(ARTICLE XVII.)

A CENTURY OF AMERICAN MEDICINE.
1776—1876.

I.

PRACTICAL MEDICINE,

By EDWARD H. CLARKE, M.D., A.A.S., late Professor of Materia Medica in Harvard University.[1]

AND

A HISTORY OF THE DISCOVERY OF MODERN ANÆSTHESIA,

By HENRY J. BIGELOW, M.D., Professor of Surgery in Harvard University.

PRACTICAL MEDICINE.

WHEN Boerhaave, the most accomplished and celebrated physician of the 18th century, died, he left behind him an elegant volume, the title-page of which declared that it contained all the secrets of medicine. On opening the volume every page, except one, was blank. On that one was written, "keep the head cool, the feet warm, and the bowels open." This legacy of Boerhaave to suffering humanity typified, not inaptly or unjustly, the acquirements, not of medical science, but of medical art at the close of the 18th century. Empiricism, authority, and theory ruled the medical practice of the world at that time. The result of therapeutical experience from Hippocrates to Boerhaave was fairly summed up by the latter in the eleven words we have just quoted. To quiet the nervous system, to equalize the circulation, to provide for the normal action of the intestinal canal, and to leave all the rest to the *vis medicatrix naturæ* was sound medical treatment, and it was as far as a sound therapeutics had gone a hundred years ago. This goal had been reached by empiricism. Wise practitioners like Boerhaave, Sydenham, Morgagni, and a few others, were content to restrain their materia medica within these modest limits. The vast majority of practitioners, however, either blindly followed the authority of the past, and bled and dosed by the book, or adopted some strange theory of planetary influence, signatures, animal spirits, or occult force, and treated disease in accordance with whatever theory they chanced to believe in. Medical practice, as a rule, deserved the ridicule of Molière and the satire of Montaigne

In making these statements we do not forget that there had been real progress in many departments of medical science. Anatomy, physiology, surgery, chemistry, and physics had made substantial conquests within their own domains. We do not forget that Harvey had discovered the circulation of the blood; that Haller, one of the greatest names in medicine, had discerned the fact of muscular irritability, and its connection

[1] The author desires to acknowledge his indebtedness to Dr. R. H. Fitz, Assistant Professor of Pathological Anatomy in Harvard University, for invaluable aid in collecting many of the data upon which this essay is founded.

with the nerves; that Albinus had introduced thoroughness and exactness, so far as the means and instruments of observation accessible to him rendered them possible, into anatomical investigation; that Morgagni had founded the science of pathological anatomy, which has since yielded such magnificent results; that Astruc in 1743 had announced the reflex phenomena of the nervous system, which Prochaska before the close of the century more fully developed; that Boerhaave, Sydenham, Mead, Hoffmann, and Stahl had rendered good service to practical medicine; that Franklin and others had brought electricity, magnetism, and galvanism into the domains of science, though their relations to medicine and physiology were not then recognized; and that chemistry had entered upon a career of investigation which it has since followed with extraordinary success. But all these discoveries were in the nature of isolated facts. They were more like islands, surrounded by an unknown ocean, than like parts of a continent, intimately connected with each other and forming portions of a grand and systematic whole.

In spite of these achievements, however, theory, empiricism, and authority ruled the medical world at the close of the 18th and beginning of the 19th century. Let us look at some illustrations of this statement.

Cullen, who flourished during the middle of that century, reasoning from *a priori* considerations, founded his pathology and nosology upon pure theory. He not only did this, but he recognized the fact that he did so and defended himself in doing it. He declared it to be the duty of a philosophical inquirer in medicine to control his observations by his theories, and not his theories by his observations. In like manner he maintained that the medical practitioner should be guided at the bedside, less by the indications of nature than by theoretical considerations. Such was the attitude with regard to the theory and practice of medicine of one of the most philosophical thinkers and learned physicians of that period. He was by far the ablest of the solidists. His views met with general acceptance in England, and excited a great influence upon the medical opinions and practice of this country, and especially of New England. Much of the practice of our fathers and many of their medical opinions may be traced directly to Cullen. He was too often obeyed as a superior. Fortunate was it that the common sense and independence of American physicians often led them to refuse obedience to his authority and to follow the guidance of rational empiricism.

Brown's theory of medicine, which appeared not long after that of Cullen, is another illustration of the speculative tendency of medicine at that time. Brown was a man of less breadth, learning, and power, but of a more practical turn, than Cullen. His practical tendencies led him to base his system chiefly on therapeutics. Its pathology was essentially that of Cullen, and its physiology a misconception of the Hallerian notion of irritability. Its essential error was that it rested not upon facts, but upon assumptions. Its motive was a desire to substitute a stimulating for a lowering method of treatment. Its practical characteristics, however, caused it to spread more rapidly and to exert a more profound influence over the medical opinions and practice of his time than that of his more philosophical contemporary. It rapidly made its way into Germany, France, and Italy. Dr. Rush, of Philadelphia, illustrious as a practitioner, writer, and signer of the Declaration of Independence, who was a disciple of Brown, imported it into America. Introduced under such auspices, it spread rapidly throughout the country and produced a deep and lasting impression upon American medicine.

The speculative tendency to which we have referred, found its most extravagant expression and attained its largest development in the theory which Hahnemann framed near the beginning of the present century. Although, on account of its manifest absurdities, it was rejected by all scientific men, yet, to the philosophic student of the history of medicine for the past hundred years, it is interesting, not only as a curious instance of the aberration of the human intellect, but because, without contributing at all to the progress of medical science, it has modified the therapeutics of the present age by reminding the physician of the limits of his art, and of the great part which nature plays in the cure of disease. Hahnemann ignored all previous medical knowledge. He denied that medicine was a branch of natural science; that any knowledge of anatomy, or physiology, or pathological anatomy, or of diagnosis, or of the investigation of the nature of disease was necessary to the physician; and also denied the existence of any curative power in the human system. Consciously, or unconsciously, abstracting from the mediæval doctrine of signatures its guiding principle, that the like colour cures the like colour, he declared that like cures like, *similia similibus curantur.* To this he added the doctrine of the potency of dilutions, and later admitted certain diseases which he called psora, sycosis, etc., as modifying elements. Symptoms and groups of symptoms were all that were worthy of the attention of the physician, and these were to be treated by potencies in accordance with his fundamental theory. It is unnecessary to allude to the modifications which this theory has undergone at the hands of his disciples. It is sufficient for our purpose to recognize it as a sort of zymotic element in the progress of medical art in this country and Europe, and one which, notwithstanding its activity for a considerable period, is now declining.

Such was the condition of medical science and art at the close of the eighteenth century. A few great minds isolated from each other, slaves to no theory, emancipated from authority and dissatisfied with the results of empiricism, busied themselves with the accumulation of facts whose value they scarcely recognized, but which the future would gladly use. Others, and a larger number, were framers, or disciples, or advocates of some sort of theory, whose foundations were almost purely hypothetical. The vast majority of practitioners, slaves of a routine which authority had sanctioned, were guided solely by empiricism. The outlook was by no means cheerful. It was evident that if medical science was to advance, and a rational therapeutics ever to become possible, some new element, or force, must be introduced. Fortunately this new element, or force appeared. It was introduced by two men, John Hunter of England, and Bichat of France, who may be justly called the founders of modern physiology and pathology.

John Hunter was one of those remarkable men who only appear at rare intervals, and who, if they enter the arena of politics, mould the fate of an empire; if that of theology, change the faith of the age; if that of science, enlarge the boundaries, and add to the sum of human knowledge. He recognized that medicine was one of the natural sciences, more or less intimately connected with all of them, and to be studied as they are by rigid and careful observation. Theory was useless, except so far as it rested upon facts. He regarded a knowledge of the whole organic and inorganic world as necessary to a just comprehension of the structure and functions of man. "He determined to contemplate nature as a vast and united whole, exhibiting, indeed, at different times different appearances, but preserving, amidst every change, a principle of uniform and uninterrupted order, admitting of no deviation, undergoing no disturbance, and presenting no real irregularity, albeit to the common eye, irregularities abound on every side."[1] With such an object before him, he proceeded to collect data of every kind. The Hunterian Museum in London testifies to his indefatigable industry, and to the extent and accuracy of his researches. His method of investigation and of reasoning has served as a model for the age that followed him. His influence upon American medicine was not less potent than upon that of England and the rest of Europe.

At the time Hunter was at work in London, his great contemporary, Bichat, was engaged in those researches in France which have rendered such inestimable services to physiology. Bichat died young, but he lived long enough to show that he was one of the world's greatest minds. "Between Aristotle and Bichat," says Buckle, "I can find no middle man." He and Hunter represent the turning-point in medicine from idealism, speculation, and theory, to accurate and close observation. His great merit lay in recognizing the fact that power depends on structure, and the additional fact that a knowledge of structures can only be obtained by studying the formation of the tissues that compose them. By following the method of Bichat, Agassiz was led to the remarkable discovery of the intimate connection of the tegumentary membrane of fishes with their whole organization; by the same method, Cuvier, Owen and others ascertained the intimate relation of the teeth of an animal to its whole organization. The great discoveries in physiology of the past hundred years, are due to the fidelity with which physiologists have substantially followed the line of investigation marked out by Bichat and Hunter.

The American Revolution, which was the forerunner of political changes of the gravest character in Europe as well as in America, was coincident with this new departure in medicine. American medical science was necessarily an offshoot from that of Europe. While it inherited the traditions, the superstitions, the theories, the authority, and the empirical results of Europe, it also gratefully welcomed the independent thought and sound method of Hunter and Bichat. William Hunter's magnificent work on the gravid uterus (which for accuracy and completeness has never yet been surpassed) appeared in 1774. It was an admirable example of the results of careful investigation, and was a most auspicious illustration of what the new century was to accomplish. From that time to this the progress of medicine in all its branches has been of the most gratifying character. Although it is true, as Tennyson says, that

> "Science moves but slowly, slowly creeping, creeping on from point to point,"[2]

yet, as we look back upon the past hundred years, we find that its march has been one of extraordinary rapidity. During this period, more of nature's great resources have been discovered, and more of her secrets found out than ever before. A thousand doubtful suggestions have ripened into facts. The telegraph, the locomotive, the steamship, the photograph, the spectroscope, and other discoveries more than we can

[1] Buckle's History of Civilization in England, vol. ii. p. 446, Am. ed.
[2] Locksley Hall.

enumerate, testify to the century's scientific activity. In politics this century has witnessed "the separation of America from Great Britain, the formation of the United States, the meeting of the Tiers-États, the revolution, the downfall of the French monarchy, the republic, the rise of Napoleon, the mighty European wars which altered the face of Europe and ended with the 'Hundred Days' and the exile of the Corsican tyrant, the restoration of the Bourbons and their ruin, the Monarchy of July, the Second Republic, the Second Empire, the Third Republic, the Commune, and the humiliation of France by a power which but the day before had been a mere federation of incoherent atoms, the Septennate, the unification of Germany and of Italy."[1] In theology all faiths, from that of Catholic Rome to that of the latest Protestant sect, have been attacked, and they themselves have given unmistakable signs of hesitancy and change. The faith of Christendom has been, and is, crystallizing into new forms, and moving to new issues. It is not an extravagant assertion to say that in all this turmoil, change, and progress, medicine has kept abreast of the other natural sciences, of politics, and of theology, and has made equal conquests over authority, error, and tradition.

If this statement seems extravagant, it is to be recollected that the brilliant discoveries in natural sciences and the arts, the great political changes, and the vacillations of long-established faiths to which we have referred, influence so obviously the fate of nations and the aspects of civilization, that they force themselves prominently upon our attention, while the progress of medicine is silent and unobserved. Yet the progress and changes of the latter are not less real than those of the former, and, perhaps, affect more profoundly than they, the development of civilization and the welfare of the human race.

During the past century, medicine has been enfranchised from superstition, quasi-charlatanism, bald empiricism, and speculation, and has developed into a symmetrical science, affiliated with the other natural sciences, studied by the same methods and the same appliances as they are, and, like them, has been planted upon the solid basis of fact and demonstration; pathological anatomy, starting from the de Sedibus of Morgagni and the labours of Baillie, and illustrated by the later researches of Rokitansky, Cruveilhier, Virchow, Recklinghausen, Cohnheim, and others, has become a fundamental branch of medical science; obstetrics, rescued from the hands of ignorant midwives, has been raised with its allied branch, gynæcology, to its legitimate position as a science; preventive medicine and hygiene, cultivated to an extent previously unknown, have prolonged the average of human life; organic and physiological chemistry have been substantially created, and achieved important and brilliant results; physiology, guided by Blumenbach, Magendie, Legallois, Dumas, Flourens, Johannes Müller, Carpenter, Schiff, Helmholtz, Claude Bernard, Hammond, Dalton, Flint, Weir Mitchell, and others, has grappled with the abstrusest problems of structure and life, and has revealed so much as to make timid people tremble at the audacity of its efforts; the reflex action of the nervous system, first discovered by Astruc and Prochaska, has been shown by the admirable investigations of Sir Charles Bell, Magendie, Marshall Hall, Claude Bernard, Brown-Séquard, and their associates, to be, next to the discovery of the circulation of the blood, the most important addition to physiological knowledge that has yet been made—one that has illustrated

[1] The Nation, August 19, 1875.

and explained the complex and almost inexplicable nature of the nervous system; the inhibitory and vaso-motor system of nerves has, in part, been discovered; the velocity with which sensation, thought, and volition are transmitted along the nerves has been measured and determined; the automatic action of the nervous system, and the position of the ganglia as centres of nervous power, have been demonstrated; the secrets of digestion and assimilation have been disclosed; by a method of exploration, which Auenbrugger and Laennec discovered, and Louis improved, and Skoda has shown to be in harmony with the laws of acoustics, the interior of the chest has been laid open to examination, so that the condition of the lungs and heart can be marked out with an accuracy like that with which the engineer maps out the topography of a mountain; the natural history of some of the gravest diseases has been ascertained, and means of preventing or curtailing them discovered; the ophthalmoscope has revolutionized ophthalmology; the microscope has penetrated the secrets of structure and tissue; the spectroscope has traced the devious wandering of drugs from the stomach to the remotest organs of the body; the sphygmograph has revealed the unseen and delicate movements of the heart and pulse; the æsthesiometer has measured the sensitive power of tissue and nerve; the dynamometer has recorded the force of the muscles; chemical analysis has traced the transformation of food into various forms of force, such as motion, heat, and thought; the materia medica has been made rational and effective by cleansing it from the disgusting animal excreta and filthy compounds that defiled it, from the absurd farragos and useless formulas that superstition or theory had foisted into it, and by adding to it numerous agents that botany and chemistry have discovered; last of all, and most important of all, the grandest discovery of the ages, that which will render this century remarkable for all time, a class of anæsthetic agents has been discovered by which surgery and death even are deprived of half their terrors, and the physician at his will enabled to compel pain to disappear and distress to be quiet.

Such has been the progress, and such are some of the achievements of medical science for the past century. They are enough to justify the enthusiastic regard in which physicians hold their profession, and enough to deserve, as they have received, the gratitude of mankind. After this survey of the general progress of medicine for the past hundred years, we are prepared to estimate more correctly than would otherwise be possible, the part which the United States has taken in aid of this progress and in attaining these results.

In making up our estimate, however, let us remember that a large amount of scientific work cannot justly be expected of the medical profession in a new country. When the nation had acquired its independence, its population extended along a narrow coast-line from what was then known as Massachusetts, now Maine, to Georgia. The inhabitants had the Atlantic Ocean in front of them, and in their rear the unexplored forests, filled with aborigines, that stretched far away towards the Pacific. As a matter of necessity they were obliged to occupy themselves almost exclusively with the task of obtaining a secure existence in a new country. For the first fifty years of the nation's life, the necessities of the present left little leisure for the cultivation of the arts and sciences. The medical profession were compelled by their position to devote themselves almost, if not quite exclusively to the practice of their profession, and to leave scientific investigation and discovery to a later period.

There was no superabundance of educated physicians. If Boerhaave, Cullen, Hunter, or Bichat had found themselves in America at that time, they would have been obliged to take care of the sick, rather than investigate the laws of disease and of life, and the world would not have heard of them as original investigators and natural philosophers.

Over fifty years ago, Sydney Smith, alluding to the slow progress of intellectual development in the first half of our national existence, said in the *Edinburgh Review* :—

"The Americans are a brave, industrious, and acute people, but they have hitherto made no approaches to the heroic, either in their morality or their character. During the thirty or forty years of their independence they have done absolutely nothing for the sciences, for the arts, for literature, or even for the statesmanlike studies of politics and political economy. . . . In the four quarters of the globe, who reads an American book? or goes to an American play? or looks upon an American picture or statue? What does the world yet owe to American physicians or surgeons? What new substances have their chemists discovered, or what old ones have they analyzed? What new constellations have been discovered by the telescopes of Americans? What have they done in mathematics? Who drinks out of American glasses, or eats out of American plates, or wears American coats or gowns, or sleeps in American blankets?"

It must be confessed there was a great deal of truth in his statements at that time. Naturally enough his words rubbed the backs of all loyal Americans the wrong way, and everybody cried out accordingly. At the present time we can read his biting language with equanimity. If the first half century of our national existence did not yield much to science and art, it produced all that could have been justly expected of it; and the last half has produced books, manufactures, discoveries in the arts and sciences of every kind that have gone over the four quarters of the globe. We can now fairly ask, Who does not read an American book? and can point with honest pride to the services which American physicians and surgeons have rendered to the world.

When Sir Humphry Davy was asked what he considered to be his greatest discovery, he replied, Faraday. In like manner we can justly say that American physicians and surgeons are the best contribution of the United States to medical science and art. The work which the physicians of the first age of the republic performed, and the way in which they performed it, proved them to be men of whom the nation need not be ashamed. Men like Rush, Physick, and Chapman, of Philadelphia, Hosack, Watson, Francis, and Mott, of New York, the Jacksons, Warrens, and Bigelows, of Boston, Dudley, of Kentucky, and many others whom our space does not permit us to name, are contributions to science of the best sort. To the example and stimulus of their lives and work, may be justly ascribed, to a very considerable degree, the honourable position, acknowledged zeal, practical judgment, and sound attainments of the American medical profession of the present day. We have already referred to the intimate connection that existed a hundred years ago, and that fortunately still exists, between the medical science of Europe and of this country. The latter is not different from the former. The two are parts of a common whole. Even the war of the revolution scarcely disturbed this connection. An illustration of it is to be found in the fact that in the same year, 1796, in which Jenner vaccinated his first patient, Dr. Waterhouse repeated the operation in Cambridge, Massachusetts, and Dr. James Jackson in the neigbouring city, Boston. Another illustration of the same thing is shown in the education of American physicians. From the era of the revolution until now a large and constantly increasing number of American physicians, after having completed the curriculum of medical study in this country, have resorted to European schools for the completion of their professional preparation. Dr. Samuel Bellingham, who graduated at the first commencement of Harvard College in 1642, afterwards obtained the Doctor's degree at Leyden.[1] The best American education has always consisted in getting the best medical instruction that Europe and America jointly impart. Our medical schools are an honourable contribution to the medical work of the century.

We learn from Dr. Carson's History of the University of Pennsylvania, that the first course of medical lectures given in Philadelphia (and probably in this country) was delivered by Dr. Cadwalader, prior to 1751. The first systematic courses of lectures on medical subjects were given in Philadelphia a little more than one hundred years ago by Drs. Morgan and William Shippen, who were the fathers of medical teaching in America. The degree of Bachelor of Medicine was first conferred in Philadelphia in 1768, and that of Doctor of Medicine in New York in 1770. From these small beginnings sprang the medical colleges, which have ripened into the large institutions of Philadelphia, New York, and Boston, and into numerous other medical schools, too many we fear for the good of the profession and of the country, that are to be found in most of the cities, and connected with many of the colleges of the Union.

These medical schools were not founded by the State, nor are they controlled or supported by it. A few and only a few of them have been scantily endowed by private individuals. Their support depends upon the fees derived from the students that resort to them. They were called into existence by the necessities of the times when they were established, and from one decade to another have been modified in their organization and methods of instruction so as to meet the demands made upon them. They are the natural and necessary growth of circumstances. It would be an interesting and easy matter to trace them from the small beginnings that we have indicated to their present proportions, and to point out the law that has governed their development; but our limits permit only the briefest possible exposition of it.

During the colonial period, and for some time after the establishment of the republic, medical students derived their professional training, not from schools or universities, but from practitioners of greater or less eminence, with whom, to use a technical phrase, they entered their names as apprentices or students. By this arrangement they had the use of the library of their master, whose shelves, if not abundantly supplied, generally held a few books, and whose house usually contained in some closet or nook a few bones of the human frame, or perhaps an entire skeleton. These the student handled, examined, and studied. His opportunities for clinical study consisted in witnessing, and often assisting in the office practice of his master. There he pulled his first tooth, opened his first abscess, performed his first venesection, applied his first blister, administered his first emetic, and there first learned the various manipulations of minor surgery and medicine. After a time his clinical opportunities were enlarged by visiting with his teacher the patients of the latter, and becoming acquainted, not in hospitals but in private houses, with the protean phases of dis-

[1] Historical Address. Dr. J. B. Beck, New York.

ease. His clinical lectures were his master's talk on the cases they had visited as they rode from house to house. After three years spent in this sort of study and practice, the young man was supposed to have acquired enough medical knowledge to enable him to commence the practice of his profession. In proportion as a physician or surgeon became eminent, students who had the means to do so flocked to him, and he became the centre of a medical school. His clinical instructions, instead of being the talks that beguiled the way of a long ride, were changed into formal lectures delivered in his study or in some private room. Those who proved to be the most popular teachers, and who lived in the same city or neighbourhood, associated themselves together for purposes of teaching. Thus were founded the medical schools of Philadelphia and other cities. They did not give, and were not intended to give, a complete medical education, but only to supplement the instruction of private teachers. The courses of lectures were few in number and brief in extent. Students still continued to enter their names, and study for the major part of the year with some medical man in their own neighbourhood, and to attend lectures, as it was called, only three or four months of the year. Gradually a larger demand was made upon the schools; their lecture terms were lengthened; professorships were subdivided; new ones were added; hospitals were utilized for clinical instruction; the schools continued to enlarge their curricula of study, and at length added summer instruction to their winter's work; museums were established; chemical laboratories were formed; microscopical departments created; and all the appliances were attached to schools that are necessary in the investigation of structure, life, and disease. This process of growth has not yet stopped. It is still going vigorously on. One university, Harvard, requires all its medical students to go through a systematic course of training, under its own supervision, by a corps of teachers of its own appointment.

It is evident, from this brief sketch of the medical schools of the United States, that they are different in their organization, and to a considerable extent in their objects, from those of Europe. It is equally evident that the former are gradually approximating the latter, though it is not likely that their organization, methods of instruction, and character will ever be the same. The fact that the European schools are founded and controlled by the State, and are to a large extent responsible to it, and that American schools are independent institutions, self-supporting, and responsible only to public opinion, necessarily impresses a distinctive character upon the medical schools of the two continents. The atmosphere of each is different; each leads a different life; and each will produce a different result. Admitting such to be the case, it does not follow that the medical schools of the United States are necessarily of an inferior character, or that the physicians who graduate from them are imperfectly educated. For the schools, except in the case of Harvard, just referred to, do not pretend to give a complete education, but only to supplement that which the student gets elsewhere. Indeed it may be affirmed that those who, like the apothecary of England and the Secundär Arzt of Germany, are charged with the medical care of the mass of the community in Europe, are not better equipped for the practical work of their profession than their average American contemporary. We do not mean to assert by this that the scientific training of our schools is equal to that of Vienna, Berlin, or Paris. But we do assert that if the necessities and different conditions of Europe and America are impartially compared, we shall find that the American method of medical education yields as good a practical result to the nation as the European method of medical education does to Europe. And we further assert that the flexibility of the American method permits of change, growth, and development, in correspondence with the demands of each succeeding age, more easily and more rapidly than is possible with the conservative organizations of Europe. Hence we are not ashamed to present our medical schools, with all their short-comings and imperfections, as substantial contributions to the practical medicine of the century. And, moreover, we can justly point to graduates of these schools, some of whom have, and others of whom have not, been fortunate enough to add to their American a European education, as in every way the peers of European physicians or surgeons.

It was a noteworthy and fortunate circumstance, that at the time of the establishment of the republic, the medical profession of the new nation contained a large number of intelligent, able, and well-educated physicians. Pre-eminent among these was Dr. Benjamin Rush, of Philadelphia, who devoted himself with enthusiasm to his profession, which he studied first in Philadelphia, and afterwards in Edinburgh. An ardent patriot, a lover of liberty, a friend of Washington, a signer of the Declaration of Independence, he was not only eminent as a physician, but distinguished as a philosopher and a scholar. Holding a high social position in a community, noted alike for its love of the arts and sciences, and for the graces of social life, he contributed largely to raise the profession of medicine in the estimation of the community in which he lived, and of the whole country. During the Revolutionary war he rendered essential service to the army by a variety of professional labours, and after its close remained permanently in Philadelphia. Notwithstanding the demands of a large practice, he found or made time for the investigation of scientific questions, and for the publication of the results of his inquiries. His treatise on Diseases of the Mind, regarded as a work full of instruction, and of great originality by Prof. Brown, of Edinburgh, contains many practical and original observations, and was a valuable contribution to psychological medicine. It is not yet forgotten. Dr. Tuke, in his late monograph upon the Influence of the Mind upon the Body, quotes from it approvingly. Speaking of another of the essays of Dr. Rush, Dr. Tuke says: "Rush wrote an able essay (and when are his essays not able?) on Hydrophobia, in which he assigns an important *rôle* to the influence of fear, and an involuntary association of ideas."[1] Few are the observers and writers whose labours are remembered and words quoted for a hundred years after they have ceased from their work. The observations of Dr. Rush on Yellow Fever were extensive and important. They produced an impression on both sides of the Atlantic. Although their pathology was erroneous and their therapeutics atrocious, they were a substantial contribution to medical science by the stimulus which they gave to the careful and exact study of disease. When Rush began his lectures as Professor of the Institutes and Practice of Medicine in the University of Pennsylvania, diseases were divided, according to the nosology of Cullen, into orders, classes, genera, and species, containing about thirteen hundred and eighty-seven diseases, for each of which there was supposed to be an appropriate treat-

[1] Illustrations of the Influence of the Mind upon the Body in Health and Disease, by Daniel Hack Tuke, M.D., Am. ed., p. 203.

ment. Rush rejected these arbitrary divisions. He paid little regard to the name of a disease, and founded his treatment on its nature and on the condition of the system. By this course he reduced his materia medica to a few active medicines, and so prepared the way for the simplification of remedies that has been accomplished since his day.[1]

Dr. Philip Syng Physick, a friend of Dr. Rush, and a favourite pupil of that great master, John Hunter, was one of the most accomplished and brilliant of American surgeons. He was not a prolific writer, but he found time, however, to study the character of yellow fever, and to publish the result of his observations, which were founded on post-mortem examinations. His researches into the character of this disease, together with those of Rush, La Roche, Alonzo Clark, Jones, and others too numerous to mention, form a library of yellow fever literature which will be more fully noticed in a subsequent essay, and which later investigators into its nature cannot afford to neglect.

While Dr. Rush was pursuing his investigations in Philadelphia, two men in Boston were labouring with equal zeal and earnestness in the cause of medical science. One of them, Dr. John C. Warren, devoted himself chiefly to surgery, and his work in that direction will be noticed in the surgical part of these memoirs. Apart from surgery he rendered a service to practical medicine that should not be forgotten. By his paper upon diseases of the heart, he first brought distinctly to the notice of the profession in this country that class of affections which Corvisart described in his remarkable treatise. Another and more important service was the foundation and endowment of the anatomical museum of the medical department of Harvard University. Under his care and that of Dr. J. B. S. Jackson, who has worked in it and for it for more than a quarter of a century with rare intelligence and devotion, it has attained a completeness and excellence that few similar collections possess, and which render it one of the best contributions to the study and illustration of practical medicine in the country. In like manner, the large museums containing anatomical and pathological specimens, that have been collected in Philadelphia and New York and other medical centres of the United States, are invaluable contributions to the same science.

Dr. James Jackson, the second labourer to whom we referred, was known exclusively as a physician. He was one of the founders of the Massachusetts General Hospital, and, like Dr. Warren, was connected with the medical school of Harvard College at its commencement. He was a large practitioner, an acute and close observer of nature, but not a prolific writer. In him that indefinable but substantial something, called common sense, was applied with singular success to the practice of his profession, to his clinical teachings at the Massachusetts General Hospital, and to his didactic lectures at the medical school. His report on typhoid fever, and Dr. Hale's paper on the same disease, which may be found in the Communications of the Massachusetts Medical Society, were based on their own observations. The results at which they arrived were substantially those of Louis.

Dr. Jackson's Letters to a Young Physician are models of sensible advice to a practitioner whether young or old, and whether living on one side of the Atlantic or the other. He never indulged in heroic practice, or in therapeutic expedients for which he could not give a reason. He believed

[1] *Vide* Thatcher's Medical Biography.

in the conservation of nature's forces. To a large extent the medical profession of New England was moulded by his teachings and example. The impression which he made is not yet effaced. Such an influence, though difficult to describe or estimate justly, is nevertheless a real contribution to practical medicine. Dr. Nathan Smith, a contemporary of Rush, Warren, and Jackson, deserves also to be remembered. He was a sound observer, who, having enfranchised himself from the bonds of authority, delighted to study nature with his own eyes, and was not afraid to follow where she led. His essay on Typhus Fever, published in 1824, had the merit of pointing out the self-limited nature of that disease, and of showing from his own experience the futility of attempting to abort it, or to treat it with violent remedies. "I have never been satisfied," he says, "that I have cut short a single case of typhus that I knew to be such. Typhus has a natural termination like other diseases which arise from specific causes." He mentions with approbation the successful treatment of a physician who gave only milk and water to his patients in this complaint. "All that is required," is Dr. Smith's therapeutical conclusion, "are simple diluent drinks, a very small quantity of farinaceous food, and avoidance of all causes of irritation." This result, which he reached by his own observations more than fifty years ago, is the same as that which has lately been loudly proclaimed in England and Germany. What Dr. Smith calls typhus was undoubtedly typhoid fever. At the time he wrote, typhus and typhoid fever were confounded together as different forms of the same disease. It is worthy of remark that Dr. Smith recognized the fact, now acknowledged, that typhoid fever arises from a specific cause, and that one attack of it prevents a subsequent one.

Typhoid fever prevails to such an extent in the United States, that our physicians enjoy ample opportunities for the study of it. Among those who have investigated it, none have done so with greater acuteness and ability than Dr. Gerhard, of Philadelphia, or have discriminated with greater clearness than he the essential differences between typhus and typhoid. He was the first, or among the first, to point out these differences with scientific accuracy. He says himself:—

> "The advantages which I enjoyed of carefully studying the pathological anatomy and the symptoms of the two fevers, enabled me to place the question of their identity (typhus and typhoid), upon more settled scientific points, than had yet been done. . . . . . It is true that after the observations, which formed the basis of the paper which I published in 1837, were collected, but before their publication, Dr. Lombard, of Geneva, who was of course familiar with typhoid fever, stated in the *Dublin Journal* that the two diseases were different; the same remark I remember to have heard Prof. Andral make on the authority of Dr. Alison, and it was obvious to many persons that the description of Dr. Louis did not apply to the British typhus, but the points of resemblance and of difference were not settled, that is, they were not scientifically demonstrated."[1]

The merit of having decided this important question, of having demonstrated the essential difference between typhus and typhoid fever, belongs chiefly, if not wholly, to Dr. Gerhard, and so far redounds to the honour of American Medicine. Previous to his paper, which was published in the *American Journal of the Medical Sciences*, the evidence as to the essential distinction between the two fevers was mainly speculative, or conjec-

[1] A System of Clinical Medicine. By R. J. Graves and W. W. Gerhard, 1848, p. 735.

tural ;[1] he made it logical, clear, and unequivocal. It is only just in this connection to refer to the papers of Dr. J. Baxter Upham, of Boston, which, published many years after the appearance of Dr. Gerhard's memoir, and founded on the careful personal investigation of Dr. Upham, confirmed the results of Dr. Gerhard, and added to our knowledge of the history of typhus. The observations of Dr. Thomas Stewardson on remitting fever form a valuable addition to our knowledge of that disease. The paper[2] which embodies his views was founded on the clinical study and post-mortem appearances of the cases which came under his notice in the Pennsylvania Hospital. In this memoir he calls attention to changes in the liver, which were present in every case, and were of a character not met with in other diseases. These he regarded as the anatomical characteristic, though not the primary seat of the disease.

Yellow fever has several times within the past century ravaged the Atlantic and the Gulf coast, so that our physicians have had unfortunately ample opportunities of studying the disease. Without detracting from the valuable labours of many other observers, it may be stated that to Dr. Deveze, then resident at Philadelphia, we are indebted for being foremost in asserting and maintaining the non-contagiousness of yellow fever; and to Dr. Alonzo Clark, of New York, for showing that the pathological change, so constantly observed, in the liver, is due to acute fatty degeneration.

Dr. S. H. Dickson, of South Carolina, had the opportunity of observing an epidemic of dengue, more than twenty-five years ago, of which he gave a highly interesting account. He considered the disease to be the same as that which prevailed at the South in 1828, and as the break-bone fever, described by Rush in 1778. The memoir is an instructive and valuable one.[3]

Dr. Gerhard's labours in practical medicine have contributed materially to its progress, and have given him a deservedly high position among American medical scientists. Though our limits forbid an enumeration of all of his contributions, we cannot refrain from calling attention to his observations upon tubercular meningitis. Together with M. Rufz, he was the first to point out clearly the essential connection of hydrocephalus with tubercles of the pia mater, and the dependence of the former upon the latter.[4] Previous to his investigations, the notions of medical men with regard to the presence and cause of water within the cranium, were confused, theoretical, and consequently inaccurate. By many acute hydrocephalus was regarded as a cause, not as an effect—as an independent disease, not as a result. Dr. Gerhard cleared away the obscurity, supplied the missing links, and showed that tubercular disease of the meninges of the brain is a distinct malady which leads to the effusion of liquid there, as certainly as tubercle of the lung leads to purulent expectoration.

From the time of Hippocrates until recently the treatment of effusion into the pleural cavity has been among the *opprobria medicorum*. With the hope of promoting the absorption of the fluid, the unfortunate subjects of it were sometimes bled, *coup sur coup*, sometimes salivated with heroic persistence, often blistered with indefatigable zeal, generally plied

[1] *Vide* Am. Journal of Med. Sciences, vol. xix. p. 289, Feb. 1837; also, Wood's Theory and Practice of Med., vol. i. p. 373.

[2] Am. Journ. Med. Sci., 1841 and 1842.

[3] Charleston Med. Journal, 1850.

[4] Am. Journ. Med. Sci., xiii. p. 313; Wood's Practice vol. ii. p. 675.

with diuretics, and by cautious practitioners treated on the expectant method, and all with the result of not interrupting the progress of the effusion. In many, perhaps in the majority of cases, the powers of nature were equal to the demand made upon her and the liquid absorbed. In a large number of cases, however, this fortunate result did not occur, and the effusion went on increasing until the patient was killed by mechanical pressure, or by the development of some disease, like tubercle or other trouble that the pressure induced. More than a quarter of a century ago Dr. Henry I. Bowditch, of Boston, whose life has been devoted to the study of diseases of the chest, was impressed with the notion that it would be possible and safe to relieve this class of cases by drawing the fluid off. He made several attempts to do this by means of incisions into the pleural cavity. The results were not satisfactory. While Dr. Bowditch was busy with these efforts, Dr. Morrill Wyman, of Cambridge, who, unaware of Dr. Bowditch's views, entertained similar notions, successfully tapped a patient, by means of an exploring trocar and canula with suction-pump attached. In 1850, Dr. Bowditch, aided by Dr. Wyman, repeated the operation with equal success upon another patient, using the same apparatus. "That apparatus," says Dr. Bowditch, "I have modified somewhat, so as to make it, I think, more convenient; but the principle of the instrument remains as suggested by Dr. Wyman."[1]

From that time to the present, Dr. Bowditch has used his modification of Dr. Wyman's instrument for this operation. In his opinion it operates more rapidly than Dieulafoy's aspirator, and quite as harmlessly and easily for the patient. He has operated upon patients of all ages and both sexes, and with almost every species of complication, and has never seen any permanent evil results. His own statement is, that he has very rarely seen anything following the operation, but ease to the patient. During the last twenty-five years he has operated 325 times upon 204 persons. In a large number of these cases relief was not only afforded to the sufferer, but imminent death was prevented. This result is a demonstration not only of the propriety but of the necessity of performing thoracentesis in appropriate cases. Dr. Bowditch considers the following to be the indications for the operation:—

"1st. To save life when immediately threatened.
"2d. To prolong life, even when complicated with severe disease.
"3d. To shorten latent pleurisy.
"4th. To give temporary relief merely in absolutely hopeless cases.
"5th. To relieve cases of common pleurisy which do not easily yield to remedies after a few weeks of treatment."[2]

Thoracentesis is now regarded both in Europe and America as a legitimate, safe, and necessary procedure, when withdrawal of fluid from the chest is indicated. It has not won this position, however, without difficulty. It has had to run the gauntlet of opposition and of severe criticism from physicians and surgeons of great experience and reputation on both sides of the Atlantic. Trousseau advocated it; Valleix condemned it. English and American surgeons denounced it as unsafe and needless. That it has gradually made its way to its present acknowledged position, is largely due not only to the brilliant results of Dr. Bowditch's personal

[1] Thoracentesis, a paper read before the New York Academy of Medicine, April, 1870, p. 6, by H. I. Bowditch, M.D.

[2] Thoracentesis, ut supra, p. 6.

experience, but to the earnestness with which he has pressed by his pen the importance of it upon the profession, and the clearness with which he has pointed out the proper method of performing it.[1]

The principle of M. Dieulafoy's aspirator, an instrument too well known to need description, and lately introduced to the notice of the profession, is the same as that of Bowditch's exploring trocar and canula with suction-pump attached. The French physician's application of "aspiration" to all parts of the human body, is a brilliant generalization of the American physician's operation of thoracentesis. It is much to be regretted that M. Dieulafoy, in his admirable monograph on aspiration, neglected to make the slightest allusion to Dr. Bowditch's previous and persistent labours. Such a neglect on the part of M. Dieulafoy must have arisen either from an ignorance of Dr. Bowditch's previous investigations, or from a desire to claim and wear the laurels that another had won.

Consumption is recognized as the most terrible scourge of temperate climates. We are so familiar wtih its presence that we have ceased to be alarmed at its existence among us, although it causes from an eighth to a fifth of the total number of deaths in New England, and a very large proportion of all the deaths throughout the United States and Europe. The ablest intellects of the profession have occupied themselves, and are still occupied with the study of this disease, hoping to unravel completely its natural history and pathology, and to learn how to check its ravages and ameliorate the suffering it produces. Among these labourers Dr Bowditch holds an honoured place. His investigations led him to the conclusion that soil moisture is a large factor in the production and development of consumption. In May, 1862, he delivered an address before the Massachusetts Medical Society upon this subject.[2] In this address he was the first to announce what is now generally received as an acknowledged fact—that consumption may be produced in a family by residence on a damp soil. His language in the address referred to is as follows :—

> "First. A residence on or near a damp soil, whether that dampness be inherent in the soil itself, or caused by percolation from adjacent ponds, rivers, meadows, marshes, or springy soils, is one of the primal causes of consumption in Massachusetts, probably in New England, and possibly in other portions of the globe.
>
> "Second. Consumption can be checked in its career, and possibly, nay probably, prevented in some instances, by attention to this law."

The estimation in which these conclusions with regard to the influence of soil-moisture as a cause of phthisis, and of Dr. Bowditch's part in the investigation of it, may be inferred from the following statement: In 1867, Mr. Simon, of England, medical officer of the Privy Council, presented the results of Dr. Buchanan's investigation into the death-rate of towns in which soil-drainage had been introduced. The latter had ascertained that moist towns, in which this had been done, had a less

[1] Those who are desirous of consulting Dr. Bowditch's papers on the subject, are referred to the American Journal of Medical Sciences, April, 1852, and Jan. 1863; American Medical Monthly, Jan. 1853, New York; Boston Medical and Surgical Journal, May 25, 1857. Thoracentesis and its General Results, address before the New York Academy of Medicine, April, 1870.

[2] Medical Communications of the Massachusetts Medical Society, vol. x. No. 2, 1862.

death-rate from consumption after doing it than before. In consequence of this result, Dr. Buchanan was ordered to investigate thoroughly the subject. He made "an elaborate examination of the distribution of phthisis as compared with variations of the soil in the three southeastern counties of England." Mr. Simon concludes from this investigation, confirmed, as he states, by Dr. Bowditch's previous researches in America, "*that dampness of the soil is an important cause of phthisis to the population living upon that soil*" (italics as in the original). Mr. Simon adds, "this conclusion must henceforth stand among those scientific certainties on which the practice of preventive medicine has to rest."[1]

While these pages were passing through the press, a work on phthisis[2] appeared from the pen of Dr. Austin Flint, of New York, which will be gladly welcomed by the profession of America and of Europe. It is based on a careful record of six hundred and seventy cases of phthisis, which are grouped and analyzed with reference to the practical deductions that may be legitimately drawn from them. The book is written from a clinical stand-point. So far as practicable, Dr. Flint follows the numerical method of investigation. For the most part the cases, which he reports, are chronic in their character, and belong to a class remarkable for the uniform character of the lesions, and of the symptomatic events and laws which are developed by their clinical history.

In addition to this recent work on phthisis, and to other labours, which we have elsewhere referred to, practical medicine is indebted to Dr. Flint for a great deal of valuable work. His reports on continued fever, and articles in the *American Journal of the Medical Sciences*, on Tuberculosis, Heart Sounds, Pneumonia, Chronic Pleurisy, have all of them deserved and received the careful consideration of the profession.

The progress of medicine, like that of all science, depends first upon the collection of facts, and afterwards upon a correct interpretation of them. Whoever recognizes a fact, however insignificant it may seem to him, and reports the discovery, makes a valuable contribution to science. The chief difficulty in the way of collecting accurate data, especially in medicine, is that few observers are gifted with the power of knowing a fact when they see it. "The hardest thing in the world, sir, is to get possession of a fact," said Dr. Johnson. Most observers report what they think to be, not what is. Whoever contrives a new instrument that increases the accuracy of physical exploration, whoever discovers a new method of examination, or modifies an old one, by which some secret of the organization is disclosed, whoever demonstrates the correct explanation of any phenomenon of the human system, whether it be the crackling of bubbles in the chest or the mechanism of thought in the brain, whoever traces back any symptom to its cause, so as to make the former the pathognomonic sign of the latter, or whoever in any way, by microscope, analysis, scalpel, or experiment, reveals anything that pertains to the structure or functions of man, in health or disease, contributes to the progress of practical medicine. It would be pleasant, if it were possible, to collect all the contributions of this sort, small as well as large, that have been made by Americans during the past hundred years to medical science and art. While the parentage of many of these contributions is

[1] Tenth Report of the Medical Officer of the Privy Council, 1868, p. 16.

[2] Phthisis; its Morbid Anatomy, Etiology, etc. etc. By Austin Flint, M.D. Phila., 1875, pp. 441.

well known and recognized, there are many others now incorporated into the body of science that cannot be traced to their discoverers; their lineage is unknown. The following pages record some of these contributions, in addition to what we have already described. We are sorry that we cannot make the record more complete than it is.

Dr. James Jackson, Jr., of Boston, whose premature death was not only a great personal bereavement to his friends, but a great loss to the science whose devoted student and servant he was, while pursuing his studies in Paris communicated in 1833 a paper to the Société Médicale d'Observation on the subject of a prolonged expiratory sound as an early and prominent feature of bronchial respiration, and one which frequently constitutes an important physical sign of the first stage of phthisis.[1] The accuracy of this observation has been demonstrated by many other observers since the appearance of his paper. At the present time a prolonged expiration, when heard in the clavicular region of the chest, is acknowledged as one of the earliest and most valuable signs to warn the practitioner of the insidious approach of disease. Probably few have ever heard even of the name of the young physician whose quick ear first caught the sound, and whose careful observation connected it with the condition that produced it.

When Laennec made his great discovery, which has revolutionized the study and indirectly the therapeutics of affections of the chest, a variety of stethoscopes were devised to conduct the sounds of that region to the ear of the observer. Most, if not all, of these instruments were clumsy and poorly adapted to the object in view. They gradually fell into disuse. Direct auscultation, by laying the ear directly on the chest, or with a single intervening bit of cloth, yielded a better result than the stiff, awkward wooden tube which Laennec employed, and which Dr. Holmes has so cleverly satirized. Dr. C. W. Pennock, of Philadelphia, while making his well-known investigations with regard to the heart and its diseases, discarded the stiff wooden instrument and introduced a flexible tube stethoscope.[2] Its advantages were obvious. It did not transmit the impulse, but only the sounds of the heart and chest, to the ear of the examiner. While using this instrument the physician was able to explore the sounds of the heart and chest undisturbed by any muscular movement. Dr. Cammann, of New York, improved upon Pennock's flexible stethoscope by adopting with some modifications the double binaural stethoscope of Dr. Arthur Leared, of London. This instrument conducts the sounds of the chest to the ear of the asucultator more clearly than any other, and does not conduct the impulse. It is the most serviceable stethoscope that has yet been devised.[3]

Dr. Alfred Stillé,[4] of Philadelphia, was among the first, if not the first, to call attention in print to a condition of the heart observed among sol-

[1] A Practical Treatise on the Physical Exploration of the Chest, etc. By Austin Flint, M.D., second edition. Philadelphia, page 191.

[2] Wood's Theory and Practice of Medicine, vol. i. p. 209.

[3] Dr. Arthur Leared, of London, exhibited at the great Exhibition in 1851 a double binaural stethoscope which he was the first to devise. Dr. Camman evidently got the idea of his instrument from that of the London physician, from which it differs in a few particulars.

[4] Address before the Philadelphia County Medical Society. Delivered Feb. 11, 1863, by Alfred Stillé, M.D.

diers as the result of prolonged and violent exertion, and now known as irritable heart; and Dr. Henry Hartshorne,[1] in the same year, more fully described the affection in a paper which he read before the College of Physicians of Philadelphia.

In a communication forwarded in December, 1862, to the Surgeon-General's Office, Dr. J. M. Da Costa[2] called attention to this same cardiac malady to which he gave the name of irritable heart, and his Medical Diagnosis, published in April, 1863, contains an outline sketch of the disorder. A few years later[3] he traced the connection of irritable heart with organic disease, and illustrated it with cases; in this paper, also, the inquiry took a wider scope and showed how exertion and strain could result in endocarditis and subsequent valvular disease, and in hypertrophy. In 1871 he published a careful and elaborate clinical study of irritable heart[4] based on upwards of 300 cases, in which he showed that irritable heart resulted from exhausting diseases, such as fevers and diarrhœa, and from strains and blows, as well as from muscular exhaustion, and further traced the connection between functional heart disorder and organic change. In it was also made a valuable contribution to a more exact knowledge of the action of remedies on the heart.[5] In 1874 he[6] called attention to the same affection occurring with the same sequelæ in civil practice. In this brief monograph the effect of cardiac strain upon the muscular walls, valvular apparatus, and great vessels of the heart, is clearly stated, as well as the general symptoms and local signs. These papers give an excellent account of the disease they describe, and make a valuable and original contribution to practical medicine.

In a recent number of the *American Journal of the Medical Sciences*,[7] Dr. Da Costa has called attention to the advantage of forced respiration on the part of the patient as an aid to the physician in diagnosticating diseases of the chest. We can ourselves bear testimony to the accuracy of his statement. Forced respiration is of especial service in doubtful cases, particularly when it is important, as it often is, to make out a differential diagnosis between bronchitis and phthisis. It renders other services than this, for an account of which the reader is referred to the original article.

Dr. Da Costa has prepared, during the past twenty years, a number of papers, based upon his own observations of disease, which are valuable contributions to practical medicine. We regret that we are unable to do more than allude to some of them. In addition to those which are mentioned elsewhere, he published, in 1855, a memoir[8] on the pathological anatomy of pneumonia. In 1859 he published the results of some observations[9] "On the occurrence of a blowing sound in the pulmonary artery, associated with affections of the lung; on the sounds of the artery in health, and the effect on them and on the heart of the act of

[1] Am. Journ. Med. Sciences, July, 1864.
[2] Ibid., January, 1871.
[3] Sanitary Commission Memoirs, Medical Volume, New York, 1867.
[4] Am. Journ. Med. Sciences, January, 1872.
[5] These papers of Dr. Da Costa, which were based upon his army experience, have lately received a German translation.
[6] On Strain and Over-action of the Heart. Toner Lecture, No. 3. Washington, 1874.
[7] July, 1875.
[8] Am. Journ. Med. Sciences, Oct. 1855.
[9] Ibid., Oct. 1859.

respiration." In 1866 he published a paper[1] on typhus fever, based upon the cases under his charge, and of course written from a clinical standpoint. In 1869 he gave to the profession a memoir on Functional Disorders of the Heart;[2] in which he attempted to show the real value and meaning of a cardiac murmur. In 1871 he recorded his observations on Membranous Enteritis,[3] which, like most of his other observations, were based on a careful clinical study of the disease, and present a complete account of it.

The importance of distinguishing the variations of pitch elicited by percussion is now universally recognized as an aid in ascertaining the condition of the organs in the chest. There are cases in which these variations afford the earliest clew to commencing disease; and sometimes when the signs are nearly evenly balanced it throws the vote which decides the verdict. The profession are indebted to Dr. Austin Flint, of New York, for calling their attention to this subject, at least in this country. Dr. Flint's statement of the value of variations of pitch in exploration of the chest, and the practical inferences from them which his acute observation and large experience suggested, and the investigations which led him to his conclusions in this matter, may be found in an essay which received in 1852 the prize of the American Medical Association. The combination of percussion and auscultation, or auscultatory percussion, as described and employed by Dr. Alonzo Clark, of New York, is undoubtedly well adapted to determine with ease and accuracy the boundaries of the heart.[4] This sort of cardiac examination cannot be made accurately without the aid of Cammann's stethoscope. When we recollect the method by which Piorry used to map out the boundaries of the heart, a task which we have often seen the distinguished French auscultator undertake in the wards of his own hospital five-and-twenty years ago, we are forcibly struck with the advance which has been made during the past quarter of a century in the physical examination of the chest.

There are few practitioners who have not sometimes been puzzled to distinguish between the solidification of pneumonia and the effusion of pleurisy. The differential diagnosis between these two conditions is sometimes a matter of great delicacy and difficulty. Here we are again indebted to Dr. Flint, of New York, for enabling us to solve the difficulty with comparative ease. He showed that by mapping out the lobar dulness which exists in pneumonia, the inflammatory condition of the lung could be discriminated from the effusion in which no such limited dulness exists. Though it does not fall within the scope of this paper to touch at all upon the subject of American medical literature, we cannot refrain from referring in this connection to the masterly digests of the vast number of memoirs, monographs, and the like, upon the subject of pneumonia, and perhaps we should add yellow fever, which have appeared from the pens of Dr. La Roche and Dr. Flint. They are substantial contributions to practical medicine. The mechanism by which the crepitant râle of pneumonia is produced is not yet perfectly made out. The explanation of it, given by Dr. E. Carr, of Canandaigua, N. Y., has been accepted by pathologists as probable, if not fully demonstrated, and deserves mention. Dr. Carr suggests that the crepitant sound is produced by air

[1] Am. Journ. Med. Sciences, Jan. 1866. [2] Ibid., July, 1869.
[3] Ibid., Oct. 1871.
[4] New York Medical Journal, July, 1840. Flint, on Diseases of the Heart, Second edition, p. 43, 1870.

rushing into and distending the bronchial vesicles which had been previously glued together by tenacious mucus. For a full exposition of his views our readers are referred to his original article.[1]

Croup, a name dreaded alike by physicians and mothers, was for centuries the generic term of several inflammatory affections of the throat that were confounded together. Gradually these different affections have been discriminated from each other and have received different names. The term croup, or as some prefer to call it, membranous croup—the diphtherite of Bretonneau—is now restricted to an inflammation of the upper part of the air passages attended with the formation of a membrane. The membrane is recognized as an essential part of the disease. Richard Bayley, Surgeon of New York, recognized the distinctive characteristics of this affection as long ago as 1781. In a letter to William Thornton, M.D., of London, which afterwards appeared in the *New York Medical Repository*,[2] he points out the difference between angina trachealis and putrid sore throat, or, in modern terms, between membranous croup and diphtheria. His observations were founded upon autopsies of the two diseases, and therefore rested on an anatomical basis. It is unfortunate that his views did not attract more attention, and make a more permanent impression than they did. They were corroborated by Dr. Peter Middleton, of New York, who satisfied himself that croup "is totally distinct from the malignant sore throat; it is not of itself of a nature malignant or infectious as the putrid sore throat may often be." These views were put forth nearly ninety years ago, and have been confirmed only within a comparatively recent period. Among those who have studied the natural history of this disease, Dr. John Ware deserves honourable mention. His memoir on the history and diagnosis of croup contributed materially to the accuracy of our knowledge of it and to its correct treatment. His paper was based upon a careful study of the cases which came under his own observation. He was satisfied that membranous croup and inflammatory croup were not different stages of the same disease, but distinct maladies, differing from each other in their character and prognosis, and requiring a different treatment. His reasons for believing in the essential difference of the two diseases are stated in the following moderate language: "The very great preponderance of fatal results in the membranous croup and a similar preponderance of recoveries in the inflammatory, and the evidence which exists that in a few cases of recovery from the former the membrane has been found, and in the few cases on record of death from the latter that a membrane has not been found, afford strong reason for believing that the diseases are essentially different."[3] Dr. Ware regarded the membrane in membranous croup more as a result of a peculiar kind of inflammation than as the essential part of the disease. As to the prognosis in the two forms, he inferred from his observations "that the only form of croup, attended with any considerable danger to life, is that distinguished by the presence of a false membrane in the air passages." To this he added the following remark: "The existence of this membrane in the air passages is in a very large proportion of instances indicated by the existence of a similar membrane in the visible parts of the throat." As far as treatment is concerned, he was satisfied that inflammatory croup gets well sooner by the aid of mild and soothing appli-

[1] American Journal of Medical Sciences, New Series, vol. iv. p. 360, 1842.
[2] New York Medical Repository, vols. xii. and xiv., 1809 and 1811.
[3] Contributions to the History and Diagnosis of Croup, by John Ware, 1842.

cations, such as emollient gargles, light diet, opiates, and occasional poulticing externally, than by heroic treatment, such as opening the jugular vein, free leeching, antimonial and other emetics and violent cathartics, with which this disease has been so frequently and unfortunately combated. In like manner it was a fair induction from his cases that membranous croup is more likely to be aggravated than relieved by violent applications. He found that the inhalation of warm vapour, an even temperature, and enforced quiet more frequently led to the resolution of the inflammation and consequent detachment and expulsion of the membrane than the lancet or caustic or other extreme measures. When we consider that these observations were made, and the record of them and deductions from them published more than thirty years ago, and observe how nearly they represent our present knowledge of the history, prognosis, and treatment of croup, we cannot resist the conclusion that Dr. Ware was largely in advance of his time in comprehending the nature of croupal affections, and that his observations on these affections were a valuable contribution to practical medicine. The minute studies of recent German investigators in this direction have substantially confirmed Dr. Ware's earlier views.

Abernethy was in the habit of urging with great earnestness the importance, especially so far as treatment is concerned, of the constitutional origin of local diseases. The late Dr. Horace Green, of New York (who achieved such a large notoriety as a specialist in diseases of the throat), insisted with equal emphasis upon the local origin of constitutional diseases. His treatise on diseases of the air passages might be regarded without injustice as a defence of such a thesis. Its real object was, of course, to present and defend Dr. Green's peculiar views. Although his pathology and therapeutics were severely, and to a large extent, not unfairly criticized, both in America and Europe, yet it is not to be denied that his observations contributed to advance our knowledge of the throat and its maladies. They not only stimulated inquiry, but showed how far local applications could be carried into those regions, and to what extent the tissues would bear cutting, slashing, and burning. Dr. Green was a bold and skilled operator, an heroic therapeutist, and was sometimes charged with magnifying his office. These qualities enabled him to do what others would have shrunk from. We must remember that the laryngoscope, which has revolutionized our notions of the throat as much as Laennec's discovery did our notions of the chest, was not known when Dr. Green was studying and treating the air passages. Since the laryngoscopic mirror has rendered visible parts of the throat that were previously invisible except after dissection on the dead body, and has rendered possible a variety of local applications and operations that would not previously have been ventured upon, it has been ascertained that Dr. Green's attempts to reach and act upon the glottis, epiglottis, larynx, even down to and below the bifurcation of the bronchi, were legitimate. Czermak, Mackenzie, and their disciples have carried the local treatment of the throat and air passages much further than Dr. Green ever attempted, but he deserves the credit of having opened the way into a region which later physicians with better appliances and ingeniously constructed instruments have explored with such success.

Autumnal catarrh, commonly called hay fever, from some supposed, but improbable and unproved connection with hay as its cause, has been recognized as a distinct disease only within a comparatively short period.

It has undoubtedly been one of the ills flesh is heir to from time immemorial, but has been confounded with ordinary catarrh, asthma, and the like. Gradually its distinctive features have been made out. Since it has obtained the status of a distinct disease, it is surprising how many people, both in this country and in Europe, have been found to be sufferers from it. Few or none die from it, and the consequent inability to obtain post-mortem information makes our knowledge of its pathology more or less conjectural. Dr. Morrill Wyman, of Cambridge, whose interest in the disease may possibly be heightened by the fact that he is personally one of its victims, has contributed more than any other observer that we are acquainted with, to a correct knowledge of its natural history and treatment. His treatise on autumnal catarrh[1] is a classical one of its kind. He has pointed out the distinctive characters which separate it from other catarrhs, its limited duration, its remissions, intermissions, and whimsical variations, its intractability to the action of drugs, the fact that certain regions are free from it and that migration to these regions relieves the sufferer almost immediately. He has made several experiments with regard to its etiology, which, while they do not demonstrate its cause, indicate the direction of study which will probably lead to the discovery of its cause, and has shown that "the disease has more of a general than local character, and falls especially upon the nervous system." By long and careful observation he has ascertained that the regions of this country which are free from the disease, the places of refuge for the catarrhly afflicted, are the northern side of the White Mountains in New Hampshire; Mount Mansfield, in Vermont, and its immediate neighbourhood; the Adirondacks, in New York; the Ohio and Pennsylvania plateau, including the high range of land in New York from the Catskill Mountains to the western border of the State; the island of Mackinaw; the northern side of the great lakes in Canada; tracts of land beyond the Mississippi, at St. Paul and in Minnesota; the Alleghany Mountains at Oakland, and other elevated points of the same region; the high lands of the interior of Maine; and the whole sea coast from St. John's quite round to Labrador. It thus appears from Dr. Wyman's observations that the regions of safety for the afflicted are by no means small; and that in this disease climate most effectually supplements the action of drugs.

It is undeniable that during the past century, and particularly during the past fifty years, medical science has made great and satisfactory progress in acquiring an intimate and accurate knowledge of the natural history, pathology, and appropriate treatment of diseases of the chest and air passages. When we consider the contributions to this progress made by the American physicians, Bowditch, Gerhard, Pennock, Da Costa, Alonzo Clark, Austin Flint, Green, Ware, Wyman, and others whose labours we have so imperfectly described, and by other physicians whose contributions we have not time to mention, we can point with honest pride to the honourable record of service rendered to the progress of this department of medical science by America.

In our allusion to Dr. Nathan Smith's papers on fevers, we referred to his conjecture, or belief, that typhoid fever could not be broken up by treatment; that it was in fact a self-limited disease. The best observers at that time were beginning to reach that conclusion. Louis' observation of typhoid fever led him to entertain the same notion; Andral's study of

[1] Autumnal Catarrh (Hay Fever), New York, 1872.

typhus led him, so far as that disease is concerned, to the same conviction. Doubtless there were other observers scattered here and there in Europe and America, who had learned to recognize the fact that some diseases were self-limited in their course, but such was not the common view. Dr. Jacob Bigelow, in a paper published in 1822,[1] was the first or among the first to make a clear and distinct statement—a grand generalization from the study and observation of disease—that self-limitation is one of the laws that govern the course of a large number of morbid processes. This paper is not only a statement of the law, but a demonstration of its truth. Dr. Bigelow did not claim absolute originality for his views, for in his paper he says: "I am aware that some of the most distinguished French pathologists of the present day incline to the opinion that many acute diseases, or at least inflammations, are incapable of being shortened in their duration by art. The opposite opinion prevails very generally in this country and in England, and it would be premature to consider the question as decided, until it has been submitted more extensively to the test of comparative numerical results." That test has since been applied, and has resulted in confirming the accuracy of Dr. Bigelow's statement. We do not partake of the enthusiasm of a medical friend, who said that he would rather have written Dr. Bigelow's paper on self-limited diseases than to have been the victorious commander at Waterloo. Still the paper was one of those clear and distinct statements of a truth, or rather of a natural law, which, by directing the attention of physicians in this country and elsewhere to a neglected and unrecognized fact, was an admirable contribution to the progress of practical medicine. It has undoubtedly saved a great many lives by preventing useless and violent medication, and has saved many more by turning the attention of practitioners to the support of the system, while disease was passing through its appointed orbit. The observation of every year since the appearance of Dr. Bigelow's paper has lengthened the catalogue of self-limited diseases. Science is beginning to learn that the laws which govern morbid processes are not less immutable than those which control the planets; and that therapeutics to be rational and successful must conform to these laws, and not undertake to neglect or thwart them.

A superficial observation would lead to the belief that delirium tremens could not be an illustration of the law of self-limitation in disease. Formerly, according to the popular and perhaps universal sentiment of the profession, delirium tremens was an affection that required prompt and active interference. In conformity with such a notion, the heaviest batteries of the materia medica were turned upon the unfortunate victims of this malady, and a rapid and unrelenting discharge of drugs kept up upon them. Opium, emetics, assafœtida, warm baths, digitalis, hyoscyamus, valerian, prussic acid, wormwood, spirits, sulphuric ether, hops, borax, and other articles were prescribed, separately or in combination, with extraordinary activity and zeal. Dr. John Ware, of Boston, was not satisfied with the results of such active and indiscriminate fighting. He accordingly determined to study the natural history of the disease. In 1831, he published a paper[2] on delirium tremens, founded exclusively upon a considerable number of cases of it which had occurred under his own observation. This memoir is an original one of marked value, and of special clinical interest. In it the expectant treatment during the

[1] Mass. Med. Soc. Comm. vol. iii.
[2] Transactions of the Mass. Med. Society, Boston, 1831.

paroxysm is highly spoken of, and its result is stated to be a termination of the attack "at a period seldom less than sixty, or more than seventy-two hours, from the commencement of the paroxysm." This result of the expectant treatment as demonstrated by his observations, he compared with the results of other kinds of treatment, as reported by those who have tried them. The inference from the comparison is not in favour of active interference. "I am satisfied, therefore," says Dr. Ware, "that in cases of delirium tremens the patient, so far as the paroxysm alone is concerned, should be left to the resources of his own system, particularly that no attempt should be made to force sleep by any of the remedies which are usually supposed to have that tendency; more particularly that this should not be attempted by the use of opium." Since the introduction of bromide of potassium and chloral hydrate as hypnotics, patients with delirium tremens have been enabled, by the aid of these agents, to pass more comfortably through the paroxysms of the malady, but it is doubtful whether the period of sleeplessness has been curtailed by them. The observation of more than forty years, that have elapsed since the appearance of Dr. Ware's paper, has confirmed the accuracy of his statements, and has also shown that delirium tremens is one of the diseases included by the law of self-limitation. Dr. Kuhn, of Philadelphia, treated this malady, nearly a century ago, after the expectant fashion[1] in a novel way, by "confining the patient in a dark cell, and leaving the disease spontaneously to work itself off." After an extensive trial of this method he was satisfied that it yielded a good result. He experimented also with the opium treatment[2] in 1783. The observations of Dr. Ware, which have just been cited, confirmed the earlier results of Dr. Kuhn.

Medical science is largely indebted to Dr. Austin Flint, of New York, an observer whose acquirements, accuracy of observation, and soundness of judgment have justly earned for him a European as well as an American reputation, for ascertaining that an affection so apparently irregular in its course, and so generally supposed to require active treatment as rheumatism, belongs to the class of self-limited diseases. A recent paper[3] of his contains a series of clinical observations on the treatment of acute articular rheumatism. It is not to be forgotton that Oppolzer instituted a similar inquiry some years ago with regard to the same disease. The distinguished German physician felt justified by his observations in asserting that drugs might mitigate the distress, and prevent or relieve some of the complications of rheumatism, but could not shorten its natural termination. Dr. Flint's observations confirm those of Oppolzer, and indicate that the rational treatment of this intractable affection consists in keeping it within its natural orbit, and not in vain efforts to curtail it.

Recent investigations, especially those of Charcot in locomotor ataxia, and those of Weir Mitchell on injuries of nerves, have disclosed an unexpected relation between certain derangements of the spine and swelling of the joints. More than forty years ago Dr. J. K. Mitchell, of Philadelphia, was led by rheumatic or rather by rheumatoid symptoms, in a case of caries of the spine, to suspect a connection between the medulla spinalis and the supposed rheumatism. He collected a num-

[1] Phila. Journ. Med. and Phys. Sciences, iii. 242.
[2] N. A. Med. and Surg. Journ., iv. 235.
[3] Am. Journ. Med. Sciences, July, 1863.

ber of cases besides those which came under his own observation, and founded upon them two papers,[1] one of which appeared in 1831, and the other in 1833. His observations were original and valuable. Their author did not follow them to the legitimate conclusions, which later investigations show might have been drawn from them. Nevertheless, as far as he went, he was in advance of his time. Dr. Flint has also recently published a paper on the natural history of acute dysentery, founded upon a series of cases observed and treated by himself. One of the practical conclusions which he drew from the study of these cases is that dysentery "is a self-limited disease, and its duration is but little, if at all, abridged by methods of treatment now and heretofore in vogue."[2]

In March, 1864, Dr. John C. Dalton read a paper[3] before the Academy of Medicine in New York, giving an account of some observations which he had previously made on Trichina Spiralis. In 1869 he supplemented this paper by another one[4] on the same subject. These two papers not only contain an account of what was previously known with regard to this curious parasite, but a number of interesting original observations upon trichinæ taken from trichinous meat, and also upon those taken from man. The two papers are valuable contributions to the natural history of trichinæ and to the best method of protecting the system from their ravages.

To arrange a series of facts so as to compare them with each other, ascertain their mutual relations, draw from them legitimate deductions, and thus demonstrate some unknown truth, or confirm one previously recognized, is to render as distinct a service to the cause of science as to collect the facts themselves. Indeed, the collection of facts without comprehending their relations to each other and to the whole world of facts, is a barren service.

Dr. Oliver Wendell Holmes, whose brilliant reputation as poet and novelist must not make us forget that he is also physician and anatomist, prepared a paper[5] in 1843 upon the important subject of the contagiousness of puerperal fever, a paper which belongs to the former of the two classes of contributions to medical science that we have just mentioned. The practical point which Dr. Holmes illustrated and proved is that "the disease known as puerperal fever, is so far contagious as to be frequently carried from patient to patient by physicians and nurses." The merit of this paper consists, not only in the collection and arrangement of the evidence that had accumulated upon an important matter, but in the logical and forcible presentation of the argument which the evidence legitimately warranted in favour of the point he maintained. Its value as a contribution to practical medicine is shown not only by the influence it exerted in this country, but also by the fact that Copland and Ramsbotham referred to it in approving terms, and that the Registrar General of England made use of it in his fifth annual report. It is interesting to note, that at the time when Dr. Holmes' paper appeared, two works that were largely, if not almost universally appealed to, as authorities in this country, viz., *Dewees' Treatise on the Diseases of Females*, and the *Phila-*

[1] Amer. Journ. Med. Sciences, May, 1831, and August, 1833.
[2] Ibid., July, 1875.
[3] Transactions of the New York Academy of Medicine, 1864.
[4] Medical Record, N. Y., April 15, 1869.
[5] This paper was published in the New England Journal of Medicine and Surgery for April, 1843.

delphia Practice of Midwifery, by Dr. C. D. Meigs, both taught the non-contagiousness of puerperal fever. At the present time the question may be considered settled in favour of the view which Dr. Holmes deduced from the facts which were then in his possession.

Not long after the appearance of Dr. Holmes' paper Dr. Samuel Kneeland, Jr., published one[1] on the connection between puerperal fever and epidemic erysipelas, in which he maintained that the two diseases are similar. His paper presented the evidence in favour of this view. Within the past year another American physician, Dr. Thomas C. Minor, of Cincinnati, has published a work[2] in which he enters into a careful and elaborate examination of the relations of puerperal fever to erysipelas, based upon the facts obtained from the census of the United States for 1870. Among the conclusions which Dr. Minor felt warranted in drawing from the evidence before him are the following:—

1st. That there is an ultimate connection existing between child-bed fever and erysipelas, and that in any place where erysipelas is found there will be found puerperal fever.

2d. Physicians attending child-bed fever cases and erysipelas at the same time were most unfortunate in their practice.

3d. Physicians having large obstetric practices, but who are known to be believers in the close connection of child-bed fever and erysipelas, returned few death certificates from either cause.

4th. Epidemic erysipelas is invariably associated with an outbreak of epidemic child-bed fever, or *vice versâ*. The *London Practitioner* for August, 1875, in a notice of Dr. Minor's work, says: "If it be asked what was the bond of the connection between erysipelas and child-bed fever here maintained, the same conclusion is suggested by the American as by the English experience, namely, chiefly the *doctor* and the *nurse*."[3]

Notwithstanding the care with which Asiatic cholera has been studied by competent observers in almost every part of the world, its pathology and treatment have not yet been clearly made out. We are indebted to Dr. William E. Horner, the distinguished anatomist of Philadelphia, for discovering one important fact with regard to it. The origin of the rice-water discharges in that disease had long been an unsolved problem. Dr. Horner first detected the fact that in cholera the whole epithelium is stripped from the small intestines, and that the turbid rice-water dejections, which are so characteristic of this disease, result from this peculiar stripping of the mucous membrane. For an account of Dr. Horner's researches, which were made with his singular patience and accuracy, and which led him to the discovery of this pathological fact, we must refer to his original article.[4] Here we can only call attention to his early recognition

[1] Am. Journ. Med. Sciences, April, 1846.

[2] Erysipelas and Child-bed Fever, by Thomas C. Minor, M.D., Cincinnati, 1874.

[3] The different departments of medical science naturally and inevitably run into each other to such an extent that it is impossible to draw a distinct line of demarcation between them. Their boundaries are fluctuating and indeterminate. It might be justly said that these references to puerperal fever belong more properly to the Report on Obstetrics and Gynæcology which will appear hereafter, than to one which is concerned only with practical medicine. On the other hand, erysipelas comes chiefly under the eye of the general practitioner. The mutual relations of erysipelas and puerperal fever may, therefore, be discussed as appropriately under the head of Practical Medicine as under the head of Obstetrics. Neither the general practitioner nor the obstetrician can afford to neglect them.

[4] Am. Journ. Med. Sciences, vol. xxi. page 289.

of it. His recognition and published record are illustrations of our previous statement, that American journals contain accounts of numerous isolated facts pertaining to the various branches of medicine which show that American physicians have not been idle scientific observers. The same observer instituted in 1827 a series of original and interesting inquiries into the healthy and diseased appearances of the gastro-enteric mucous membrane. He endeavoured to ascertain the healthy condition and appearance of this membrane, its appearance in congestion from the agonies of dying, and its appearance in genuine red inflammation. His conclusions were, that congestion is not an active condition of the part affected, but is most frequently the result of mechanical impediment to the venous circulation.[1] Dr. Horner also instituted an inquiry into the anatomical characters of Infantile Follicular Inflammation of the Gastro-intestinal Mucous Membrane, and into its probable identity with cholera infantum. This paper pointed out very clearly the changes which occur in the follicular apparatus.[2] Dr. Horner's labours in other directions, which have given him such a distinguished place among American anatomists, do not fall within the scope of this essay.

The investigations of Dr. John Neill, of Philadelphia, on the mucous membrane of the stomach, made a quarter of a century ago, were original, and added to our knowledge of the structure of that organ. The results of his investigations were given to the public in a paper[3] entitled "On the Structure of the Mucous Membrane of the Stomach," which may be consulted at the present day with advantage.

The liver has always presented an interesting and difficult field of study to the physiologist, the pathologist, and the practitioner. The problems which it offers to the student are far from being solved at the present day. While there is an agreement on many and important points among medical scientists, there are many others which are still debated. Some of the ablest living physiologists and histologists, like Claude Bernard, Ch. Robin, Kölliker, Schiff, and others, have been led by their investigations to entertain and defend different, and, sometimes, opposing views of the intimate structure and functions of the liver. American physicians and physiologists have not been mere spectators of these efforts to disentangle and clear up such knotty questions. Dr. Leidy's paper on the comparative structure of the liver[4] is the most exact and complete essay in the department of microscopic anatomy which has appeared in any American medical journal, and is a most valuable contribution to our knowledge of the liver.

To this we may add the researches of Dr. Austin Flint, Jr.,[5] of New York, upon cholesterine, which have thrown a good deal of light upon one of the obscure functions of the liver. According to him, says Küss,[6] "the excrementitial product formed by the disassimilation of the brain and of the nerves, at the expense of protagon, is represented by cholesterine, separated from the blood by means of the liver, and then thrown into the intestinal canal. This view is based upon a number of experiments which show, moreover, that the excretion of cholesterine is in

[1] Amer. Journ. of Med. Sciences, vol. i. 1827.
[2] Ibid., vol. iii. 1828.
[3] Ibid., Jan. 1851.
[4] Ibid., Jan. 1848.
[5] Ibid., Oct. 1862.
[6] Lectures on Physiology, by Professor Küss, translated by Robert Amory, M.D., p. 27.

direct ratio with the nervous activity. The common expression, to feel bilious, seems justified by one of the elements of the bile, viz., cholesterine." The connection between derangements of the liver and disturbance of the functions of the brain has long been clinically recognized. Whatever explains the mechanism of this connection, is as much a contribution to practical medicine as to physiology. Professor John C. Dalton, of New York, has rendered efficient service in this direction by his efforts to explain the glycogenic function of the liver. Schiff and Pavy maintain that the sugar found in the liver is a post-mortem product. Dr. Dalton, whose experiments were conducted, to say the least, with as much care, ingenuity, and rapidity as those performed by Schiff, Pavy, or Bernard, demonstrated the presence of sugar in the living liver. The practical relation of his experiments and their result to the question of diabetes is obvious.

Our present knowledge of gastric digestion is largely due to the opportunities, which gastric fistulæ have afforded physiologists for the inspection of the living stomach, or more exactly, of the stomach at work. Medical science owes a debt of gratitude to Dr. William Beaumont, surgeon in the U. S. Army, for leading the way in this method of experiment and observation. The subject of his experiments was Alexis St Martin, a French Canadian voyageur, who was wounded in 1822, in such a way as to produce a permanent gastric fistula. Fortunately Dr. Beaumont was able to keep St. Martin under his observation for a long time. By means of the fistula he made a series of extended, careful, and valuable experiments upon the digestibility of different articles of food and drink, and noted the behaviour of the stomach in a state of quiescence and in one of activity. His experiments and the inferences which he drew from them are so well known that it is unnecessary to describe them here. They are not only valuable in themselves, but opened the way to a method of investigation, which, both in this country and in Europe, has yielded in the hands of various physiologists important results to practical medicine.[1]

Dr. J. J. Woodward, now in Washington, D. C., published in 1864 a work on the *Chief Camp Diseases of the United States Armies as Observed during the Present War*. It is a practical contribution of great value to military medicine, and can be studied with profit by physicians in civil practice. Its account of camp diarrhœa and malarial fever are of especial interest.

No department of medical science has been studied with greater earnestness than that of the nervous system. Its importance justifies the labour and time expended upon it. Among American physicians and physiologists who have endeavoured to unravel its intricacies, Dr. S. Weir Mitchell, of Philadelphia, is *facile princeps*. He has done much valuable work in this direction, to which we can only briefly refer.

During our late civil war Dr. William A. Hammond, of New York, himself eminent as a neuro-physiologist and neuro-pathologist, established, at the time he was Surgeon-General of the United States Army, a hospital for nervous diseases, and invited Dr. Mitchell to take charge of it. Drs. Morehouse and Keen were associated with Dr. Mitchell in the management of the hospital. The experience acquired in this hospital by the gentlemen in charge of it led to the publication of a number of

[1] The Physiology of Digestion, with Experiments on Gastric Juice, by William Beaumont, M.D., U. S. A. The first edition of this work was published in 1833.

communications by them on nervous affections. One of the most important of these was entitled "Gunshot Wounds and other Injuries of Nerves," published in 1864. In the language of Dr. Mitchell, "this volume describes at length all the primary and secondary results of nerve wounds, and especially many hitherto undescribed lesions of nutrition, as well as a novel form of burning pain previously unknown, as a consequence of gunshot wounds. There are also full details of treatment, and a report of thirty-one cases of nerve lesions." With regard to this book, the *Edinburgh Medical Journal* says that it is valuable to practical surgeons, from the many details of treatment which it contains, and that it is "specially interesting to physiologists and neuro-pathologists, from the extreme care with which the cases appear to have been taken, and the exactness and minuteness of the descriptions of the effects of the injuries on motion and sensation." "The glossy skin," previously noticed by Paget, is here described in detail, and shown in many cases to be connected with the peculiar burning pain that is noticed. The same observers put forth a paper on Reflex Paralysis in 1864. In this paper a novel theory of "shock" from injuries is set forth, and cases related where a ball-wound of one limb caused paralysis of remote parts of the body.

The monograph on gunshot wounds was supplemented by Dr. Mitchell in 1871, by a memoir on "The Diseases of Nerves resulting from Tying." This was published in the medical volume of the Reports of the U. S. Sanitary Commission. In 1872 Dr. Mitchell published a work upon Injuries of Nerves and their Consequences, which he dedicated to Dr. Wm. A. Hammond, "whose liberal views," says Dr. Mitchell, "created the special hospital which furnished the chief experience of this volume." The work was chiefly based on the author's own observation. The *British and Foreign Medico-Chirurgical Review*, in a notice of this treatise, says it is "the first complete treatise on the subject the English language has been in possession of," and adds, the volume is "written not only up to the present time, but in many respects far in advance of it," to be referred to now and in the future "with the utmost confidence and satisfaction." In 1874 Dr. Mitchell published a paper on post-paralytic chorea,[1] in which he pointed out the fact that organic palsies, especially hemiplegia, "are occasionally followed by hemichorea, or a still more limited local development of that disorder." In other words, his paper shows that "as there is a post-choreal paralysis, so, also, is there a post-paralytic chorea." Our space forbids our pointing out the amount of original matter and suggestions which these various books and papers contain. As a whole they form the most valuable contributions to neurology and medicine in general which this country has produced. They are admirable as to style, logic, and ideas, and are full of suggestive hints and generalizations.

Any account of American contributions to neurological science and therapeutics, would be incomplete without a reference to the labours of Dr. William A. Hammond in that direction. His investigations upon the physiological action of remedies will be referred to in another place.

In his treatise on sleep[2] he has added materially to our knowledge of the physiology of that mysterious condition, and to the therapeutics of insomnia. So far as priority of discovery is concerned, the credit of

[1] American Journal of the Medical Sciences, vol. lxviii. p. 342, Oct. 1874.
[2] Sleep and its Derangements, by William A. Hammond, M.D.

ascertaining that sleep is due to a partial anæmia of the brain belongs to Drs. Durham and Fleming. Dr. Hammond, before he had heard of Durham's experiments, made similar ones, and arrived at similar results. His treatise, however, not only gives an account of his own original experiments upon the state of the intra-cranial circulation during sleep, but presents the whole subject of sleep and its derangements in a clear and satisfactory manner.

A full account of Dr. Hammond's contributions to neurological science may be found in his recent work on Diseases of the Nervous System. Of this treatise the author says in his preface: "One feature I may, however, with justice claim for this work, and that is that it rests to a great extent on my own observation and experience, and is therefore no mere compilation. The reader will readily perceive that I have views of my own on every disease considered, and that I have not hesitated to express them." The size of the work forbids our attempting to analyze it here. For any accurate notion of Dr. Hammond's peculiar views, and original observations, we must refer those interested in the matter to the work itself. We desire, however, to call attention to the account which it gives of athetosis, a disease first recognized and, we believe, first described by Dr. Hammond. His description of this rare affection is illustrated by two cases of it, which have come to his knowledge.

Electro-physiology, and electro-therapeutics for the last twenty-five years, and especially since the appearance of the treatise of Duchenne of Boulogne, upon those subjects, have attracted a great deal of attention. American as well as European observers have been busy with efforts to discover the relations of electricity to the nervous system. By far the most important contribution made by any American observer to this subject, is the treatise[1] of Dr. Charles E. Morgan. Unfortunately the author died before the work went to press. It was published under the editorial care of Dr. William A. Hammond, who thereby bore unequivocal testimony to its value. We learn, moreover, that so high an authority as Professor Rosenthal would gladly have undertaken the revision and editorship of this work, not only as a proof of his esteem for its writer, but also from his conviction of its eminent scientific value. An obscurity of style due partly to a lack of personal revision, and partly to German methods of expression, which the author's long residence and study in Germany had led him into, pervade the book. Whoever masters his style and gets at his thought will agree with the editor "that there is nothing in the English language which at all approaches it as regards the scientific treatment of the whole subject of electricity." It is mainly physiological, only about twenty-five pages being devoted to the therapeutics of the subject. The character of the results at which he arrived, and the stamp of his mind, may be derived from the closing paragraph of the book. "Such are the definite scientific applications of electricity to medical purposes; of the many others it need only be said that they are either based on incorrect theory or diagnosis of disease, or an imperfect or incorrect knowledge of electro-physiology; although I do not deny that future researches may enable us to do more, far more than has hitherto been done in this direction."

The New York Society of Neurology and Electrology recently appointed a committee consisting of Prof. John C. Dalton, Dr. George N.

[1] Electro-physiology and Therapeutics, Charles E. Morgan, A.B., M.D., New York, 1868, pp. 714.

Beard, and three others to examine and report upon the existence and localization of motor centres in the cerebral convolutions. The committee made a number of carefully conducted and ingenious experiments upon dogs.[1] The results at which the committee arrived confirmed the most important of those obtained by Hitzig and others who have followed him in this line of experiment. Although these and similar investigations are purely physiological in their character, yet they have such an obvious bearing upon diseases and treatment of the nervous system that they really belong to practical medicine.

The drugs of the Materia Medica, which are fortunately no longer regarded as the only or chief agents by which disease is prevented or combated, still justly hold an important though secondary place in the armamenta medicorum. The contributions of America to this department of practical medicine during the past century have been numerous and valuable. Our space permits a reference to only a few of them. As South America does not come within the limits of our survey, we are prevented from referring to cinchona and its alkaloids, a contribution to the resources of medical art, American in its origin, without which the modern practice of medicine would be sadly crippled. Excluding this and all other South American medicinal products from our consideration, let us glance at what the United States has contributed in the past century. As we shall have occasion to see by and by, it has led the way in the introduction of one class of agents whose value cannot be over-estimated.

Contributions to materia medica are of two classes. The first class comprises new, or previously unknown agents, whether vegetable or mineral in their origin, as veratrum viride or wild cherry, and also new chemical combinations, as chloroform or chloral. The second class comprises researches, either clinical or physiological, into the action of medicines, by which their therapeutical power and limits are determined. This class of course includes experiments by vivisections or otherwise on animals and various sorts of chemical analyses.

Let us glance for a moment at the first of these classes. For two or three hundred years previous to the beginning of the present century, there was a popular notion floating about in the community, especially in Germany and parts of France, to which physicians gave very little credence, that ergot was an oxytocic. It was commonly known in Germany by the name of *mutterkorn*, and in this country, as well as in Europe, was sometimes called *pulvis parturifaciens*, names that indicate the popular notion of its power. Notwithstanding the efforts of a distinguished French accoucheur, Desgranges, who recognized its value more than a century ago, and endeavoured to bring it into use, it was forgotten or not accepted by the faculty. Dr. John Stearns, of Saratoga County, New York, in a memoir[2] published in 1808, again called attention to ergot as a remedy for quickening childbirth. The paper gives an admirable account of the article it describes, and the profession since its time have acquired very little additional information with regard to it, for Dr. Stearns not only recognized its action upon the uterus, but its constringing power over the small bloodvessels, through the intervention of the nervous system. Soon after the appearance of Dr. Stearns' paper other observers confirmed his statements. Dr. Oliver Prescot published

[1] New York Medical Journal, March, 1875.
[2] New York Medical Repository, 1808, vol. xi. p. 303.

in 1814 a paper,[1] giving an account of the natural history and medical effects of secale cornutum. This paper though a less valuable contribution to medical science, than that of Dr. Stearns, had merit enough to be honoured by a French translation, and an introduction into the *Dictionnaire des Sciences Médicales.* The medical profession were now fully aroused to the value of ergot. The use of it spread rapidly over this country, and it was not long before European physicians recognized its virtue. It was established in the place it now holds as one of the important articles of the materia medica. American medical science may fairly claim the merit of restoring to therapeutics an agent, whose virtues Europe had failed to recognize.

We have the authority of the United States Dispensatory for the statement, that "chloroform was discovered by Mr. Samuel Guthrie, of Sackett's Harbour, N Y., in 1831. At about the same time it was also discovered by Soubeiran in France, and Liebig in Germany." Though the priority of discovery belongs to the American chemist, yet it is evident that the discovery was made by each of the three observers independently of each other; it is also evident that none of them had any notion of the anæsthetic virtue of chloroform to which we shall refer further on. In connection with the importance that chloroform afterwards attained, it is interesting to recall the language which Mr. Daniel B. Smith, of Philadelphia, used with regard to it in 1832. "The action of this ether" (meaning chloroform) "on the living system is interesting, and may hereafter render it an object of importance in commerce. Its flavor is delicious, and its intoxicating qualities equal to or surpassing those of alcohol. It is a strong diffusible stimulus, similar to the hydrated ether, but more grateful to the taste."[2]

Dr. Stillé, in his *Therapeutics and Materia Medica*, makes the statement that the American Indians were acquainted with some of the virtues of podophyllum. At any rate it was for a long time popularly known and used as a cathartic in this country before physicians employed it. Dr. Jacob Bigelow accurately described both the plant and its medicinal properties more than forty years ago. It did not come into general use, however, until its active principle, known as podophyllin, or more exactly resina podophylli, had been extracted. It is now freely used both in this country and in Europe, and cholagogue as well as cathartic properties are attributed to it. Although its virtues have been exaggerated, as have those of leptandrin and gelsemium, yet all of them are valuable additions to the materia medica.

The wild cherry, or prunus virginiana, is another contribution from the flora of America that deserves honourable mention. Its tonic and sedative properties were recognized more than fifty years ago by Dr. John Eberle, whose *Therapeutics and Materia Medica* introduced to the acquaintance of physicians a number of articles, previously unknown or little known, derived from the Americam vegetable kingdom. Dr. Eberle's experiments made upon himself with an infusion of wild cherry, by which he demonstrated its sedative influence upon the heart, deserve to be remembered not only on account of their intrinsic value, but because they show a recognition by him, at that early period, of the importance of making the physiological action of drugs the guide to their therapeutical employ-

[1] A Dissertation on the Natural History and Medical effects of Secale Cornutum or Ergot, by Oliver Prescot, Medical and Physical Journal, 1814, vol. xxxii. p. 90.

[2] Journal of the Phila. Coll. of Pharmacy, iv. p. 118.

ment. Dr. George B. Wood, of Philadelphia, one of the most accomplished of American physicians, has pointed out in his *Therapeutics and Materia Medica* the value of wild cherry in phthisis. Some of the foreign journals have also recorded observations in confirmation of these statements.

In 1850, Dr. W. C. Norwood, of South Carolina, proclaimed[1] in somewhat extravagant language the sedative virtues of veratrum viride. Dr. Tully, of New Haven, and other American physicians had previously employed it, but the attention of the profession was not generally directed to it until Dr. Norwood so ardently advocated its employment. Since that time, its active principles, viridia and veratroidia, have been isolated, and they, and the plant from which they are derived, have been subjected to a careful examination, so as to ascertain their physiological action. European and American physicians and chemists have interested themselves in this inquiry. The experiments[2] of Dr. H. C. Wood, Jr., of Philadelphia, are among the most valuable that have been made, and are satisfactory and conclusive. While they do not sustain the extravagant claims of Dr. Norwood, they demonstrate that veratrum viride deserves a place in the materia medica.

As we have already intimated, we cannot undertake to give an accurate catalogue of the numerous articles that have been introduced into the materia medica from the American vegetable kingdom. We can barely refer to such articles as geranium maculatum, whose astringent virtues have a local reputation; sanguinaria canadensis, which posseses emetic and expectorant properties; spigelia marilandica, whose anthelmintic virtues were described a century ago by Drs. Garden and Chalmers, of South Carolina; apocynum cannabinum, an emetic and cathartic, whose vulgar name of Indian hemp has led some practitioners to mistake it for a very different article, the Indian hemp of India; senega or seneka root (introduced by Dr. Tennant, of Virginia, in 1731), whose valuable expectorant properties are recognized in Europe and in this country; serpentaria or Virginia snakeroot, used by the American aborigines as a remedy for snake-bites, and considered by physicians of the present day to have stimulant, tonic, and other properties; eupatorium perfoliatum, which, under the less learned name of thoroughwort, is largely used in domestic practice; lobelia or Indian tobacco, an agent of undoubted activity as an emetic, a sedative, and an expectorant, largely used by a notorious charlatan and his disciples, and which possesses a value that gives it a place in our modern materia medica; gillenia, whose virtues as an emetic would enable it to replace ipecacuanha, if the latter could not be easily obtained; sassafras, sabbatia, gaultheria, and a variety of other plants. These, and other articles that might be mentioned, are additions of greater or less importance to the practitioner's list of remedies. Some of them are only known locally; others have travelled beyond the sea, and enjoy a transatlantic reputation. They are mentioned not only on account of their intrinsic merits, but to show that American physicians have not neglected to explore their native forests and fields, with the hope of enriching the materia medica of the world.

Our account of American contributions to materia medica would be imperfect without a reference to the works of Dr. Jacob Bigelow on medical botany. In 1814 he published an octavo volume upon the plants of Boston and its environs. A few years later, he published his

[1] Southern Med. and Surg. Journal, June, 1850.
[2] Amer. Journ. Med. Sci., 1870, and Philadelphia Medical Times, vols. ii. and iii.

American Medical Botany in three volumes, illustrated. This was a contribution to medical science of the highest order. Its descriptions are accurate, concise, and complete. The fifty years that have elapsed since its appearance have taken very little from, and added very little to his account of the plants he described or of their medicinal virtues. It still maintains its place as an authority upon the subjects of which it treats. In this connection the Medical Botany of Dr. W. P. C. Barton should be mentioned. It covers a different ground from that of Dr. Bigelow, but like his is a valuable addition to medical science.

Let us now pass to the second class of contributions to materia medica, viz., clinical, or physiological researches into the action of medicines upon the human system. American physicians and physiologists have begun to cultivate this delicate, difficult, and important field of inquiry. Prominent among the explorers of this region is Dr. William A. Hammond, of New York. His researches[1] upon the physiological effects of alcohol and tobacco upon the human system, upon albumen, starch, and gum, upon the excretion of phosphoric acid, and upon certain vegetable diuretics have added to our knowledge of these articles. Many of the experiments upon which these papers are founded were made upon himself. His paper upon alcohol appeared nearly fifteen years ago. It was limited chiefly to the action of alcohol upon metamorphosis of tissue. Since its appearance, the well-known, laborious, and entensive researches, in the same direction, by Lallemand, Perrin and Duroy, Anstie, Parkes, Binz, Duchek, and others, have greatly increased our knowledge of the physiological action of alcohol, but have not substantially shaken the conclusions of Dr. Hammond. The diuretics whose action he investigated were squill, juniper, digitalis, and colchicum. His object was to ascertain their influence over the quantity of the urine, its specific gravity, and the amount of its solid organic and inorganic constituents. His results explain and confirm the conclusions with regard to the therapeutical value of these drugs that physicians have generally held.

The experiments of Dr. Hammond as to the physiological action of diuretics upon healthy adults, were supplemented by a series of clinical experiments[2] with diuretics by Dr. Austin Flint, of New York. Dr. Flint experimented with squill, digitalis, nitrate of potassium, iodide of potassium, acetate of potassium, colchicum, and juniper. He gave these articles separately and in combination. His conclusions with regard to their action upon the solid and liquid constitents of the urine, substantially confirm those of Dr. Hammond. Dr. Flint modestly calls these researches "a small contribution to the study of the effects of diuretic remedies;" they are not only valuable in themselves, but at the time they were made, fifteen years ago, had an especial value as indicating the proper method of the clinical observation of remedies.

The researches of Dr. H. C. Wood, Jr., upon the physiological action of drugs are admirable illustrations of the modern method of ascertaining their action. We have already referred to his study of the action of veratrum viride and its alkaloids. He has made several other similar contributions, which are embodied in his recent work, *Therapeutics, Materia Medica, and Toxicology*. This work is an original contribution to practical medicine. It not only presents a condensed statement of the

[1] Physiological Memoirs by William A. Hammond, M.D.
[2] American Med. Monthly, New York, Oct. 1860.

author's investigations, but of the best European investigations upon the physiological action of medicines.

In this connection, it should be mentioned, that the recent experiments of Dr. Robert Amory, of Boston, and of Prof. H. P. Bowditch, of the same city, upon the absorption and elimination of the bromide of potassium and its kindred salts, have enlarged our knowledge of the action of those remedies in this direction. Dr. J. H. Bill, U. S. A., made a series of experimental researches into the action and therapeutic value of the same article, which he published[1] in 1868. The experiments were made on man. They were carefully conducted, and led him to the conclusion that "bromide of potassium, in its legitimate action, is an anæsthetic to the nerves of the mucous membranes and a depressor of their action."

Dr. S. Weir Mitchell, assisted by Drs. Keen and Morehouse, made some admirable observations and experiments,[2] at the United States Army Hospital for injuries and diseases of the nervous system, upon the antagonism of atropia and morphia. They also made an examination into the power of conia, daturia, atropia, and morphia to destroy neuralgic pain. Their observations led them to the conclusion that morphia and atropia act as mutual antagonists in certain of their effects. The paper is marked by the care, accuracy, and completeness that characterize all of Dr. Mitchell's researches. It deserves to be read in connection with Dr. Fraser's exhaustive researches in the same direction.

An American, travelling in Hungary not long ago, attended some sort of a public meeting in one of the large towns of that region. One of the speakers had occasion to allude, in the course of his remarks, to Boston in the United States. He referred to it as a place well known to the audience, and distinguished not as the cradle of the American revolution, not for its commerce, not for its literature, not for its statesmen, its authors, its poets, and its theologians, not for its manufactures, not for its common school system, but as the place where anæsthesia by means of sulphuric ether was discovered. This discovery has rendered the name of Boston familiar to the dwellers on the Danube and the Caspian. Such a fact may not be gratifying to the vanity of Bostonians, but it testifies to the universal recognition of the inestimable value of artificial anæsthesia. In every part of the civilized world, and wherever, in those regions called uncivilized, the pioneers of civilization have penetrated, in Japan, in China, in the islands of the sea, the power to produce anæsthesia, which ether first revealed, is acknowledged and blessed. The knowledge of it is so universal, and the blessings which attended its use are so constant, that we are sometimes apt to think as little of its existence and power as we do of the presence and power of light. It is impossible to estimate or form any adequate conception of the amount of human suffering which anæsthetics have relieved and prevented. To their discovery the human race owes the blessing that no pain follows the course of the surgeon's knife into any living tissue; that the accoucheur can, when necessary, alleviate or abolish the agonies of travail; that sleep can be procured in spite of any agony; and that at the word of the physician any sufferer may be rendered unconscious of torture. Such a power, which John Baptista Porta had strangely prophesied centuries ago, which mesmerism hinted at, which mystics have now and then proclaimed, but which the world

[1] Amer. Journ. Med. Sciences, July, 1868. [2] Ibid. July, 1865.

never dared to expect, was shown to exist and to be capable of safe and easy application by the use of sulphuric ether at the Massachusetts General Hospital in 1846. It was, perhaps, the greatest contribution to practical medicine that the world has ever received. Of itself, it is enough to render American medical science honoured and memorable.

As soon as it was generally known that the inhalation of the vapour of sulphuric ether would produce insensibility, experiments were made with various substances by physicians in this country and in Europe, who hoped to discover other agents, the inhalation of whose vapour would produce anæsthesia as well as ether, or, perhaps, better than that article. The most distinguished of these experimenters, Sir James Y. Simpson, of Edinburgh, tried chloroform, a substance which was previously regarded chiefly as a chemical curiosity. The experiment of Sir James, made upon himself, disclosed the fact that chloroform was an anæsthetic of great power. The inhalation of its vapour acted rather more rapidly than the inhalation of ether, was less disagreeable to the patient, and in a very small quantity produced profound anæsthesia. The knowledge of the discovery of this new anæsthetic spread rapidly over Europe and this country. The ease of its application, the profound insensibility which it produced, its freedom from any unpleasant odour, and the fact that it was discovered in Europe, while ether came from the wilds of America, and other circumstances, led to its adoption almost universally throughout Europe as an anæsthetic in preference to ether. It was also used very largely in this country, but not as exclusively as on the other side of the Atlantic. The experience of a quarter of a century has shown that the inhalation of chloroform is fatal in a certain proportion of cases, while the inhalation of ether is comparatively innocuous.

American medical science has not only contributed to practical medicine the discovery of artificial anæsthesia, but insists that the anæsthetic, which it first presented to the world, is still the best that is known to science; it insists upon its demonstration of the fact that pure concentrated sulphuric ether is preferable, as an anæsthetic, to chloroform; and, as a logical conclusion from this, it insists that the persistent use of chloroform by European physicians is a grave and serious error. This is not the place, even if we had the time, to discuss the comparative merits of ether and chloroform. Physiological experiments and clinical experience are both in favour of ether, and against chloroform. The prolonged inhalation of ether by man affects, first, the cerebrum; second, the sensory centres of the cord; third, the motor centres of the cord; fourth, the sensory; and fifth, the motor centres of the medulla oblongata. When ether kills, it does so by producing asphyxia, leaving the pulsations of the heart to warn the surgeon of the approach of danger—warnings which only the most reckless carelessness can fail to notice. Chloroform, like ether, affects chiefly the brain and spinal centres; but its action upon the sensory and motor centres is more rapid than that of ether. Upon the heart it produces a steadily depressing influence. When it kills, it does so by cardiac paralysis, acting directly upon the heart-muscle, and not by asphyxia; consequently there is no warning of impending death, and the greatest carefulness cannot avert the fatal issue. When ether kills, death is due to the carelessness of the operator; when chloroform kills, death is due to the rashness, wilfulness, or ignorance of the operator in the selection of his anæsthetic. Dr. H. C. Wood, Jr., whose contributions have been previously referred to, says:—

"As an anæsthetic, chloroform possesses the advantages of quickness and pleasantness of operation, smallness of dose, and cheapness. These advantages are, however, so outbalanced by the dangers which attend its use, that its employment under ordinary circumstances is unjustifiable. It kills without warning, so suddenly that no forethought, or skill, or care can guard against the fatal result. It kills the robust, the weak, the well, the diseased, alike; and the previous safe passage through one or more inhalations is no guarantee against its lethal action. Statistics seem to indicate a mortality of about one in three thousand inhalations; and hundreds of utterly unnecessary deaths have now been produced by the extraordinary persistence in its use by a portion of the profession. It ought, therefore, never to be employed except under special circumstances, as in some cases of puerperal eclampsia, when a speedy action is desired, or in the field during war time, where the bulkier anæsthetics cannot be transported."[1]

These pages have made no reference to the contributions which American medical science has made during the past century to surgery, to obstetrics, including gynæcology, or to medical literature. An account of these contributions, and of the work of American physiologists, will be presented in future essays. The limited portion of the field, however, which we have surveyed has yielded enough of interest and importance to justify an honest pride on the part of American physicians in the work they have accomplished, and to give assurance of continued and zealous labour in the future.

The blank pages of the book, containing all the secrets of medicine, which Boerhaave bequeathed to the future, were prophetic of the work which medical science was destined to accomplish. The science of his age could inscribe only a single sentence upon a single page. The present century, whose closing hours the nation celebrates, has filled two or three additional pages with the secrets it has discovered, calling them vaccination, anæsthesia, and preventive medicine. It now transmits the volume to the coming ages, confident that each succeeding century will make new discoveries, till all of Nature's secrets are discovered, and then the title of the book shall be the just index of its contents.

The discovery of artificial anæsthesia was an event of such transcendent importance that it becomes necessary to give a complete account of it in this connection. As soon as its value was established, a number of individuals claimed the honour of its discovery. The controversy which the various claimants and their partisans have carried on has been prolonged and sometimes bitter. Most of those who were familiar with the way in which the discovery was introduced to the public and acquainted with the claimants to it, and in a position to form an impartial and correct opinion of the value of their claims, are no longer living. Of the surgeons of the Massachusetts General Hospital, who were present when the first operation under ether, the experimentum crucis of the new discovery, was performed, only one is now living. Fortunately that one, Dr. Henry J. Bigelow, the distinguished and accomplished professor of surgery in Harvard University, was not only present when the first operation was performed, but was personally acquainted with most of the steps in the early progress of the discovery, with the claimants to the honour of it, and with all of importance that appertains to the history of it. He did more than any other living person to bring it before the med-

[1] Treatise on Therapeutics, etc., by H. C. Wood, Jr., M.D., p. 251.

ical public of this country and of Europe, to assert its real value, and to point out the best methods of utilizing it. A quarter of a century has elapsed since its discovery. This period is long enough for the heat of partisanship to cool, and to afford an opportunity for an impartial statement, by an impartial observer, to be heard with calmness and interest. Moreover, the centennial anniversary of the nation's existence is an auspicious moment for putting on record the testimony of an intelligent and disinterested witness to the discovery of an agent in which the whole human race are interested. With the hope of obtaining from Dr. Bigelow a history of the discovery of anæsthesia, the following note was addressed to him:—

"Dr. HENRY J. BIGELOW, *Professor of Surgery in Harvard University.*

DEAR SIR: I am preparing a report on the progress of practical medicine in this country for the past century. In such an essay a notice of America's greatest contribution to medicine, modern anæsthesia, is indispensable. If you have the time and are willing to prepare a history of its discovery, with which you are so familiar, you will not only confer a favour upon all interested in it, but will put on record an authentic account of the discovery, by one who was an eye witness and actor in its early scenes, and to whose statements, on account of their disinterestedness, great value is attached."

To this letter the following paper was received in reply.

A HISTORY OF THE DISCOVERY OF MODERN ANÆSTHESIA.

MY DEAR SIR: A quarter of a century ago, your simple proposition would have re-awakened a discussion which had already exhausted the subject. Even so long ago as 1848, I thought it discreet to preface a paper upon the abstract question of discovery, as decided by historical precedent, with the disavowal of an intention to "dig up the well-worn hatchet of the ether controversy." But I see no objection to a review now—final, so far as I am concerned—of the occurrences you refer to, especially if I offer no opinion without its reason.

The singular persistence of the controversy was due to a variety of causes. People differ in their views about what constitutes a discovery or a discoverer. A voluminous mass of sworn testimony availed little in those days, for want of some machinery to reach and fix a decision based upon it. One of the contestants, at least, felt this, and vainly urged a court of justice.[1] Preponderating opinions and evidence were laboriously and repeatedly brought to the surface by Congressional committees, and by other bodies and committees of those most familiar with the circumstances; but, for the lack of a tribunal accustomed to estimate the weight and quality of scientific evidence, not to say evidence of fact, no absolute decision was reached. The result was, that every new discussion began and ended like the preceding, and to as little purpose.

When the discovery was announced (October, 1846), the circumstances were few and recent, and the details of its progress were known. But when history was obscured, when another State, and even another Nation, had set up each its own discoverer, and readers were perplexed with volumes of new reports and new testimony, it became less easy to sift the evidence. Claims, till then distinct, overlaid each other. Each alleged inventor, with his partisans, aimed to secure the whole honour. Opinions were pro-

[1] See Congressional Debates for the sessions of 1853 and 1854.

nounced by people who knew little of the facts. Attempts were even made, in more than one instance, to forestall or manufacture public opinion, by procuring in promiscuous medical assemblies and legislatures sudden votes designating some discoverer by name, with a view to influence the erection of statues. In a scientific view such votes are not worth the paper that records them; but it cannot be doubted that in a free country every citizen has the inherent right, of which the late Lord Timothy Dexter so liberally availed himself, to erect in his front yard a statue with an inscription.

The claimants to the discovery are three—Dr. Wells, Dr. Jackson, and Dr. Morton. In discussing their claims, we cannot overlook the fact that the discovery was equally possible to either of them, or indeed to any moderately ingenious person whose attention should have been directed to the subject. This greatest single step forward in the history of medicine, like the division of the Roman printing-block, was a very small advance in strictly scientific knowledge. Facts of insensibility to pain, produced both by gas and ether, were already known to the world. Art needed only an extension of their application; and so far as art or science was involved, either Wells, Jackson, or Morton was competent to the work.

This simple statement comprehends certain points of vital importance. The first essential requisite of modern anæsthesia is, that it shall be always attainable, and, when attained, complete. A second requisite is, that the insensibility shall be safe. The discovery embraced the threefold and essential novelties, that it is, under proper guidance, *complete*, *inevitable*, and *safe*, and not, like all previous stupefaction, partial, occasional, or dangerous. If only partial or occasional, or if dangerous, neither the patient, the dentist, nor the surgeon would consider it of value. Even so late as a year after the discovery, many surgeons, and, extraordinary as it may now appear, some hospitals, absolutely declined to use the new means of producing insensibility, on the ground that it was attended with danger.

Readers of the present day may not remember how surprisingly far previous knowledge of anæsthesia had extended. Although there has been a want of discernment in attempts to point out precisely what the new advance upon previous knowledge was, one great difficulty has been, that this advance was so small, in a strictly scientific view, that it was not easy to measure it. This difficulty was enhanced by the magnitude of the spoil, whether mere honour, or, as beyond all question it should have been, emolument.

A rapid review of the history of early anæsthesia will bring us to the period with which we are especially concerned.

In the anæsthetic state, the action upon the brain may be a primary one, as by its compression, or by hypnotism—or secondary, as by narcotic and inebriating agents absorbed into the blood, from the lungs, the digestive tube, the skin, or other tissues. A few extracts, abridged from the familiar literature of the subject, will show that surgical anæsthesia in these various forms has been long known and longer sought.

The use of poppy, henbane, mandragora, hemp, etc., to deaden the pain of execution and of surgery, may be traced to a remote antiquity. Herodotus ascribes to the Scythians the use of a vapour of hempseed to produce drunkenness by inhalation. In China, Hoa-tho, in the year 220, administered hashish (*Mayo*) and performed wholly painless amputations;

the patient recovering after a number of days. Hashish, described by Herodotus twenty-three centuries ago, is the active agent of the modern *Bhang* of India.

Pliny, who perished A.D. 79, says of mandragora: "It is drunk before cuttings and puncturings, lest they should be felt." Dioscorides gives an elaborate method of preparing mandragora to produce anæsthesia (ποιεῖν ἀναισθησίαν) in those who are to be cut or cauterized—" or sawed," adds Dodoneus, and who in consequence "do not feel pain." Half an ounce, with wine, says Apuleius, a century later, enables a patient to sleep during amputation "without sensation." "Eruca" was used by criminals about to undergo the lash. "Memphitis," a "stone," so "paralyzed parts to be cut or cauterized that they felt no pain."

Theodoric, about the year 1298, gives elaborate directions how to prepare a "*spongia somnifera*," by boiling it dry in numerous strong narcotics, and afterwards moistening it for inhalation before operations. In 1832, M. Dauriol, near Toulouse, cites five cases of insensibility during surgical operations, induced by him with the aid of a similar "sponge" It is said that persons unpacking opium have fallen suddenly.

In 1532, Canappe described the inhaling-sponge of Theodoric, and at the same time warned surgeons against giving opium (*à boire*), which "sometimes kills." But in later years, and until ether was introduced, it was the rule to give opium before operations.

September 3, 1828, M. Girardin read to the Academy of Medicine a letter addressed to his Majesty, Charles X., describing surgical anæsthesia by means of inhaled gases.

Richerand suggests drunkenness in reducing dislocations. Patients, while dead drunk, have been operated upon painlessly, and a dislocated hip was thus reduced after a bottle of Port wine. Haller, Deneux, and Blandin report like painless results in surgical and obstetric experience.

Baron Larrey, after the battle of Eylau, found in the wounded who suffered amputations a remarkable insensibility, owing to the intense cold. Of late years congelation has become a recognized agent of local anæsthesia.

Hypnotism is a very remarkable cerebral condition, by which surgery has been rendered painless. It is the grain of truth upon which the fallacies of mesmerism, animal magnetism, and the rank imposture of so-called spiritualism have been based.

The experiments of the Grotto del Cane are familiar, as also are recoveries from accidental asphyxia after complete insensibility.

About a year ago, two healthy men, at my request, inhaled atmospheric air from a common gas-bag. As carbonic acid replaced the oxygen, they both became livid, and, to every external sign, utterly insensible. One was really insensible; the other nearly so, but, being a plethoric subject, it was deemed prudent in his case not to press the inhalation further.

Nitrous oxide after a time asphyxiates, owing to the chemical combination of its gases—on that account parting reluctantly with its oxygen. But it also exhilarates, and its anæsthesia is probably something more than a condition of asphyxia.

These facts show that from time immemorial the world has been in possession of an anæsthesia which was occasionally resorted to, and not unfrequently amounted to complete insensibility. But, as a rule, the following propositions held good in respect to it.

1st. It could not be relied on to affect everybody.

2d. It was often insufficient.

3d. It was sometimes dangerous.

What surgeons and patients needed was an inevitable, complete, and safe condition of insensibility; and this they were soon to have.

The moment arrived. In three months from October, 1846, ether anæsthesia had spread all over the civilized world. No single announcement ever created so great and general excitement in so short a time. Surgeons, sufferers, scientific men, everybody, united in simultaneous demonstration of heartfelt mutual congratulation.

It is to be regretted that no single individual stood out clearly, at this time, to receive the homage and gratitude of the world.

Nothing like the new anæsthesia had been known before. Whatever has been devised since has been a mere imitation and repetition of this—I may almost say, with no single substantial advantage over it. Our English friends, with a pardonable pride in matters of scientific discovery, not unfrequently formulate their convictions thus: "A. had indeed shown this, and B. that; but it was reserved for our own C. to make the imperishable discovery." It is probable that the average Englishman still believes that modern anæsthesia is identified with chloroform. But the discovery of a practicable, safe, and efficient means of insensibility had been made a year before chloroform was thought of, and nothing important has been added to it since. Chloroform is at a first inhalation of an agreeable odour, more portable and less inflammable than ether, qualities which eminently adapt it to army use; but it will do nothing that ether does not do as well, and is sometimes, though quite rarely, it is true, followed by fatal consequences.

We are now to examine the claims of the three individuals mentioned, to the discovery of the new anæsthesia. Let us look first at those of Dr. Wells. And as preliminary to the examination of his claim, let us here revert to an interesting point in this history. It is impossible to read the annexed statement, familiar though it be, without renewed amazement that this great blessing to animal existence was distinctly offered to scientific men, and as distinctly neglected by them for half a century.

The following are the words of Sir Humphry Davy, at the beginning of the present century; half the century had nearly elapsed before they were again thought of:—

"In one instance, when I had headache from indigestion, it was immediately removed by the effects of a large dose of gas" (nitrous oxyde), ... "though it afterwards returned, but with much less violence. In a second instance, a slighter degree of headache was wholly removed by two doses of gas.

"*The power of the immediate operation of the gas in removing intense physical pain I had a very good opportunity of ascertaining.*

"In cutting one of the unlucky teeth called *dentes sapientiæ*, I experienced an extensive inflammation of the gum, accompanied with great pain, which equally destroyed the power of repose and of consistent action. On the day when the inflammation was most troublesome, I breathed *three large doses* of nitrous oxyde. The pain always diminished after the first three or four inspirations; the thrilling came on as usual, and uneasiness was for a few minutes swallowed up in pleasure. As the former state of mind, however, returned, the state of organ returned with it; and I once imagined that the pain was more severe after the experiment than before. . . . ."

Towards the conclusion of his book he adds:—

"*As nitrous oxyde, in its extensive operations, appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place.*"

The great discovery was here clearly pointed out to every tyro in medicine and chemistry. There were three experiments, of a completely original character, and with a new agent, in a direction to which contemporaneous attention was not, as afterwards, leaning. Upon these an original hypothesis was methodically constructed and distinctly enunciated in print. It only remained for somebody to test this hypothesis, this guess, and to convert the guess into a certainty. But neither dentist nor surgeon responded, and the world for nearly fifty years attended exhibitions where people danced, laughed, and became unconscious, while hospital patients were undergoing amputation, alive to all its agony.

In 1844 Horace Wells appeared, exactly repeating the hypothesis that Davy had printed. Whether he got this idea from Davy, or from his friend Cooley, or from his own brain, is not to the purpose. Davy had announced it, and the scientific world knew it. Did Horace Wells convert into a certainty the probability of Davy? He did not. He signally failed to do so, and, mortified by his failure, he gave up all attempts in that direction. More than two years elapsed, and the ether discovery was made and completed. Then, and then only, was Wells stimulated to renewed effort. But it was too late; the discovery of a surgical anæsthetic had been made.

These facts deserve a careful examination. Wells had experimented in about "fifteen cases," and with varying success. This he states distinctly in his first reclamation, before his claim had expanded so as to embrace, as it afterwards did, the whole anæsthetic discovery.

He was "so much elated with the discovery" (to use his own words) "that he started immediately for Boston," and obtained from Dr. Warren permission, as Dr. Morton, imitating Wells, subsequently did with ether, to exhibit the anæsthetic properties of gas before the medical class. Dr. Warren was the principal New England surgeon of that day, and it was the obvious thing to do. This experiment, which was in tooth-pulling, was an utter failure, and was called, as Wells says, "a humbug affair." He was completely discouraged; went home, told his friends that the gas "would not operate as he had hoped," and wholly ceased to experiment, from the date of his failure in Boston, in December, 1844, until the spring of 1847, after he returned from Paris, an interval of more than two years. Wells's want of success can now be satisfactorily explained. He had, through Colton, in following Davy's instructions, made use of the traditional exhilarating-gas-bag, and of Davy's exhilarating dose. This volume of gas is inadequate to produce anæsthesia with any certainty; and Wells failed to suggest a larger dose. This small omission closed his chances. He narrowly missed a great invention. Inventors, by thousands, have missed inventions as narrowly.

Modern dental insensibility by nitrous oxide is unfailing, because the volume employed is much larger. It is also usual to exhale it into the atmosphere. Horace Wells had no hand in this method, of which the first demonstration was in a breast excision performed by myself at the Massachusetts General Hospital, in April, 1848, by means of about sixty gallons of gas, as a substitute for ether.

From all this it will be seen that Wells did not, as has been claimed for him, "discover that the inhalation of a gaseous substance would *always* render the body insensible to pain during *surgical operations*," but only that it would *occasionally* do so; and until long after the ether discovery, his experiments were *not "surgical operations," but only tooth-*

Hartford Oct 20. 1846

Dr Morton
Dear Sir
Your letter dated yesterday is just received, and I hasten to answer it for fear you will adopt a method in disposing of your rights which will defeat your object. Before you make any arrangements whatever, I wish to see you. I think I will be in Boston the first of Next week – probably Monday night. If the operation of administering the gass is not attended with too much trouble and will produce the effect you state, it will undoubtedly be a fortune to you provided it is rightly managed

Yours in haste

H. Wells

pulling. Wells's anæsthesia had no value to patient, dentist, or surgeon. In endeavouring to trace dental anæsthesia, as Davy had directed, from toothache to tooth-pulling, his experiments unfortunately and erroneously showed that what availed in Davy's hands for toothache would not always avail for tooth-pulling. His slight, but fatal, error of using an inadequate volume of gas damaged the knowledge of his day; so that a scientific person who had read Davy's encouraging and unqualified prediction, based upon his three successful experiments, would have been discouraged and thrown off the track by witnessing Horace Wells's contradictory results.

Wells returned from Boston to Hartford. Having hoped much from anæsthesia as a money speculation, he now left it for more promising enterprises. He got up a panorama or exhibition of Natural History at the City Hotel at Hartford; initiated an extensive business in the sale of patent shower-baths; and somewhat largely invested in cheap copies of Louvre pictures painted in Paris, to be framed and sold by auction in this country. This carried him to Paris about December, 1846; an important event in his career, as we shall presently see.

Before he went, however, the ether discovery was made (October, 1846), and he received from Morton the following letter:—

"BOSTON, October 19, 1846.

"FRIEND WELLS. Dear Sir: I write to inform you that I have discovered a preparation, by inhaling which a person is thrown into sound sleep. The time required to produce sleep is only a few moments, and the time in which persons remain asleep can be regulated at pleasure. Whilst in this state the severest surgical or dental operations may be performed, the patient not experiencing the slightest pain. I have perfected it, and am now about sending out agents to dispose of the right to use it. I will dispose of a right to an individual to use it in his own practice alone, or for a town, county, or State. My object in writing to you is to know if you would like to visit New York, and the other cities, and dispose of rights upon shares. I have used the compound in more than one hundred and sixty cases in extracting teeth, and I have been invited to administer to patients in the Massachusetts General Hospital, and have succeeded in every case.

"The Professors, Warren and Hayward, have given me written certificates to this effect. I have administered it at the hospital in the presence of the students and physicians, the room for operations being as full as possible. For further particulars I will refer you to extracts from the daily journals of this city, which I forward to you.

"Respectfully yours, WM. T. G. MORTON."

To this Wells returned the following remarkable and conclusive reply:—

"DR. MORTON. Dear Sir: Your letter, dated yesterday, is just received, and I hasten to answer it, for fear you will adopt a method in disposing of your rights which will defeat your object. Before you make any arrangements whatever, I wish to see you. I think I will be in Boston the first of next week, probably Monday night. *If the operation of administering the gas is not attended with too much trouble, and will produce the effect you state, it will undoubtedly be a fortune to you, provided it is rightly managed.*

"Yours, in haste, H. WELLS."

(A fac-simile of this letter is here appended. The original is in the collection of the Massachusetts Historical Society.)

Wells would not have thought that Morton's "operation" of "administering" his so-called "gas" (meaning ether) would prove "a fortune" to him, if his own results of two years before had, in the opinion of himself or anybody else, any considerable anæsthetic value.

The rest is briefly told. Wells soon after sailed for Europe, to prosecute the picture business already mentioned. The distinguished American dentist, Brewster, resident in Paris, hearing of his brother dentist Wells, sent to him, "begging him to call on him," and asked him, "Are you the true man?" "His answers, his manners," writes Brewster, "convinced me that he was." "*Dr. Wells's visit to Europe*," writes Brewster again, "*had no connection with his discovery*, and it was only after I had seen the letters of Drs. Ellsworth and Marcy that I prevailed upon him to present his claim to the Academy of Sciences, the Academy of Medicine, and to the Parisian Medical Society." The quarrel of Jackson and Morton was Wells's opportunity, and Brewster's persuasion thus secured for him a European hearing.

Thoroughly uneasy, Wells returned home (March, 1847). The world was everywhere ringing with ether announcements. From this period his claim rapidly expanded, until it embraced the whole discovery, unsettled his business relations, embittered his life, unhinged his reason, and he at last died, in New York, a sudden death, after extraordinary acts which led even to his arrest, but for which he could not be considered responsible.

Thus perished Wells, volatile, ingenious, enterprising; an experimenter, like scores of others, in the field of anæsthesia, but, like them, unsuccessful in establishing anything of value. So far as his labours went, he left scientific knowledge, as well as its application to art, just about where Davy had left it half a century before. But he had kept the subject alive, and had unintentionally planted a seed in the mind of his ambitious partner which yielded fruit.

We now come to the claims of Dr. Jackson and Dr. Morton; and these, for convenience, we will consider together.

It is significant that Wells, Jackson, and Morton were all in contact at some period of their anæsthetic experiences, of which they shared in some degree a common knowledge. Wells, while in Boston, visited Jackson's laboratory; and Jackson says that he knew of Wells's experiments; and it should be observed that his advice to Channing and Peabody was after Wells's visit. Morton had been Wells's partner in dentistry, and boarded at Jackson's house. In 1844 he had been a student of Jackson, who testifies in a certificate that he was a "skilful operator in dentistry," and had "studied the chemical properties of the ingredients required for the manufacture of artificial teeth."

Gas and ether were long familiarly known to produce effects so similar that whoever thought of one as an exhilarator or anæsthetic must have thought of the other. For example while in College, in 1837, I twice made a gasometer of nitrous oxide, and then substituted for it ether, as affording equal exhilaration. Brewster, in 1847, said, "It required neither a physician nor a surgeon to tell . . . . that ether would produce insensibility, ... as there is scarcely a school or community in our country where the boys and girls have not inhaled ether, to produce gayety, and many are the known cases where they became insensible." In short, gas and ether experiences were very similar. Wells had been, at the outset, distinctly advised to try ether, but elected gas as less dangerous. If, when afterwards "disheartened by the failure" of his gas experiment in Boston, he remembered ether, he doubtless thought it would be hardly worth his while to recur to an agent so similar in its effects.

In September, 1846, Jackson and Morton had their well-known interview. At this interview Jackson made a suggestion which was soon followed by a discovery, and by a controversy concerning the value of this suggestion. Jackson claimed that the suggestion was the whole discovery. Morton took the extreme opposite ground in behalf of his experiments. These he alone had conducted, and, while Jackson beyond all question kept aloof, he, recognizing their generally conceded danger, had gone on with them, notwithstanding, and proved what was before only suspected.

The interview was briefly this; and as it is the only point at which Jackson touches the progressive line of Morton's investigation, it should be stated strongly for Jackson. On the 30th of September, 1846, Dr. Morton went to the laboratory of Dr. Jackson, whom he knew well, having been a student under him, and recently in his house, and took from a closet an India-rubber gas-bag. In reply to an inquiry of that gentleman, he said, in substance (and all that Jackson claims Morton to have said may be admitted), "that he meant to fill the bag with air, and by its aid extract the teeth of a refractory patient."[1] A conversation ensued upon the effects of the imagination, also, among other things, of nitrous oxide, in producing insensibility. Jackson treated Morton's proposition lightly. He told him to go to an apothecary and procure sulphuric ether—the stronger, the better—which would produce the insensibility he desired. The ether was to be spattered on a handkerchief and inhaled; in a moment or two perfect insensibility would be produced. "Sulphuric ether," said Morton, "what is that? Is it gas? Show it to me." Jackson showed him some ether, and after further conversation Morton went to procure it. Such was the substance of the interview at which Morton professed ignorance of ether, and Jackson entire knowledge of it. Jackson's knowledge and Morton's alleged ignorance we may now consider.

Dr. Jackson, if we may judge from his later attitude, was not indifferent to renown; nor was he regardless of the suffering of other people. He claims to have discovered, in 1842, that ether insensibility was infallible, thorough, and safe, and yet he cared so little for his reputation that he did not publish his discovery; and he forgot his humanity so far, that he allowed patients, the world over, to encounter the agonies of amputation during four years more. This extraordinary circumstance cannot strengthen belief in the fact of the discovery at this early date.

In a long communication to Humboldt, in 1851, and in certain other communications to learned foreign societies and individuals, Jackson lays great stress upon the elaborate mental process which enabled him to construct, in 1842, an hypothesis of insensibility, based upon the distinct functions of nerves of motion and sensation, superficial and deep sensibility, etc. But the more studied and complete this hypothesis, and the more circumstantial the evidence of its careful elaboration, the more remarkable is it that it was laid on the shelf for four years. Without questioning the entire honesty of Dr. Jackson's convictions, it is

[1] It is to be observed here, that, if the patient was intelligent enough to obey instructions, and if Morton really meant to administer air, the patient would have become insensible by asphyxia. (See p. 166.)

safe to say that it is difficult to measure the original strength of any belief which has lain dormant for four years, especially if that belief has since proved to be a valuable truth. Dr. Jackson claims, indeed, to have mentioned his hypothesis to several persons during this interval; but this testimony, if carefully examined, relates chiefly to a narrative of his chlorine gas experiences. In fact, some of Dr. Jackson's immediate family had, during this same interval, in 1844, submitted to painful tooth extraction by Morton, and yet no anæsthesia was mentioned then. The hypothesis seems to have had for several years a precarious existence.

The only striking testimony is that of Peabody, a pupil, who was advised by Jackson to employ ether in having a tooth drawn, but who, after consulting his father, an accomplished amateur chemist, was deterred from doing so by the reputed danger, which Jackson's suggestion did not outweigh. But this advice was a year after Wells's experiments and failure in Boston, and his conversations at that time with Jackson. Whenever tooth-pulling was brought to Jackson's notice, how could he fail to think of Wells's experiments with gas? And who could think of gas as an inebriator, without its co-inebriator, ether? the obvious and only possible conclusion being, that what gas had done, ether might do, namely, sometimes succeed and sometimes fail. I have no doubt that this thought did occur to Jackson's mind when tooth-pulling happened to be talked of, especially after Wells's experiments.

It also occurred to Morton's mind. He knew more of Wells and of his varying experiments than Jackson did, and there is no question that Morton the dentist, oftener than Jackson the chemist, dwelt upon painless dentistry. His business was "mechanical dentistry," making sets of teeth; and he was daily suffering in purse because patients feared to have their teeth drawn. "I will have some way yet," said Morton to Gould, in August or September, 1846, a short time before the discovery, "by which I will perform my operations without pain." "I smiled," replied Dr. Gould, "and told him, if he could effect that, he would do more than human wisdom had yet done, or than I expected it would ever do."

Who that remembers the late Dr. Gould, cautious, accurate, truthful, judicial, the friend and brother scientist of Dr. Jackson, will doubt this sworn testimony? It was Dr. Gould, who, when his wife told him of rumors that Dr. Morton had drawn a tooth without pain, under the influence of something inhaled, exclaimed, "Yes, that can be done; ether will do it." So obvious was the transition from gas inebriation to ether inebriation, from gas insensibility to ether insensibility, in the mind of one who happened to have his attention drawn to the subject of anæsthesia by inhalation.

Morton knew of gas, and of Wells. He was in eager pursuit of anæsthesia. He believed in it. If he knew, also, of ether, he was, in all human probability, on the verge of discovery. Did he know of ether?

It is fortunately established beyond all question that Morton made long inquiries about ether in July, and also, a short time before the October experiments. If we reject the evidence of Wightman and Metcalf on this point, both of them disinterested, accurate, reliable, we must reject all human testimony.

Wightman the philosophical instrument-maker, afterwards Mayor of Boston, narrates a long conversation with Morton in the cars, when moving his family from his country residence to Boston. During this conversation he informed Mrs. Wightman, who asked him who Dr. Morton was, "that he was a dentist who was experimenting upon the relief of pain in dental operations." Wightman fixes the date, September 28th, of this journey, by items of expense charged in his day-book, of which the leaf was produced, as part of his sworn testimony. This enables him to fix the time of several previous conversations with Morton, concerning mesmerism, the effects of the imagination, etc., and especially of one as to whether rubber or oiled silk bags would hold "common ether."

Metcalf, the well-known apothecary, a scrupulously conscientious witness, equally substantiates the date of a conversation half an hour long, about ether, with Morton, who held in his hand a bottle of it which he had brought with him. In this conversation, "entirely about the inhaling of ether," interspersed with anecdotes of exhilaration and insensibility, Metcalf told Morton of "a person to whom he had given it, who was exceedingly wild, and who injured his head while under the influence of it, and did not know, when he got over the influence of the ether, that he had hurt himself, until it was called to his attention." "Morton," Metcalf testifies, "when he went away, knew as much about it as I did, for I gave him all the information which I had." Metcalf sailed for Europe in July, 1846, just after this conversation. While in Italy, in the early part of 1847, he took up a newspaper announcing the discovery, by a Boston dentist, of insensibility through ether inhalation. "I said at once," testifies Metcalf, "that I was sure Morton must be the man, for he was engaged upon ether before I left home; and that I now knew why he had been so curious, and at the same time shy, in his conversation with me." To those who know Mr. Metcalf this evidence has all the weight of personal experience.

There can be no question that Morton knew about ether. How much he knew about it is less important. But it should be mentioned that he claims to have made repeated experiments with it upon animals in the summer of 1846.

Morton's explanation of his professed ignorance of sulphuric ether was this. During the summer, while boarding at Dr. Jackson's, he had heard frequent and protracted expositions of Jackson's claim to the invention of the electric telegraph, then recent, and the important features of which Jackson was satisfied he had communicated to Morse during an ocean passage. Dr. Jackson had, indeed, a well-stored and suggestive mind, which made it more than likely that he had furnished information, of which Morse, while originating and mentally evolving a system of electric telegraphy, may have been glad to avail himself. A sharp public discussion, with pamphlets, ended with a verdict in Morse's favour. In going to Jackson, Morton feared that if there were any deliberate conference, Jackson might set up a similar claim of participation in his own search for painless dentistry, and therefore took the shortest way to exhaust his knowledge for his own benefit, without discussion. There can be little doubt that Morton was in this matter reticent, as Metcalf states, and intended to keep it a secret from his brother dentists. I am also inclined to believe that Morton at first cared little about the abstract question of discovery, and would have willingly left a large share of any honour unquestioned in the hands of Jackson. But when Jackson made a claim upon the patent, and the profits, beyond the amount to which Morton conceived him to be entitled, then he defined his own claim to the discovery. It may be stated in this connection, that the surgeons of the Massachusetts General Hospital, who had no interest whatever in this difference, and could have none, were friends of Jackson, and strangers to Morton. They yielded, when it became necessary to take sides, only to their deliberate conviction of the justice of Morton's claim.

It will be advantageous, at this point, to take a general view of the "ether controversy." For this purpose, I find I can do no better than to quote the following letter, written, when the occurrences were fresh, to the Hon. Robert C. Winthrop, in Washington:—

January 26, 1848.

DEAR SIR: I believe most fully, that Dr. Morton deserves any reward Congress may grant to the discoverer; because, although many people have *thought* that a man could be intoxicated beyond the reach of pain, Dr. Morton alone *proved* this *previous possibility* to be *a certainty*, and *safe*. A diagram will make the matter plainer than words:—

Before October, 1846, who made the suggestion? Here is the only ground of dispute.	Discovery in Oct. 1846. Consecutive experiments by Morton.	After October, 1846, *Morton alone* took the responsibility of danger, and proved that it was 1st. *Certain.* 2d. *Safe.*

The two last points, namely, the consecutive experiments, and their confirmation, *which nobody disputes to Morton*, make him, in my eyes, the discoverer. The only doubt is, who made the *suggestion?* *To me this is of no importance.* Dr. Jackson says, "I did. I told Mr. Morton to try the experiment; and unless I had so told him, he would never have tried it." Dr. Jackson adds: "I first tried ether when I was suffering from chlorine, in 1842. I afterwards recommended it to Mr. Peabody." But Dr. Morton confutes even these positions. He says to Dr. Jackson: "1st. I show, by the evidence of Dr. Gould, Mr. Wightman, and Mr. Metcalf, that I was experimenting with ether before the interview in which you claim to have brought it to my notice. 2d. In 1842 you only re-discovered what was before clearly in print in Pereira's *Materia Medica.* 3d. You claim that you told Mr. Peabody what you *knew* of ether. Now you could not *know* it. You have stated all your grounds of deduction, and the widest inference you could draw from them is, a *suspicion* of the properties of ether; and a *suspicion* in science, an *unconfirmed theory*, amounts to nothing. Finally, what you claim to have discovered in 1842 you kept to yourself during four years. Do you expect the world to believe you knew its value? Do you expect it to reward you for letting people suffer during that length of time? Besides, the suggestion of anæsthetic agencies occurred to Davy; especially was it followed out, though unsuccessfully, by Horace Wells, who, disgusted with failure, abandoned his attempts." These and others had hypotheses as well as Dr. Jackson. Morton alone proved the hypothesis. Without Morton there is no evidence that the world would have known ether to the present day. I believe this covers the ground of important argument and difference in the pamphlets. . . . .

Respectfully your ob't servant,

HENRY J. BIGELOW.

At the interview referred to, Jackson's partisans tenaciously insist that he assumed direction, as a physician might have done, of the administration of a remedy; while Morton acted only as a "nurse." Let us examine, then, the often-quoted "nurse" argument with which the opponents of Morton have endeavoured to handicap him at the outset. Here is its fallacy. A physician, by common understanding, possesses a positive knowledge of the effect of his remedies, in advance of their administration. Such knowledge was impossible to Jackson. Again, a physician is employed expressly to direct, and a nurse to obey. Under these circumstances a nurse is not likely to get much credit for originality, which is in fact absolutely excluded by the terms of her contract. There was no such contract here. Morton was not a mere agent, without preconceived plan, automatic in action, and irresponsible as to results. On the contrary, he was already conducting an independent investigation. He was in pursuit of anæsthesia when he went to the laboratory of Jackson, with whom the subject, even admitting his claim, had slumbered for years. He had been before, and in the same way, to books, apothecaries, instrument-makers, in short, to various usual and available sources of knowledge, as is customary with every investigator or discoverer. The purpose, the investigation, the patient, the discretion, the responsibility, were all his. Morton was not a "nurse."

Morton had a combination of qualities such as few other men in the community possessed. Fertile in expedients and singularly prompt in execution, he was earnest and persevering beyond conception. His determined persistence is remembered even at this interval of time, as having been a terror to his best friends. Nobody denies that Morton, recklessly and alone, faced the then supposed danger attending ether stupor. If all accredited scientific opinion had not been at fault, and in the case of any fatal result, he would have infallibly been convicted of manslaughter, with little probability that anybody would have come forward to say, "The responsibility is not his, but mine."

In fact, Dr. Jackson endeavoured at this time, by word and deed, to keep both himself and his reputation clear of Morton, as a reckless and dangerous experimenter. The only operations under ether witnessed by him during the first three months were on November 21, 1846, and January 2, 1847; and a part of this time he was absent from the State.

"But," it has been a hundred times said, "Jackson made a suggestion, and Morton used it." It is evident that we cannot escape some discussion of the relation of "suggestion" to "discovery," no matter how little the suggestion may be intrinsically worth, or how fortuitous its success. The simple question is, What was the actual value of Jackson's suggestion to Morton at the time it was made—I distinctly mean before Morton had handled it?

A suggestion in science varies in value as much as a suggestion in common life, where advice is not always sound advice. It may, indeed, turn out fortunately, like a suggestion, for example, to wager money on the ace of spades. But because it may prove so, the advice does not necessarily imply merit in the adviser. This is an extreme case. At the other extreme is a mathematical certainty, such, for example, as that twice two make four: a truth the value of which is not afterwards augmented for intelligent people by a material test or demonstration that twice two apples make four apples. A similar example of mathematical certainty is that calculated by Leverrier (liable only to the instrumental fallibility of dividers, telescopes, and equations), which did not become more absolute, nor more true, when Galle saw the planet where Leverrier told him it must be. Mathematics are unerring, while predictions in physiology are as uncertain as predictions about the weather a week hence. Yet it has been argued that Jackson was Leverrier, and Morton Galle.

Jackson's alleged hypothesis, before Morton took hold of it, had only the value of a lottery ticket, which, through Morton's unaided, dangerous, and acknowledged efforts, drew an immense prize—or of "the cast of a die"

—"for in that light," says Watt, whose name is identified with the history of steam, and the soundness of whose practical views no one will dispute, "I look upon every project that has not received the sanction of repeated success." A statement of the grounds upon which this view is based will enable others to draw their own conclusions. Let us begin with Jackson's experiments.

"Having, in 1841–2," says Jackson, "got my lungs full of chlorine gas, which nearly suffocated me, I immediately had ether and ammonia brought to me, and alternately inhaled them, with great relief." The next day, still suffering, and "perceiving a distinct flavour of chlorine in my breath," the experiment was repeated "with perfectly pure washed sulphuric ether," and "with entire loss of feeling." "All pain ceased." He "fell into a dreamy state, and became unconscious of all surrounding things."

In other words, he inhaled ether, until, seated as he was "in a rocking-chair, with his feet in another chair," he fell asleep. So far there was nothing new. Pereira, for example, says (*Elements of Materia Medica*, etc., London, 1839, pp. 210–211):—

"The vapour of ether is inhaled in spasmodic asthma, chronic catarrh, and dyspnœa, whooping-cough, and *to relieve the effects caused by the accidental inhalation of chlorine gas.*" "When *the vapour of ether sufficiently diluted with atmospheric air* is inhaled, it causes irritation about the epiglottis, a sensation of fulness in the head, and a succession of effects *analogous to those caused by the protoxide of nitrogen* (vide p. 156), *and persons peculiarly susceptible to the action of the one are also powerfully affected by the other.* (*Journ. Science*, vol. iv. p. 158.) *If the air be too strongly impregnated with ether, stupefaction ensues.*"

Such was contemporaneous knowledge. Jackson's experience was identical with that recorded by Pereira. And Pereira further distinctly calls attention to the similar effects of inhaled gas and inhaled ether, and to the stupefaction caused by ether.

Pereira adds, in regard to the danger of this condition :—

"In one case this state continued, with occasional periods of intermission, for more than thirty hours; for many days the pulse was so much lowered that considerable fears were entertained for the safety of the patient. (*Op.cit.*) In another case, an apoplectic condition, which continued for some hours, was produced."

I shall revert to this subject of danger.

But Dr. Jackson alleges that he now advanced a step further, and logically inferred the entire safety and inevitable certainty of ether anæsthesia, in all cases, and during the severest surgery. This was an unwarrantably wide inference, as we shall see.

Further still, Dr. Jackson claims to have invented a method upon which the success of his hypothesis largely, as he supposed, depended; and he offers this method as evidence that he made the hypothesis and the discovery. It is plain that a discovery may be in this way rendered more probable. Whoever is in possession of a new method is more likely to find the way to a new truth. But if the alleged method proves to be partly erroneous and partly an old and familiar matter, then the proof of alleged discovery is no stronger because of it. In fact, just so far as the hypothesis was apparently stronger by reason of the method, it becomes weaker when the method falls to pieces. Let us, then, examine this alleged method.

Dr. Jackson has from the first insisted upon two points, as peculiar to his invention, and essential to his discovery. By these safety and certainty are secured, and a neglect of them explains previous uncertainty and danger. They are,

1st. Purity of the ether;

2d. An admixture with it of atmospheric air.

If these conditions are either not essential or not new, we find ourselves, at least so far as method is concerned, where Pereira left us.

From time immemorial, the familiar way of inhaling ether has been with air from a handkerchief. Pereira, as before quoted, distinctly stipulates for air. Says another writer, "Animals breathe oxygen. Without oxygen a man must die. Ether would not have saved Desdemona." This needs no ghost for its enunciation. But the fact is, in etherizing by a sponge or cloth, the difficulty is as often to exclude air enough as to admit air enough. Some good authorities maintain that there is advantage in its exclusion—that the insensibility from gas, for instance, is due to asphyxia. Even in etherizing, they aim to take advantage of this condition by restricting the air supply. The French ether-bag, still in use, is expressly arranged for this purpose. In short, while it requires especial effort to exclude air, partial asphyxia is not dangerous; and a claim to the discovery of the safety or certainty of ether stupor based upon the admission of air cannot stand.

A claim based upon pure ether is equally void. Tolerably pure ether is better, but by no means essential, for safe insensibility. The specific gravity of pure ether is not very far from 0.718, and is difficult to attain; that of our usual inhaling ether of Powers & Weightman, 0.724; and of Squibb (*fortior*) about the same. The ether of the old Pharmacopœia, and of the shops in the year 1840, was not far from 0.750; and this is a very practicable anæsthetic. Its slight adulteration with alcohol and acid is not especially deleterious, when inhaled. In fact, anæsthesia resembles dead-drunkenness, which is equally possible with brandy, or with brandy and water. The real danger was not from impure ether, but from over-inebriation—a danger which exists to-day with the best ether.

The discovery was not in the admission of air, nor in the use of a particular quality of ether. It was, that the inhalation of ether, which had been familiarly resorted to for years, could be prolonged beyond the usual stage of exhilaration to a stage of stupor, possessing the *complete* insensibility of a dangerous coma, yet, unlike that condition, *safe;* and that all this could be effected *in every case.*

The history of ether anæsthesia was the gradual discovery of the following facts:—

1st. That ether inhaled produced, capriciously, in certain instances, unconsciousness. (Old.)

2d. That it possessed the power of producing stupor in every case. (New.)

3d. That this stupor could be exactly graduated as to time. (New.)

4th. That it could be increased or diminished, and arrested short of danger, and so made safe. (New.)

5th. That there were certain infallible indications of danger. (New.)

6th. That, while thus controllable and safe, it could be made at will so profound that even amputations should not be felt. (New.)

All that is new belongs to Morton.

A person inhaling ether from his handkerchief would drop it when his hand and senses were benumbed. As air entered the lungs he would revive. Such was Jackson's self-experiment, already covered by Pereira. But a second person, who should now seize the handkerchief and compel the continued inhalation to the stage of stupor, might obviously make a valuable study of this new ground. He could draw his patient's tooth, or amputate his leg, and thus measure the insensibility. He could repeat the experiment until satisfied that insensibility could be always attained, and that it was safe. All these experiments Morton tried upon others, and when he had tried them the discovery was made.

Jackson virtually claims that his inference extended to the *universal efficacy*, the *completeness*, and the *safety* of the stupor.

Of the *universal efficacy* of the new stupor Jackson could have no valuable opinion. Such knowledge was possible only through observation of many cases, with an experience which could say, "Administer ether as for exhilaration. Protract its inhalation beyond this usual stage, and you will inevitably reach an ulterior condition of stupefaction. During this process a patient may struggle with a giant's force; or perhaps will tell you of his extreme distress; or after five or ten minutes of inhalation will satisfy medical bystanders, as patients sometimes do now, that he is not amenable to ether. Another will assure you, with apparent reason, that he is dying; or, livid with asphyxia, will compress his lips and cease to breathe; or may, for a long time, alternate between lividity with a failing pulse, and arterialization with a partial return of consciousness.[1] But, through all these apparently alarming indications, urge the process discreetly, and you will ultimately and inevitably reach a stage of stupefaction."

Dr. Jackson could not say all this, because he knew nothing of it; nor did anybody else, until Morton established the fact that there was no exception to the potency of ether. These and other contingencies, once startling, still occur with the best ether, and the experience of a quarter of a century. Had Morton been a timid or a discreet man, anæsthesia might have been delayed beyond the present generation. Morton compelled inhalation in spite of indications to arrest it, incurred the responsibility of doing so, and is entitled to the credit.

The completeness of the insensibility—its adequacy, for example, in amputations—was settled only by repeated observation, with varying results, but gradually accumulating evidence: from the tooth first painlessly extracted, through several failures to comparative success in dentistry; then the removal of a venous tumour of the jaw, where the patient was doubtfully insensible; then a fatty tumour of the arm, with complete insensibility; and finally, after an inconsiderable operation or two, the amputation of a leg, practically successful. Everybody awaited this final test by amputation, and then only was the accumulated evidence deemed conclusive. It was, indeed, beginning to be felt that the process was a safe one, and that it promised satisfactory results; otherwise the hospital surgeons would not have permitted this test of it in a patient weakened by disease. But this experiment, like the rest, so far as anæsthesia was concerned, was Morton's, and a part of his well-organized enterprise to investigate and establish the new insensibility. As this amputation has been repeatedly published as mine, it should be stated that it was performed by the late Dr. Hayward. That anæsthesia was employed at all on that occasion

[1] Such patients are familiarly known to our hospital attendants as "bad etherizers." Some of them are inebriates. If they return for a second operation, they seldom fail to exhibit the same symptoms as before.

Alice Mohan has to thank me; although, if anæsthesia had not been employed in this particular instance, the test of amputation would have been delayed only till another should occur. The circumstances may illustrate the sort of obstacles Morton encountered. The looked-for opportunity had arrived; but, through various antagonistic influences, Morton, in spite of his earnest request, had been notified that ether would not be administered, and that he need not attend the operation. Of this he informed me the night before, and by arrangement with him I carried him to the hospital the next day, just before the operation, there to await events. A dose of laudanum had been administered, and Alice Mohan was carried to the amphitheatre, for operation without ether. I there strongly urged the employment of the yet ostensibly secret agent; partly on the ground that it then was really known, but especially from the consideration that humanity ought to supersede any doubts connected with professional etiquette. This and other considerations prevailed, and, after a delay of half an hour, Morton, whose presence had been till then unknown except by me, was brought up, and the patient was etherized.

The evidence of all this slowly accumulating anæsthetic surgery, at which Dr. Jackson was not present, was claimed for him in virtue of the "nurse" argument. What light could a repetition of the chlorine experiment of Pereira throw upon the question whether a patient could sleep during an amputation?

Ether exhilaration was familiar; but, on the other hand, it was well understood that ether stupefaction was in certain cases dangerous. Physiologists had also found that the smaller animals very frequently died of it. Brodie wrote of the fresh discovery: "I have heard of this before, and had tried it on guinea-pigs, whom it first set asleep and then killed. The great question is, Is it *safe?*" This was, indeed, the question. Could danger be avoided? what was its exact character? and what were its indications?

On the second of January, nearly three months after the discovery, Jackson came to the hospital for the first time. To that date, he had been present at one operation only, that at the Bromfield House, November 21. He was not cognizant of current experiment, and brought voluntarily a bag of oxygen, which he urged upon the hospital surgeons as a necessary precaution against danger, erroneously supposed by him, at that late date, to be asphyxia, instead of over-inebriation, of which the essential indication is the pulse. It was some weeks after the discovery, that this source of danger, and its sign, were understood; and in the mean time Morton might have killed anybody. A patient was, in fact, in great danger from over-inebriation at the first private operation. He was inhaling, in the continuous way that was at first supposed to be essential to protracted insensibility, through a glass globe of ether, and long after insensibility was manifested. The operation was far from completed, when a bystander happened to feel the pulse. There was no special reason for doubt, inasmuch as the patient was, in general appearance, like all former thoroughly etherized patients. The pulse proved to be barely perceptible, and the patient to be etherized almost beyond recovery. The bystander, after repeated observation of other cases, published the fact, then first observed, that in ether anæsthesia the pulse stood as a beacon between safety and danger, between harmless inebriation and fatal narcotism. This was the discovery that ether was not dangerous; because this showed that its danger gives warning, and is under control. The operator was Dr. Dix, the bystander myself, and the discoverer Morton. To his impetuous, unremitting, reckless experimentation to establish anæsthesia, surgeon, bystander, patient, ether, and apparatus, were all for the time, and in that relation, subordinated. Morton had asked me to be present, because I was more familiar with the new process than anybody except himself, and for the purpose of aiding him, in emergency, with professional advice. But the anæsthesia was his. I assumed no responsibility. Had the patient died in a "stupor," as he might well have done, Morton was liable; and as the patient did not die, his was the credit. This was real danger. But there was other danger, more startling, though only apparent; such as prostration, "trance," or "mania," lasting for hours, and for which Morton was in one instance threatened with prosecution. What was Dr. Jackson's responsibility? None whatever. He was then absent from the State. What had Pereira's chlorine experiment taught him about all this danger? Absolutely nothing; nor could it do so. And he could impart nothing.

In view of these facts, which leave Jackson standing upon the naked experiment of Pereira, we may fairly pause, and ask, What, in the fullest sense, were the exact significance and value of the suggestion made by Jackson to Morton?

Caleb Eddy says, in his sworn testimony, "I said to Dr. Jackson, 'Dr. Jackson, did you know, at such time, that, after a person had inhaled the ether and was asleep, his flesh could be cut with a knife without his experiencing any pain?' He replied, 'No, nor Morton either: he is a reckless man for using it as he has.'"

Waiving this testimony, it is clear that the whole of the peculiar knowledge embraced in the suggestion of Jackson to Morton would have been accurately and fully conveyed in the two words, "Try ether." This suggestion should be fully credited to Jackson. Morton never questioned the fact of the suggestion; on the contrary, he at once proceeded to square the account. Jackson at first distinctly agreed to receive five hundred dollars, as full compensation for the assistance he had rendered. As the evidence grew, and the greatness of the discovery became more apparent, Jackson raised his demand to ten per cent. on the sales of patent rights. Later, when it was clear that the lottery ticket had drawn an immense prize, Jackson again increased his demand to twenty-five per cent., which Morton refused. The controversy was then opened.

I very early urged the inexpediency of a patent, if only on the ground that, like Whitney's cotton gin, for example, this invention was so valuable that the world would combine, as the event has shown, to take possession of it; and that the question of equivalent might safely be left to public generosity, which has generally recognized such debts. Ether anæthesia was at first opposed on the ground of its danger, of quackery, of religion, and of professional etiquette. Much of the early opposition to a patent[1]

[1] The first statement of the fact of operations under insensibility was a paper in the *Boston Medical and Surgical Journal*, Nov. 18, 1846, entitled "Insensibility during Surgical Operations, produced by Inhalation. Read before the Boston Society for Medical Improvement, Nov. 9, 1846, an Abstract having been previously read before the American Academy of Arts and Sciences, Nov. 3, 1846, by Henry Jacob Bigelow, M.D., one of the Surgeons of the Massachusetts General Hospital." A copy of this was sent by a gentleman to his friend, Dr. Boot, of London, and by him transmitted to several London surgeons. Their replies to Dr. Boot I have in my possession. This paper also announced the patent, and the connection with it of the names of Morton and Jackson.

came from dentists, who desired to use the new method without pay; and they confused with it the question of humanity to suffering. But in those days dentists had secret methods to which they attached a money value, and which went far to justify both secrecy and patent. The question of secrecy should, however, be detached from that of equivalent. Dentists and physicians, lawyers and clergymen, dealers in food, heat, light, labour-saving methods, in short, in comfort, knowledge, and value, rightly exact a pecuniary equivalent, proportioned to their services, from those who can pay it without inconvenience. The more distinctly this is recognized, the better we shall understand the nature of real humanity, and the more readily lend assistance and charity to those who need them. Jackson was right in expecting a money return for the service he had rendered. The only question here is, How much he himself, at first, considered a fair equivalent for this service. This has been shown.

After it became evident that the patent was worthless, Jackson repudiated the division, and claimed the whole discovery, in virtue of the "nurse" argument. Under these circumstances Morton properly insisted that the "suggestion" could have been picked up from almost any source, by any man actively searching for painless dentistry, who knew everything about Wells's experiments with gas, and who also knew the familiar and similar action of gas and ether. Morton was right.

But Morton also urged that this was a discovery not in science, but in art; that surgical anæsthesia was due, not to any great scientific novelty in the long recognized ether insensibility, but to his having worked out the application of this insensibility to use in art, with enterprise and perseverance, through many details, in the midst of danger, till he gave to the world a perfected system of efficient and safe anæsthesia. Morton was again right.

When a discovery is great, not from the intellect invested in it, but because it ameliorates the condition of mankind, then its recognition has more of a business character, and the gratitude and honour bestowed by the world are more nearly an equivalent for value received. The world does not concede the equivalent until it has received the value; and it is apt to examine with business exactness the claims of those who profess to have acted as agents in the matter.

This suspicion of discoverers who do not appear until after the world has been made to recognize the discovery is justified by the fact that hardly an invention of importance was ever made known that it was not at once claimed, often simultaneously, from a variety of sources. This is not remarkable. The world, whether in science or in art, is built up to a certain point by the easy and wide transmission of knowledge. Upon this elevation stands a multitude of philosophers, engaged often in identical researches, and possessed of much information upon the subject in question. Each of these may honestly believe that his imperfect knowledge is the perfected knowledge in question. In such a case the world is liable for a short time to confound claims, to confuse the incomplete result of a few data with the completed demonstration from many, the unproved with the indisputable, theory with fact. Recognize this fallacy, and the question of invention is comparatively simple. Yet it is not recognized. There is at this day the same claim to priority in invention as existed half a century ago. The writer of a *Life of Fulton* then said: "Those who question Mr. Fulton's claim are precisely those who have been unsuccessful in their own attempts; and it would seem that exactly in proportion as their efforts were abortive, and as they had thrown away money in fruitless experiments, their claims rose in their own estimation and that of their partisans." The witness—I believe before the House Commons—probably did not overstate the matter when he gave it as his opinion, that, if a man were to show that he had found a road to the moon, his neighbours would testify, that, if they had not been there themselves, they knew several persons who were familiar with the road in question. It is hardly too much to say, that, at the present day, every invention or discovery having a large supposed value is systematically contested.

Morton, in his attitude of investigator, had a right to receive a hint or suggestion of greater solidity than that of Jackson, without impairing his merit as inventor. Every invention is preceded by such hints or suggestions derived from experiment, books, or people. Curtis (on Patents) says: "It is clear that many suggestions may have been made, or many hints taken from others, without invalidating the claim of a party to be considered as the author of the invention."

Of a hundred instances easily cited to illustrate this, let us confine ourselves to a medical one, that of the invention of vaccination to avert smallpox.

The young countrywoman of Sodbury said of smallpox: "I cannot take that disease, for I have had cowpox." The Duchess of Cleveland, when Lady Mary Davis and other companions taunted her as likely to deplore the loss of that beauty which was her boast, as the smallpox was then raging in London, said that she had no fear about her beauty, for she had had a disorder which would prevent her from ever catching the smallpox. Were these discoverers? Surely, yes, if Dr. Jackson was one. In fact, the hint that they and others gave to Jenner in the vale of Gloucestershire, where he resided, embodied more knowledge of vaccination than Dr. Jackson's suggestions did of ether anæsthesia.[1] But neither the milkmaid nor the Duchess of Cleveland was ever honoured as a discoverer of truth, or an inventor of method, while Jenner was so honoured. The reason is obvious. They, like Dr. Jackson, asserted a doubtful fact, and had neither time nor inclination to pursue the subject; but Jenner, by multiplying instances of the cowpox inoculated like smallpox, which was already supposed to be, like smallpox inoculation, protective, conclusively proved that it was thus protective, and also safe. He did with cowpox what Morton did with ether.

The parallel in this case is very close. Jenner and Morton were in pursuit of what the scientific world regarded as a chimera. Because they believed in its possibility they encountered prejudice and opposition. They both received from others a hint, suggestion, or surmise, which afterwards proved to be capable of development into a truth of great value. This suggestion was based on narrow induction, and had, therefore, obtained no previous general credence. It had also slumbered for years at that stage of its development. The world had not believed it.

Jenner and Morton, both men of singular persistence and perseverance, took hold of this idea, of which, from the nature of their daily pursuits,

[1] The experiences of the milkmaid and the Duchess might easily have apprised them of their own immunity from smallpox. But Jackson, through his chlorine experiments, could have no evidence that ether stupor was capable of affecting all persons, or that it gave immunity from real surgical pain, or was free from danger. No self-experiment could touch these points.

they felt the immense value. But for them it might have slumbered indefinitely. They dragged it through till the world recognized it and them. This they did at the risk of injuring people's health, of killing them, and of being held responsible for so doing. Nobody has ever doubted that Jenner was the inventor of vaccination, and nobody should doubt that Morton was the inventor of modern anæsthesia. Here the parallel ceases. The English people voted Jenner a reward of $150,000.[1]

The world demands convincing demonstration—and not by an individual for himself or to himself, but to them and for them, with overwhelming clearness. Then, and not till then, it responds with acknowledgment, concession, or gratitude.

Sydney Smith, in the *Edinburgh Review*, insists on this. In fact, he wittily overstates the claim of the mere publisher of a novelty, when he says that "he is not the inventor who first *says* the thing, but he who says it so long, loud, and clearly, that he compels mankind to hear him."[2]

[1] In decisions relating to discovery, unanimity is not to be expected. There can be none where partisanship and large interests are involved. The question, then, is, Where the weight of evidence lies. Even Jenner, with no rival, encountered great hostility both to himself and his discovery. The House of Commons (June 2, 1802) voted him ten thousand pounds by a vote of 59 to 56, a majority of three only. A further sum of twenty thousand pounds was voted (June 29, 1807) by a vote of 60 to 47, a majority of thirteen. Morton's award of $100,000, for his patent, passed the Congressional Committee. It was arrested, not wholly by Jackson, but by the partisans of Horace Wells, who published what afterwards filled an octavo volume, containing little argument, but full of bitter invective against Morton, and promised, if only delay were granted, to make a conclusive case for Wells.

[2] It is easy to understand what is meant here. For instance: I have amputated more legs after applying a tight bandage from foot to hip before tightening the tourniquet, than in any other way. Similarly to the arm for a needle in the hand. A hundred surgeons have done the same thing. Sir Charles Bell went a step further, and announced the dry method in print. "I may here observe," he says, "that by the management of the tourniquet, blood may be lost or gained. If the limb be uniformly rolled before amputation, the veins are emptied into the general system, and blood is saved instead of being withdrawn. In a very exhausted state of the patient, it may be of service to attend to this." (*Great Operations of Surgery*, London, 1821, p. 58.) But Esmarch was so impressed with its importance, that he erected it into a system, and urged it upon the attention of every surgeon in the civilized world. To many the idea was new. In fact, to a considerable part of the surgical world Esmarch was the discoverer of an important truth. He deserves to be avowed as such, in requital of his pains to perfect and to publish to the surgical world, a useful point, new to many of them. He is so recognized.

Antiseptic precautions in surgery are not new; but Lister published his views, as did Esmarch. Whether germs are essential to the theory or not, there can be little doubt that it is well to free a wound from coagula. and to wash it out with diluted carbolic acid or its equivalent. Then the subsequent free use of the antiseptic, by hindering decomposition without, tends to maintain vitality within the wound. These views have been enforced and brought home to the world by Lister, with the pertinacity of Jenner or of Morton. The world at large owes its attention to these points to Lister's announcements, and properly attaches his name to the antiseptic method.

Mere publicity is notoriety; but with merit it is fame; with originality it is discovery. To all these publicity is essential. He who keeps his discovery comparatively to himself discovers, or uncovers, nothing. If he claims to have done so, the world will scrutinize and suspect his claim.

The act of publication, indeed, adds little to the claim of Morton; his impregnable position being that he elaborated and perfected a new art. But facts like the above go far to show that the failure of Jackson for four years to publish his alleged guess, of itself extinguishes any claim to its recognition during that time.

Thus put, however, the statement throws light on Jackson's prominence after the discovery. An unproved hypothesis in physiology, which was his whole claim, would usually be considered of little account. But Jackson knew the machinery of fame. As Wells by accident, so Jackson by his scientific relations, and through his friend and former teacher, Elie de Beaumont, got at once possession of the scientific rostrum of the French Academy of Sciences and other foreign learned societies. He also subsequently addressed Humboldt. He thus compelled the European world, even so far as Turkey, to listen to his exclusive statement that the whole was his. In the mean time, however, there was in Boston a scientific jury of the vicinage. When Morton's statement at last crossed the water, the French Academy did what it could at that late day, and awarded to Morton the same honour and recognition it conferred upon Jackson. It could do no less. It could then do no more. But had the case been at the outset reversed, and had Morton made a suggestion to Jackson, does anybody doubt that the humbler Morton, and his suggestion, would have been by scientific precedent wholly absorbed and assimilated ?

All honour, then, to the inventor of the art of anæsthesia!—for there was little science in it. He found the practice of ether inhalation an amusement of chemical lecture-rooms and schools; he left it the sovereign anodyne of the human race in its moments and hours of agony. He found ether stupor as hazardously uncertain as was the narcotism produced by pouring down the opium "*à boire*" of Canappe; he left it as manageable and safe as the sleep that follows a dose of laudanum.

There is hardly an inhabitant of the civilized world but can remember some one of those nearest to him in whose experience the anguish of the knife or of disease, of birth or of death, has been assuaged by anæsthesia, perhaps converted into a pleasant dream. Yet he is willing to take this priceless boon as a gratuity from those whose sole patrimony it was, and who have been brought nigh to want that he might enjoy it. If the world should cancel a fraction of its debt, the family of the inventor could afford to be generous to the families of his former friends, who, without impairing his title to the discovery, contributed to his success.

Wells's sad story has been told. Morton fell with apoplexy, induced by a publication in behalf of Jackson, of a nature to prejudice a subscription then arranged in New York for his benefit. Jackson, it is to be feared, is at the present time hopelessly bereft of reason.