, repr. (1923) by Gregg International Publishers, Westmead, Farnborough, Hants.

Howard, D.L., (1958), John Howard: Prison reformer, London, Johnson.

Keevil, J.J., (1954), 'Leonard Gillespie, M.D., 1758-1842', Bull. Hist. Med., 28; 301-332.

Keith, W., (1844B), 'Practical observations on the lateral operation of lithotomy', Edinb. med. surg. J., 61; 396-417.

Le Cat, C.N., (1766), Parallèle de la taille latérale de Mr. Le Cat avec celle du lithotome caché, Amsterdam, Rey.

Lind, J., (1777), An essay on diseases incidental to Europeans in hot climates, 3rd ed., London, Beckett and De Hondt.

-- -- -- (1808), The same, 6th ed., London, Richardson et al.

Locke, J., (1894), _An essay concerning human understanding_, collated and annotated by A.C. Fraser, Oxford, Clarendon.

Meiklejohn, A.P., (1954), 'The curious obscurity of Dr. James Lind', _J. Hist. Med., 9_; 304-310

Mills, T., (1813), _An essay on the utility of blood-letting_, Dublin, Gilbert and Hodges.

Rolleston, (Sir) H.D., (1933) 'The two Heberdens. William Heberden the Elder (1710-1801). William Heberden the Younger (1767-1845)', _Ann. med. Hist., 5_; 409-427.

Sand. R., (1948), _Vers la médecine sociale_, Paris and Liège, Baillière and Desoer.

Smith, G.M., (1917), _A history of the Bristol Royal Infirmary_, Bristol, Arrowsmith.

Warner, J., (1745), _Cases in surgery_, London, Thomson and Draper.

Wilson, L.G., (1978), 'Fevers and science in early nineteenth century medicine', _J. Hist. Med., 33_; 386-407.