Again, looking back on the early meetings of the Association, we find how beneficial were its effects on the subject of vaccination, on the medical relief of the poor, the registration of disease, and its advocacy of a national organisation for the promotion of social science. If time admitted, much might be said as to what has been done by the Collective Investigation of Disease so well calculated to settle many disputed points, and establish our knowledge of disease on a firm basis as well as upon many other subjects, but I must hurry to a close.

In conclusion, I desire to defend our Association from a charge which has been brought against it, and others of a cognate character, and that is that we meet chiefly, if not altogether, for amusement and recreation. To this we may give an emphatic denial, for labour takes the first place, and then come recreation and social intercourse, the value of which, we are free to admit, we highly appreciate. Virchow, in his address at the last meeting of the Association of Naturalists and Physicians at Berlin, well observed, after pointing out that the greater part of the time at their disposal should be devoted to earnest work: "Aber nach der Arbeit ziemt es sich wohl der Erholung und dem freundschaftlichen Verkehr ihr Recht zu lassen." The original statutes of this Association, now sixty-four years old, laid it down that the chief object of the meetings was to make the members personally known to each other. No one proclaimed this with more force than Alexander von Humboldt in opening the meeting fifty-eight years since. He said: "Der Hauptzweck dieser Gesellschaft ist die persönliche Annäherung derer welche dasselbe Feld der Wissenschaft bearbeiten." These are the words of the renowned author of the *Cosmos*.

In conclusion, I beg to reiterate my thankful acknowledgments for the high honour which has been conferred upon me, and to utter an earnest, heartfelt aspiration that the meeting which I have opened this day may tend to bring together more closely, and harmonise into one friendly communion the scattered members of our great profession.

ADDRESS IN MEDICINE,

BY

W. T. GAIRDNER, M.D., LL.D. Edin.,

M.D. (*honoris causa*), T.C.D.

Physician in Ordinary to Her Majesty the Queen in Scotland; Professor of Medicine in the University of Glasgow.

HAS THE ART OF MEDICINE ADVANCED DURING THE PRESENT CENTURY?

MR. PRESIDENT AND GENTLEMEN,—The importance always attached to the Address in Medicine in this Association has given me many a qualm of conscience as regards undertaking it; for I am in the position of a man fairly worked out of late years in the matter of addresses, having been obliged officially and otherwise to give more of them during the last half dozen years than most men, or even most university professors have to do in a lifetime. But I admit that it is not unreasonable on your part to desire to listen to the opinion, such as it is, of one who has been engaged in teaching the art and science of medicine from youth to a reasonably advanced age, upon the doctrines and practice of the art during the years he has been so at work. This desire I shall endeavour in some measure to satisfy; in some measure, I say, for if you expect from me anything like a *catalogue raisonné* of medicines or of processes, or even of methods of healing, such as they are among us at the present time, you will certainly be disappointed. I shall not even attempt such a catalogue, interesting as in some respects the task might be, for even the attempt would necessarily wear out your patience, while it would leave me open at the end to the charge of having omitted here or slighted there, or unduly enlarged upon in another place, something in which respected friends in this Association might have a special interest, *pro* or *con*. I am not willing to incur such a responsibility, even if I felt able for the task; indeed, anyone who has attempted to deal with the subjects included in a course of practice of medicine from year to year, in presence of students of medicine, knows better than most how disastrous it is to attempt to convey into a single address more in the matter of details than the allotted number of minutes will reasonably hold. And I feel, too, that from a teacher, if from anyone, you have reason to look for something more interesting, if not more useful, than such a *catalogue raisonné*. He has been in the habit, from year to year, of surveying officially the healing art as a whole. He

has watched the rise and fall of doctrines, of methods, of tendencies and currents of opinion. He has come, or ought to have come, to see that there has been an evolution of the art going on in his time; an evolution not at all the work of any one man or of any one school, but rather of what Matthew Arnold and the Germans call the *Zeitgeist*, the spirit of the age. Can he at all seize upon those aspects of his subject which most reveal that inner spirit? Can he expound them so as to give a vivid conception of whither we are tending, or how, and by how much, we differ from our fathers? This, at least, seems to me to be the possible theme of an interesting address, though I am very far from believing that I shall be able to do it justice to-day. I only desire to show you what it is in my mind to set before you; what I would willingly sketch out for you as an artist blocks out his picture in a first sketch, for it can be little more. And if I can succeed in any measure even in this, it will have one great advantage for you and me; that upon the basis of the sketch thus presented each of you can fill in details according to his own special knowledge or field of experience. The field, indeed, is vast, as anyone must be fully aware who has perused with attention even one thoroughly modern work on the practice of medicine, or, still more, any considerable portions of the vast encyclopædias of Ziemssen, or Dechambre, or Jaccoud, or Eulenburg, or even the less bulky collections, comparatively speaking, in English of Reynolds or of Quain. Are there any ruling principles at all throughout this vast field, or is it a mere aggregate of laborious investigations guided by no common purpose, no fixed or polar star? We recoil instinctively from this conclusion, and yet it must be confessed that the various doctrinal systems of the past, and not a few of the gross charlatanisms of the present, have had a seductiveness for the public, and even for some minds in the profession itself, founded mainly on the notion that here, at last—namely, in the particular formula or perhaps exclusive remedial method advocated—is to be found a ruling principle, a law or compendious statement of doctrine intended to illuminate every corner of the art of medicine; hence it follows as a corollary to minds so preoccupied, that we, *physicians* as we call ourselves in English and in the very old French of the thirteenth century—that is, naturalists or observers of nature—are bound to bring all our accumulated stores of knowledge into court, and make them all do homage to this one doctrine, one law, thus set forth as the only true law of the healing art. I need not now illustrate this remark by any of our old exploded controversies. Every one of you, disciplined in the paths of genuine medical science, knows that to such a universal proposition, when made, there can be only one answer. No such exclusive or single principle or law of the healing art can be said to exist. The action of remedies, the resources of hygiene and of preventive medicine, refuse to submit to any formula comparable in its simplicity with that of the law of gravitation. The healing of disease, in other words, is still largely empirical, and what is more, takes no shame to itself in being and remaining so. It is guided and moulded, every day more and more, by advancing science, but it remains in the end only the application of a more carefully instructed experience. Rational and empirical medicine are not opposed to each other, as one distinguished teacher in my own early days used to represent them. They rather interpenetrate each other more and more every day and every year, and each is the better and the sounder because of this interpenetration.

A CURIOUS INQUIRY—HIPPOCRATES.

In reflecting over this address, even so far as these introductory sentences, I have been led to think of another "Address in Medicine," delivered before this Association just twelve years ago (*JOURNAL*, 1875, vol. 2, p. 164), by one who was then at the head of his profession in Scotland—my late dear friend, Dr. Warburton Begbie. Some of you who heard that address may have been struck by the almost grotesque character of a question imported into it from the writings of a very great philosopher—a question to which Dr. Begbie attempted a reply at the time; the immediate result both of the question and of the reply being to set me a-thinking, to what effect you shall presently hear.

"Has the *practice* of medicine (that is, the art as distinguished from the science) made a single step since Hippocrates?" Such, alas! was the doubting, almost mocking, query suggested by the late most eminent Sir William Hamilton, Professor of Logic and Metaphysics in Edinburgh University, in an article reviewing Cullen's writings, published in the *Edinburgh Review* in 1832, and afterwards reprinted in a volume of *Discussions* published in 1853, not very long before the author retired from public duty with shattered health. To this question, thus propounded in cold blood by a distinguished and marvellously learned authority, Dr. Begbie then addressed himself

with a quite wonderful amount of Hippocratic erudition, and certainly with great skill and tact, contrasting modern with ancient practice. I was much instructed by his answer at the time; and yet it would scarcely be correct to say that I was wholly satisfied with it. The interval which he endeavoured to bridge over, from 400 B.C. to 1875 A.D., was quite too enormous to be so dealt with. A fair case was made out on behalf of certain modern inventions and improvements; but the history of progress, on the whole, was not so clearly displayed; perhaps, indeed, could not have been at all adequately displayed, after the manner attempted. Hippocratic medicine, after all, and nineteenth century medicine are not comparable quantities. We still use not a few of the old resources and tools of the art, and have acquired a still greater number of new ones; but we have no details as to the actual use made of these resources in ancient times; no statistics, no accurately-kept hospital records, no scientific basis, so to speak, by which to test the question of the Hippocratic practice in detail, as compared with our own. Nothing is more difficult, perhaps, even in our own day, than to attain to positive conclusions in regard to the behaviour of the same disease under different modes of treatment. How then shall we even attempt to compare, as regards its absolute success, the cure of acute rheumatism by salicin, or of chorea by arsenic, or of syphilis by mercury and iodine, or of aneurysm by iodide of potassium or by Tufnell's method, with anything that we can read of in the works of a Greek, or even an Arabian, physician? Dr. Begbie was most kindly disposed towards the ancients, and had no difficulty at all in showing that they had worked marvellously well, and had anticipated us of this century in several of the inventions and observations that we are apt to claim as our own. He was also most faithful in expounding many of the claims of the newer medicine to therapeutic success; and no one certainly of his time was in a more advanced position, or was better fitted by personal experience, to give effect to the claim. But I think the outcome of the whole was scarcely that of an effective answer to Sir William Hamilton's mordant suggestion. It was much more easy to show that we knew more, than that we cured better, than Hippocrates and Galen. I never had, and have not now, any doubts on the subject, but if I had gone to work in the spirit of Sir William Hamilton, I think it would not have been difficult, after the manner of the *advocatus diaboli* before the Roman Curia, to have made out something like a case in answer again to Dr. Begbie. And this because, as I have already said, we really know almost nothing as to the degree in which, or even the circumstances in which, the physicians of the Hippocratic era successfully grappled with the diseases of the people at large. We know something, indeed, about the doctrines and methods of one or two of the greatest of them, but we know almost nothing of a secure and well-established kind as to how far the influence of these great men extended, or was applied in the service of sick humanity in general. Within a century or two after Hippocrates, we know that the whole of his compacted doctrine had, as it were, been blown to the four winds in a multitude of discordant sects; and it was not till the great and eclectic system of Galen in the second century of the Christian era gathered up the fruits of this dispersion into a body of doctrine which held its own, almost unquestioned, throughout what we call the Middle Ages, that the art of medicine can be said to have had even any appearance of stability at all. Certainly anyone who may have lived at any great seat of medical learning, or of medical practice, in the time of Celsus, or of Cælius Aurelianus, must have had not a few reasons for putting Sir William Hamilton's question to himself. He must have found the opinions and the doctrines of the great Greek master of the age of Pericles very often disparaged, and his most cherished principles and remedies often discarded; cut-and-dry systems, like that of the so-called Methodists, founded on the *stricturem* and *lacum*, as a cause for almost all diseases, often occupying the field, together with numerous bold innovations in treatment, not less out of accordance with rigid or orthodox Hippocratic tradition than those that have become known to us in the present day. It was given to Galen to build up again the fame of Hippocrates; and had it not been for the overwhelming power and authority of Galen's immensely numerous writings, as well as the really pious regard which he always showed for the older and (I think) greater man, and the valuable commentaries he wrote upon many of the Hippocratic treatises, it is very doubtful whether the original father of the Greek medicine would have come down to us as anything much more than *nominis umbra*. For this, even if for nothing else, Galen would deserve the thanks of every student of medical history, and every admirer of one of the most transcendent names of all antiquity—the Æsculapian priest who first emancipated the art of medicine from the trammels of priestly control on the one hand, and of philosophic dogmatism on the other.

CULLEN AND HIS AGE.

Now, gentlemen, it occurred to me at the time of Dr. Begbie's address that a less ambitious programme—one, indeed, not precisely in the terms of Sir William Hamilton's suggestion, but nevertheless including or comprehending what is most important in it for us—might be devised, which would give an opportunity for surveying at leisure some of the landmarks of medical progress, not during the twenty-three centuries since Hippocrates, but during the one century that has elapsed since William Cullen, Professor of Medicine first in Glasgow and then in Edinburgh University, was at the height of his fame. This theme I was so bold as to propound in a few words hastily prepared¹ and spoken the very day after Dr. Begbie's address. It is this theme to which I propose to revert on the present occasion, so far as the time will allow, and as my own slender capacity for dealing with it will permit.

The reasons in 1875 for adopting Cullen as our traditional starting-point lay on the surface. First of all, we were then meeting in Edinburgh, the scene of his longest activity and of his most eminent fame as a practitioner of the healing art. Secondly, it was in reference to Cullen, and in reviewing his works, that Sir William Hamilton was led to raise the philosophic doubt referred to—namely, as to whether the art of medicine had made a single step since Hippocrates. Regarding Cullen, therefore, as the inheritor of the Hippocratic tradition in his own day, it might fairly be argued that, if it should appear that medicine had advanced in any considerable degree since Cullen's time, *a fortiori* the art might be said to have made, not one, but several steps in advance since the remoter era in question. Thirdly, there could be no doubt at all that Cullen was in fact, more than anyone in these islands—perhaps more than any European physician since Hoffmann and Boerhaave—the inheritor of the Hippocratic tradition; and that he had connected this with the practical and theoretical activities of his own time with remarkable insight and success. Fourthly, I was myself, as it happened, the occupant of the chair of Cullen—not, indeed, in Edinburgh, but in Glasgow, where he began his great career as a teacher of the medical art, leaving it for Edinburgh in 1756. This last particular I mention, of course, not as having any public interest, but as having a certain influence in determining the line of my reflections on the occasion of my own very brief address in 1875.

Bear with me, then, while I so far repeat myself as to quote for you only a very few sentences from that brief address; the whole text of which may be found by any curious investigator in the JOURNAL for August, 1875, p. 200.

"It is impossible for anyone, being in Edinburgh, and inheriting the traditions of the Edinburgh Medical School, not to associate Cullen with the *genius loci*, of which, indeed, he was eminently a representative man. The fame of Cullen was, moreover, world-wide, and extended beyond medical circles; he was a distinct and noteworthy personage in that literary and scientific society which included Adam Smith, David Hume, Robertson the historian, and Black the distinguished chemist. Cullen had, by reason of his accomplishments, something in common with all of these and all the other eminent men of that time, and was highly esteemed by them all. In point of medical experience, he was at the very head of his profession; a most distinguished clinical teacher, constantly associated with his students in the observation of hospital cases, and, besides this, engaged in private consultations which brought him into extensive correspondence (as his manuscripts in the College of Physicians show) with all parts of the country. Now, one of the most remarkable facts in the history of the epoch of Cullen is one which was not alluded to by Dr. Begbie, but which, nevertheless, might be made available in discussing, or in reflecting over, the signs of progress in the medical art to which he alluded. About a hundred years ago, when Cullen was at the height of his fame, the Chair of the Royal Medical Society was filled by a man who had been the pupil, the friend, almost the dependent, of Cullen, and who then and afterwards became his most bitter antagonist. He was not a man of large personal experience in the art of medicine; his learning and accomplishments were of a very limited kind; his private life and character were the reverse of laudable; his personal and social standing was in every point so much inferior to that of Cullen as to admit of no comparison. Yet this man, first in the Royal Medical Society among the younger students, and afterwards by a literary and doctrinal influence which extended in a wave of controversy to the very extremities of European civilisation, became the recognised author of a system which all but subverted that of Cullen for a time in Edinburgh, and, up to the end of the century, occupied the minds of

¹ An Address delivered at the Opening of the Section of Medicine, British Medical Association, August, 1875. (BRITISH MEDICAL JOURNAL, 1875, vol. ii, p. 200.)

men in Italy and Germany to an extent that we can hardly even comprehend now-a-days from anything we know of the system itself. I will take it upon me to affirm that there are not two men in this room—if, indeed, there are so many in the whole British Medical Association (and I do not pretend to be one of the two)—who could at this moment give off-hand a reasonably clear account of the doctrine which was called Brunonianism, and of its application to the leading varieties of disease and their treatment. Yet it would be an inquiry fraught with the deepest interest to us, and I know none which would more effectually gauge for us the progress of the art of medicine during these last hundred years, and thereby show us if we are in reality getting upon solidier and surer foundations, than to ask ourselves whether, if some such man as this John Brown were to arise now and to broach such a system as he advocated in 1775, it would have a shorter life, and be less revolutionary in its effects. I cannot enter upon that question to-day; I cannot even talk to you, however briefly, of the respective merits of Cullen's and of Brown's teaching. . . . But if anyone should ask, Could such a practice, however indulgent to the weaknesses and follies of self-indulgent people, be successful in our day in floating such a general theory of disease and cure as the Brunonian system? I should incline to reply in the negative; and I should rely in giving that answer not only upon the great advances that have been made in physiology and pathology, the sciences upon which all genuine improvement in the medical art is founded, but also upon the much more exact and complete methods of clinical observation pursued in the present day, even as compared with the time of Cullen. I refer especially, of course, to the methods taught to the average general practitioner in our schools; for no one would wish to deny that we have had great and true, nay, eminently exact, clinical observers in the past, even in the remotest ages."

BRUNONIANISM.

Do not suppose for a moment that I mean to occupy you on the present occasion with such an utterly unprofitable discussion as would be in this our day one on the Brunonian system. For, immense as was the reputation of this very unworthy John Brown² in the later decades of the eighteenth century, it may be said to have soon died out, and the man himself to have been almost forgotten in the land of his birth. In France, the new birth of pathological anatomy, represented by the great name of Bichat at the very beginning of the nineteenth century, and afterwards by a long succession of brilliant and successful workers, put a stop to all such vagaries by furnishing a more tangible and real pabulum for the medical understanding. Only in Vienna, and afterwards in the north of Italy (strange to say, in the very country of Morgagni), were idle and resultless disputes continued in the schools, founded on the notion of *plus* and *minus*, as applied to the unknown and utterly incalculable quality of "excitability." But in Italy the controversy, in one shape or other, lingered on, in Bologna and elsewhere; and the division of diseases and of remedies into "hyper-" and "hyposthenic" was still largely responsible for the fame of Rasori, Tommasini, and others, up to the time of men who are, perhaps, still living. It may amuse you to hear that I once, but only once, encountered in the north of Italy a physician in whose eyes Brunonianism was still a very notable heresy, and John Brown almost a towering medical genius, however mistaken. This was in the great hospital at Milan, in the year 1852. I had gone there almost by accident, and was introduced by the late Professor Sigmund, of Vienna, to one of the visiting physicians—a typical example of the "old school." He was, indeed, a phenomenon in my eyes, as I daresay the Vienna professor (not to speak of his younger companion) was in his. What remains in my memory as to his hospital visit on this occasion was that he ordered about every third or fourth patient to be bled; but he did not once use, or seem even to think of using, the stethoscope. On the contrary, I remember well that when I suggested, in a case of acute or subacute rheumatism, that it might be well to listen to the sounds of the heart, he, with a gesture of impatience, repudiated the suggestion that there could be anything wrong there: "tutto è possibile" was his expression; "ma ciò non è;" and in the same breath he gave orders for "salasso alle dodici" (bleeding to twelve ounces). When I was first introduced to this old gentleman in the character of a young graduate of the Edinburgh school—then nobly represented all over Europe by Christison, Alison, Syme, Simpson, Goodsir, Bennett, all at the height of their fame in various lines of medical enterprise—it appeared to me just possible that their names might be known within the precincts of the

² It need scarcely be pointed out to the present generation, but might perchance require explanation at some future day, that the John Brown of the Brunonian system was *toto cœlo* different from the exquisite humourist and most charming and lovable companion who inherited the same name, though by no means similar ambitions, in Edinburgh—the author of *Rab and his Friends*.

Ospedale Maggiore of Milan. But, so far as this venerable representative of that hospital staff was concerned, I had pretty good evidence that it was not so. "Edinburgo," he said to me, in seeking for a compliment to my *alma mater*; "Ah, sì, sicuro—gran scuola, Edinburgo; Cullen era d'Edinburgo; sì—Cullen—e Brown! veramente, gran scuola!"

Those were days before any of the revolutions representing modern medical science had made their mark in Italy; before Cavour had perished under the lancet, in such a way as to make all Europe cry out against the abuse of that potent, and also at times very deadly, weapon. Italy and Spain were, in fact, several decades at least behind the age, and the belated march of medical progress had not been quickened there appreciably by the breath of public opinion in France, Germany, or England at this date. Nor was my venerable friend in the Ospedale Maggiore the only representative of the old school then flourishing in that great institution; for, as Sigmund himself told me on meeting me after a brief visit to the venereal wards, it was still possible to see there a man who had just been ordered to be bled for the third time in a gonorrhœa! Evidently in Milan, and in 1852, therefore, it might reasonably have been argued that the art of medicine, in its main traditions and practices, had not made many steps in advance since the time of Cullen, or possibly even of Hippocrates.

THE HIPPOCRATIC TRADITION: BLOOD-LETTING.

I should not have lingered over this personal incident had it not been that it enables me to give you a slight foretaste of an argument to which I wish to return in the course of the present address. There are very few of those now before me, I suppose—certainly not one of the younger members of this Association—who can possibly remember, I do not say any incident at all parallel to this, but anything at all approaching it in the shape of blind faithfulness to what was at one time supposed to be the Hippocratic tradition. Mark me, I do not say that the practice of medicine by Hippocrates himself was truly thus represented; my own clear opinion, entertained for many years, and without, I think, any undue prejudice, is that it was not so. It is rather remarkable that in the genuine Hippocratic writings we have only one example, if I am not mistaken, of blood-letting freely used under circumstances fully stated to us in detail—the case of Anaxion, in the third book of the *Epidemics*. The fact has been often noticed, and in the palmy days of venesectionist practice was explained away by a process of *ex post facto* reasoning, not very dissimilar in kind from that by which the facts of modern geological science are brought into an apparent harmony, or, at least out of apparent collision, with the book of Genesis. I have no time, however, to fairly discuss this point, or any other point of merely scholarly importance in the present address. I must ask you to take it as the matured conviction of one who has given some attention to the subject, that the genuine Hippocratic tradition was not nearly so extreme³ in respect of blood-letting as that of the mediæval, and still more that of many of the comparatively modern inheritors of the doctrines ascribed to him. It has already been stated that in the four or five centuries which preceded the firm establishment of the Roman empire, the practice of medicine had abundant time and opportunity to grow out of the leading strings of the supposed Hippocratic authority. Sects were rampant, and in the city of the Cæsars they were peculiarly so. Every doctrine of what was regarded even then as a venerable antiquity was thoroughly, and even keenly, discussed, with a freedom—though not, in all probability, with a freedom from prejudice—similar to what we may observe in the highest class of medical minds in our own day. Blood-letting had to hold its own against the most uncompromising opponents, and in some instances, no doubt, opponents on principle, of whom it is only necessary to mention by name Erisistratus, and this chiefly because, as an anatomist of celebrity, even in these early ages, he may probably have represented something like the attempt to raise up, independently of the Hippocratic tradition, a school of what we should now call scientific medicine. But in answering the question propounded by Sir William Hamilton, and again by Dr. Begbie, it would be absolutely necessary to take count not only of what Hippocrates did or taught personally (a thing not always easy to find out), but also of what he established and transmitted as a legacy to his remote successors. Now,

³ Take the following brief statement of it, which, although not certainly by Hippocrates himself, is probably quite in the spirit of his teaching: "You will bleed in acute affections if the disease appears to be severe, if the patients are in the vigour of their age, and if their strength is well maintained." (*Regimen in Acute Diseases*, Appendix, sect. 2.) Anaxion, above referred to, was bled on the eighth day of what appears to have been a very acute pleuro-pneumonia. There was only one bleeding, so far as appears, though it was characterised as being a "large one, as was necessary." There was a return of fever on the twenty-seventh day, the complete crisis occurring on the thirty-fourth, after an abundant expectoration, "with the characters of coction." The above may be compared, or contrasted, with the practice of James Gregory, to be referred to further on.

as I have already said, it was certainly not to Hippocrates, but much more to Galen, that any apparent stability of the art during the succeeding centuries is due. For good or for evil it was Galenist doctrine, much more than Hippocratic doctrine, that moulded the ideas of the later Greeks, of the Arabians, and of the whole Middle Ages and revival of letters. And it was from Galen, far more than even from Hippocrates, that Cullen received, indirectly as well as directly, the body of traditional doctrine on which he worked by way partly of criticism; and the practice of blood-letting, which he, like most of the men of his time, largely adopted.

INSTABILITY OF THE MEDICAL ART IN CULLEN'S TIME.

Now, to return for a moment to the Brunonian system, or heresy, and the apparent defeat which, for a time, it appeared to inflict upon the traditional or more orthodox medicine, represented by Cullen. The moral that I wish to point, as derived from this incident, is that the art of medicine, towards the close of the eighteenth century, was in a condition of very unstable equilibrium. Cullen, no doubt, did his very best for it, and compacted it into a system so luminous and so admirably set forth in his *Nosologia Methodica* and his *First Lines*, that, if I mistake not, it will be found that the influence of these two books has saturated, so to speak, almost the whole of the British, and certainly not less the Irish, tradition down to the time, at least, of Sir Thomas Watson in London, Alison and Christison in Edinburgh, Graves and Stokes in Dublin. The eighteenth century, in fact, was a century, in relation to the medical art, of system-building. Stahl, Hoffmann, and Boerhaave built each upon the foundation, or rather in great part upon the ruins, of his predecessors; and yet of all these great compacted systems of medical doctrine⁴ only that of Cullen, and perhaps of Boerhaave (through the extremely copious and readable commentary of Van Swieten), retained any authority or practical importance at the end of the century. It seemed as if the medical mind, oscillating between the claims of humoralism and solidism and vitalism, could find no resting-place, but was constantly exposed to be drawn out of the path of traditional medicine by any new theory which presented a certain amount of apparent simplicity and plausibility, all the more if it contemptuously dismissed, as unworthy of serious study, the laborious and positive acquisitions of clinical and pathological facts which had even then been largely gathered in, especially by Morgagni, as to the natural history of disease; and this was exactly what the Brunonian system did. It represented the treatment, not of one disease, but of almost all diseases as bound up with, and at the same time confined by, one formula or method of which the practical outcome was the copious administration of alcoholic stimulants and of opium. Substitute infinitesimals for the very palpable and potent remedies of John Brown, and exactly the same remark may be made of the homoeopathic doctrine, also a revolutionary child of the eighteenth century. But on this subject I have no time to enlarge.⁵ I allude to it only as one other illustration of the instability of medical doctrine in the time of Cullen, and this in Germany as well as in England.⁶

Have we attained to greater stability now? This question I ventured to answer, in 1875, in the affirmative, but with a very brief, necessarily brief, allusion to the circumstances which appeared to lead to this conclusion. I adhere, however, to the following statement, as then made; and I wish now to make it the subject of a few more remarks in this place. "There seems reason, therefore," I said, "to believe that we have been gradually gaining for the art of medicine a more secure position in connection with demonstrable or demonstrated facts, and that we have gone some way towards making it impossible for any man, coming forward with a new theory not based upon the careful study of facts, to make such a revolution in medical practice as was the case in Brown's time, and such as has been over and over again witnessed in the history of medicine almost from the days of Hippocrates."

DIRECTION OF MODERN PROGRESS.

The implication which is carried by the sentence just quoted is that the progress of modern medicine since Cullen's time has been in

the direction of *stability*—a kind and degree of stability not possessed by the art of medicine in any previous stage of its progress, not even in those "dark ages" (as we call them) when it had the *prestige* of a kind of immutability, or, as a theologian would call it, *orthodoxy*, founded on the overwhelming authority of Galen. Can I at all make good this proposition to you in the minutes that remain to me for this address? In order to do so at all, I need not say that I can deal only with a very few salient points in a vast field of inquiry. I must leave on one side to a great degree the enormous outgrowth of new remedies, new processes, new theories and practices in respect of disease which may at present be occupying all of your minds, as the proofs of our advance, but which, on the other hand, an adversary might declare to be still on their trial, and possibly even a questionable indication of progress, inasmuch as a profusion of means is not the same as a positive attainment of results. To take a single illustration from surgery, which is out of the direct line of my inquiry, but which cannot fail to be a striking one to most of you: it is not open to me even now to claim for *antisepticism*, in the strict sense of the word, the credit of an advance all along the line, because, as you all know, we have eminent surgeons among us who claim to obtain, and I believe do obtain, brilliant results without using the particular remedies called antiseptics; and therefore the theory of antisepticism, and the separation of what is permanently from what is only incidentally true in this great issue is a research of no small complexity, and perhaps of extreme difficulty for a professor of medicine, who nevertheless believes, as I do, that the impulse communicated to surgery through antisepticism is one of the greatest in the history of the art. And yet it is surgery which, as respects the more tangible and obvious character of its processes and results, has been humorously defined as "*Quod in medicina non dubium*!" and I am bound to speak to you, if I speak at all, of facts and theories even less doubtful than this.

As regards that great revolution in the medical treatment of fevers and inflammations, which in some minds occupies in relation to medicine as advanced a position as antisepticism does in relation to surgery, I can only declare in this place that while I have no doubt at all as to the advantage in some cases, and, in a limited sense, of the so-called antipyretic or antithermic treatment, I have yet to be convinced of the advantage of its application as a routine in typhoid or in any other fever. In other words, it has not as yet been made quite clear to my mind by evidence, statistical or other, that the treatment of any fever or inflammation by such means as were recorded some years ago by Liebermeister and Jürgensen, the one as regards typhoid fever, the other as regards pneumonia, has resulted in a permanently lessened mortality when set against results obtained or obtainable in this country, without these very active procedures. Having stated my opinion to this effect some time ago, however, I feel bound to add now that I am by no means insensible to the wonderful patience, research, and apparently scientific exactness shown by these authors in their latest researches on the subject. Certain modifications of the practice have also tended towards the removal of distrust, and should another great epidemic, say, of typhus fever, occur in Scotland, I should very gladly witness or concur in a therapeutic experiment such as would practically settle the question for us. In the meantime, however, and on grounds which I had occasion to state at the Belfast meeting of this Association, I must regard the antipyretic treatment in all its various forms as still under controversy. For the purposes of the present address, therefore, it is still, even if full of promise, not capable of being claimed as a clear step in advance—"adhuc sub judice lis est." But it may be readily conceded that both in typhoid fever and in pneumonia, as previously treated in German hospitals with an enormously high mortality, the method in question marks a distinct advance.

When we come to individual remedies and processes of cure, in respect of internal medicine, we have, of course, to deal with an exaggerated form of the same difficulty; unless, indeed, I am to make mere assertions go some way to supersede arguments. We have no time, in fact, to discuss in this address the action, or to appreciate the exact position in reference to the past or the future, of individual remedies. What shall we say about cod-oil, for example, in the treatment of phthisis? I am very sure, speaking as a practical man, that the general management of all manner of consumptive disorders has immensely improved since the introduction of cod-oil into their medical treatment. But is the oil a specific, or is it merely a food? and how does the oleum morrhue take rank—whether food or medicine, or both, as a therapeutic agent among the almost innumerable new means, and, as I think, distinct advances, in the management of consumptive diseases within my own personal recollection? I am well assured that most of you will agree with me that these questions cannot be adequately answered now. At all events, here is one of the

⁴ A carefully written account of them will be found in Dr. John Thomson's *Life of Cullen*, Vol. i.

⁵ I have very fully discussed it, however, in its relation to medicine and medical history in *Edinburgh Essays*, by Members of the University, Edinburgh, 1856, p. 95; and in relation to the personal history and character of Hahnemann, with references also to Paracelsus and John Brown, in a small volume (long out of print), entitled *Medicine and Medical Education*; three Lectures, with Notes and an Appendix, Edinburgh, 1858. (*A Triad of System-Builders*, p. 79.)

⁶ The very notable disturbance of tradition by the system of Broussais, in France, at a later period, although running entirely in the opposite direction, may perhaps be alluded to as a somewhat similar phenomenon, although here the corrective was speedily applied in the admirable researches of Laennec and Louis, based upon imperishable foundations of fact.

most popular of remedies, which has been fully before the British medical public almost as long as the British Medical Association itself; and yet I am not in a position, even now, to dogmatise about it as marking, *per se*, a distinct step in advance in subduing one of the commonest of diseases, without a host of rival agencies immediately claiming attention, as sharing with cod-oil the glory, if any, of the supposed advance.

No one having any familiarity with modern medical practice can overlook the large place occupied in it by certain powerful, and it would be hardly too much to say indispensable, remedies acting on the nervous system, many of which are entirely modern; that is, not only new to therapeutics, but comparatively new in respect of being unknown, even as substances, until a date very much within the bounds we have assigned to the present inquiry. The most prominent, and perhaps also the most unquestionable of these is chloroform; and if forty years can ever be regarded as sufficient for testing an advance in medicine, or surgery, it might be safely affirmed that the reputation of our day and generation might be regarded as saved by this one great achievement. Yet there are not wanting evils to be discounted, and failures to be set against the benefits of this enormous boon to the suffering human race; and it would not surprise me to find that there exist, even now, well-informed persons who believe that the Lethe which comes from modern chemicals, like that which of old was born of poppy and mandragora, has been a questionable advantage to humanity at large. What then shall we say of hydrate of chloral, butyl-chloral, paraldehyde, urethan; and what of the condensed forms or alkaloïds of the older narcotics, so immensely convenient, but also so powerful for mischief? I must leave you, gentlemen, to answer these questions. I, for my part, have no doubt that the balance inclines, at least in instructed medical hands, greatly in favour of the service these remedies are doing, and may yet do, to great numbers of the sick, and especially the fatally afflicted members of our race. And it is also not to be forgotten that in the treatment of one of the most incurable diseases, as well as in many others, we have secured, since 1857, a palliative (to say the very least) of the greatest power in the bromides. The almost accidental discovery by Sir Charles Locock of a very special therapeutical application of bromide of potassium has in this case opened up quite a new chapter in the resources of the medical art as regards a host of other diseases, and chiefly of epilepsy, which from the time certainly of Hippocrates had always been regarded as one of the chief reproaches of the art, and accordingly given over to every kind of physical and metaphysical superstition.

Again, the modern use of digitalis, strophanthus, and other cardiac tonics; and again, the use of nitrite of amyl and of nitroglycerine in angina pectoris and the allied conditions: Who is there among you that doubts (for I certainly do not) that in these, when well employed in judicious and well-instructed hands, we have obtained a very distinct and very valuable addition to our resources in cardiac diseases? And yet, is there any one of you who will expect me to prove this in an unassailable manner, to the satisfaction of a sceptical friend or foe within the limits of the present address?

Once more: the benefit derived from the salicyl compounds in acute rheumatism is, to my mind, very satisfactorily established; and I should be personally disposed to claim this as a very distinct advance in the healing art within a comparatively recent period, and a partial removal, moreover, of one of the old *opprobria medicinae* which I have lived long enough to have been obliged, over and over again, to recognise as such. But will anyone say that my simple assertion of this (for we could not hope to get further into the argument now) can reasonably outweigh all the sceptical or contradictory statements on this point which it might be expected to call forth? Evidently, in regard to remedial agents and processes not more than twenty years old, it is vain to expect such a concurrence of opinion as to entitle anyone to set them forth to you dogmatically, as the permanent and unquestionable milestones in our progress, which future ages too may be expected to acknowledge as such. In considering stability, therefore, we have not only to lay our account with possible new discoveries, but also with the perhaps unknown or unappreciated dangers attaching to practices now vaunting themselves in the full sunshine of apparent prosperity and success. While, therefore, I have and profess the most entire faith that the efforts of hundreds of young and enthusiastic workers are being fruitfully directed towards a science of pharmacology, on the joint bases of experimental research and scientifically directed observation, we are still, I fear, too much in the transitional stage to allow of many of these researches being adduced in evidence of the increasing stability of the medical art. Rather might it appear to some, even of ourselves, as if the multitude of new remedies which come to us from all the ends of the earth,

the too brief reputation of some of these, and the evidently over-sanguine estimates of others, are the proofs of an unrest which is quite the reverse of stable; which cannot be satisfied with the old remedies, old prescriptions, old methods of cure; but holding itself bound to drive on the *currus triumphalis* of modern therapeutics at all costs, is likely to land us once more in a new series of controversies such as attended the famous work of Basil Valentine on the healing virtues of antimony, perhaps three hundred years ago.⁷

But I hold, and have never ceased to hold, as a teacher of medicine, that all this fermentation, so to speak, of opinion with respect to current remedies and modes of treatment, all these tokens of unrest and dissatisfaction are, in reality, an advantage to the medical art, and tend both towards progress and towards stability; an altogether different kind and measure of stability, however, from that implied in the authority of Galen, or the traditions of the Greeks and Arabians. Allow me again to quote a very few sentences spoken rather more than twenty years ago, but to the general sense and scope of which I am disposed still to adhere, with the proviso that the twenty years in question, and especially the newer labours in pharmacology, which I only do not name in detail in order that I may not be supposed to be indulging personal preferences in a semi-popular exposition, have done much to carry my convictions as to a real and solid advance, as compared with the earlier date.

ORTHODOXIES V. REAL SCIENCE.

"The art of medicine," I said, "is at this moment in a peculiar position. The day of orthodoxies is over, the day of real science is only just dawning. It is no longer possible to condemn a man, even by implication, for having ceased to believe what our fathers believed; but it is extremely difficult to state in general terms what we believe ourselves, and still more difficult to forecast the future, and to lay the foundation of the faith of our successors."⁸

Notwithstanding the apparently sceptical ring of these sentences, I hope to be able to show you immediately that there are at least some points of our progress in which the dissolution or weakening of what I call orthodoxies has been accompanied by a truly constructive process, and that the new theory and corresponding practices which have replaced the old differ from it in this—that, being the result of applied science and not of mere tradition, they are correspondingly secured against revolutionary overturn.

1. *Hygienic Remedies.*—The fruitful, and I think unquestioned, progress of preventive medicine (otherwise called sanitary science) during the last thirty or forty years is not only a very striking advance in itself; but it may also be maintained, I think, without contradiction that curative medicine (with which we are chiefly concerned here) has largely and quite steadily advanced along the same lines.

The prevention and the cure of disease are not, indeed, identical, but the laws which guide the former are in a large measure, and necessarily, applicable to the latter. The same noxious influences which beget typhoid fever, for example, tend in a remarkable degree to exaggerate, if not to cause, a host of minor diseases, and very notably to increase the risks of the healing of wounds, and of the puerperal process. The same overcrowding, which is in a great degree the cause of typhus fever, is a source in hospital wards of pyæmia and erysipelas, in barracks and gaols of consumption (and this often among picked men), in certain endemic localities of contagious pneumonia, and probably of diphtheria.

It is impossible, in the face of modern sanitation, to escape the conclusion that the causes and the remedies of disease alike are often to be sought more in the study of groups and even of multitudes of cases than in the ordinary text-books, or in the conventional remedies set down for the particular *nomen morbi*. In short, the treatment of diseases of every kind has been largely, even if silently, influenced by the thought that the chief disease-breeding factors must also be, in individual cases, adverse influences to be carefully studied and kept in check by those who would cure disease; and that, in chronic cases especially, a large part of the cure consists in the removal or modification of the injurious action of the cause. I had an opportunity, however, of enlarging a little more on this great theme than I can do to-day in my address in 1883 before the Sanitary Insti-

⁷ I have given a brief description of this strange controversy in one of my Introductory Addresses (see reference below), p. 65. The date, and even the personality of Basil Valentine are extremely obscure, but it is certain that the administration of antimony was formally condemned by the Faculty of Paris in 1566, and again in 1615, both Acts being ratified by the Parliament of Paris, and numerous prosecutions, with endless paper-warfare, following thereon. In 1666 the use and administration of antimony were formally allowed, the disputes having extended over more than a century.

⁸ "Medical Education, Character, and Conduct," Introductory Addresses, delivered to students of Medicine in Edinburgh and Glasgow, 1855, 1866, 1882; Glasgow, 1883, p. 58.

tute' at their congress in Glasgow, when I occupied the chair of the Section of Sanitary Science and Preventive Medicine. I will, therefore, abstain from saying more about it now than that the changed point of view implied in the more fruitful and practical study of causes has in many cases made it impossible to cling to the old and traditional aspect of certain diseases as entities,¹⁰ arising we don't know (and don't strive to know) how, and only to be met by a *farrago remediumum*¹¹ addressed to individual symptoms, or, at least, directed to the consideration that a morbid influence, or something unknown, has got to be battered down in the recesses of the economy by sheer expenditure of therapeutic violence, at whatever cost to the physiological functions of the sufferer.

2. *Self-healing Processes in Acute Diseases: Abuses of Venesection.*—Closely allied with this change, which I take to be a permanent one, making in the direction of stability for the medical art, is another which has come to a head, or at least has begun to bear fruit largely, only within my own recollection, and which I can distinctly recall as most powerfully influencing my own mind and practice from the very earliest period of anything like independent thinking about it. I do not wish, even for a moment, to lose sight of the fact that the irrational orthodoxies (as I ventured to call them) of the past have been unsettled, very largely owing to the prevalence and the counter-vailing influence of certain heresies, of which no rational mind can approve. But it is one of the privileges of our art in its more advanced and, as I think, more stable condition in this day of ours, that where there is no orthodoxy there can be no heresy (in the old ecclesiastical sense of the term); or, rather, that our freedom of opinion is such that we can afford to absorb into our daily practice, and even into our theories of disease, the instruction that comes even from the most erratic and monstrous of heresies, no man making us afraid. Forty years ago, for example, it was still a point fairly open to question¹² whether pneumonia and other acute inflammations could possibly be cured without blood-letting. You all know, as well as I do, what has come of this burning controversy, and how completely the old traditional theory, as it came down from Cullen and Gregory, has been overthrown; but it is surely a cardinal instance of the essential instability of that theory, that after being for a time supplanted, in Cullen's own day, by the ridiculous extravagances of a vain-glorious charlatan like John Brown, it should have been reinstated in greater power than ever as regards this particular point in therapeutics, only to be subverted once more, and more completely, by a far better man, the late Sir John Forbes. He it was first, I think, who, becoming distrustful of the old tradition, and anxious to learn in a genuine spirit of scientific humility about "Nature and art in the cure of disease," did not refuse to accept truth, even at the hands of homoeopaths, and of others not much, if at all, superior to the Brunonianists of Cullen's day in their claims upon genuine medical science.

Although it will be impossible for me to enter into the details of this controversy, I happen to have the means of giving you a very instructive and vivid glimpse into the state of opinion out of which the controversy arose, for I have the great privilege of being able to read to you, from the manuscript notes of the late Sir Robert Christison, placed in my hands by his sons, the actual teaching of the Edinburgh chair of medicine on the subject of blood-letting in pneumonia in or about the year 1819.¹³ Dr. Christison has paid an ample tribute, in his Auto-

biography, to the excellence and power as a teacher, as well as to the great practical experience as a consulting physician, of Dr. James Gregory; and I will only add, before reading these sentences from his lectures, that the state of mind they set forth as regards the imperative necessity of blood-letting in certain circumstances is one simply impossible now; in saying which, however, I wish to make the reservation that there still exist good men and true who believe that a much more qualified use of the remedy is in accordance both with experience and with the dictates of true science. My own opinion is not, perhaps, of much consequence; but I may state that, having at a very early stage of my career become convinced of the great abuses of venesection, I have not had anything to cancel or recall in this respect; but am rather inclined, if anything, to admit that the reaction of opinion against this and other powerful therapeutic agents may have gone a little too far, as will perhaps occur to some of you also in reading an article by an able and skilful practitioner, a very good friend of mine, in the number just published of the *International Journal of the Medical Sciences*. I forbear, in accordance with the whole plan of this address, from indicating living writers; but I feel sure that this gentleman, and several other friends whom I could name, engaged more or less in country practice, are quite capable of reading lessons to us of the towns, and of the hospitals and schools, which will one day bear fruit. But as to what may be called the Gregorian practice, probably differing (for the worse) even from Cullen's, and certainly from the genuine Hippocratic tradition, I expect that there can be now only one opinion among my hearers.

Here then are, as nearly as we can now obtain them, the very words and phrases of James Gregory, as addressed, under all the sense of high responsibility as a teacher, to the students of Edinburgh University:

"By far the most effectual way of checking inflammation of the lungs is blood-letting. In most cases, more than one bleeding is required, though some very mild cases may be successfully treated by one bleeding, and that not a very large one. I have seen some patients cured by a single blood-letting, to the extent of 10 or 12 ounces only; but it is very seldom that practitioners are called in early enough to effect a cure so easily.....Six, eight, or ten times in the course of two or three weeks is commonly sufficient to stop an ordinary pneumonia. It is often found necessary to bleed twice, sometimes three or four times, in one day. But this very rough practice is by no means to be employed without a very sufficient reason.....We must always be aware of the necessity of not urging it too far." (!)

"It is not easy to regulate the quantity necessary to be drawn at each bleeding. From 12 to 20 ounces Cullen reckons an ordinary bleeding—under 12 a small, above 20 a large bleeding; but in many cases it is found both practicable and expedient to go far beyond 20 ounces. The greatest quantity I have ever ordered to be drawn from a patient under pneumonia is 35 ounces.¹⁴ From a man of middle age, and under the middle size, I once took that quantity at the eighth bleeding, and on the seventh day of the disease. Before the last bleeding the symptoms were very severe, and they were accompanied by violent hiccough. If this arose from any affection of the stomach, or from general weakness, I knew that the man's life was irrecoverably lost. But there was a chance that it depended on the inflammation having spread to the diaphragm. Considering this supposition as the only chance of cure, I ordered him to be bled till the hiccough went off. It did go off, and the man ultimately recovered. In seven days he lost 160 ounces of blood."

Other instances are given in detail which I prefer to summarise, in order to save your time, although they are well worth reading. In one, 123 ounces of blood were drawn in little more than twenty-four hours. In another, a medical student, 264 ounces were lost in the space of about a fortnight, and at one of these numerous bleedings (for a cerebral affection arising from injury) 60 ounces were taken at once; "a far greater quantity," adds Gregory, "than I have ever attempted to take." In another case, communicated to him by a pupil, 560 ounces of blood were lost, on account of pneumonia, between July 25th and August 16th; 204 of these being in the first week of treatment. But the following example is, perhaps, more startling in its brief and simple record than any merely numerical statements:

"A man of a small stature laboured under pneumonia, and had been repeatedly bled. On the evening of the second day, my clinical clerk came to inform me that, pursuant to my request, at noon he had got him bled to 32 ounces, but that the symptoms were not

¹⁰ *Transactions of the Sanitary Institute of Great Britain*, vol. v, p. 71, 1884.

¹¹ This subject was discussed more at large in a Presidential Address delivered by me at the annual meeting of the Medico-Psychological Association, August 2nd, 1882, and published in the *Journal of Mental Science* for October, 1882. See also *Insanity: Modern Views as to its Nature and Treatment*. Glasgow: Maclehose, 1885.

¹² The pharmacopoeia of Sydenham (who was in his day considered the most enlightened of physicians) contained, not infrequently, thirty or forty drugs combined in one prescription. I have had occasion to remark unambiguously (though, of course, without giving names) upon prescriptions even of contemporaries containing eighteen separate ingredients, and several of these strong and poisonous remedies. The late Sir Robert Christison's eminently wise, though uncontroversial, attitude towards the polypharmacy of his own day, has been the subject of some remarks in the second volume of his life, recently published, Edinburgh, 1886, p. 131.

¹³ See the numerous articles, letters, etc., directed particularly to this question, and to the simpler or more expectant treatment of diseases generally in the later volumes of the *British and Foreign Medical Review*, edited by John Forbes, M.D., F.R.S., F.G.S., and especially the very candid statements of the editor himself in the postscript appended to the concluding vol. xxiv, 1847, p. 594, as to the influence of these discussions on the sale of this journal, and probably on its discontinuance. See also the controversy, famous in its day, on the particular subject of pneumonia, introduced by Dr. Bennett and Dr. Alison into the Edinburgh Medico-Chirurgical Society in 1856-57, and chronicled in the *Edinburgh Medical Journal* of those years.

¹⁴ The copy of notes in question is not dated, and may have been written in 1818. Sir Thomas Watson attended Dr. Gregory's lectures in 1820, and carried away an equally vivid impression of them with Dr. Christison, as his own famous work shows at several points. For Sir Robert Christison's personal impressions, see his Autobiography, in the *Life*, edited by his sons, vol. i, pp. 76-85; also vol. ii, pp. 117, 118. Gregory died in 1821.

¹⁴ On this the reporter (Christison) remarks in pencil (perhaps at a later date) as the result of his own experience: "I once took 60 ounces from a negro, and 40 more in thirteen hours. Dr. Graham, Professor of Botany and Clinical Medicine, once took 72 ounces for pleurisy."

abated; that he was very weak, and did not appear capable of standing further blood-letting. I went with the clerk to see him, and, notwithstanding the unpromising condition of the patient, I resolved to bleed him again, *ad animi deliquium*. After 22 ounces were taken away, there was no appearance of his fainting; but severe hiccough came on, and occasional twitchings of his face. I was exceedingly alarmed at this; but they disappeared on laying him in the horizontal posture; the pain and hardness of the pulse abated, and he became soon convalescent. In the course of two days and a half he lost 98 ounces."

There is a good deal more in these instructive notes that would bear citation with respect to my present purpose; but I dare say you will permit me to close the interesting record with one more very brief paragraph, which is chiefly valuable as showing the well-established reputation of the speaker, and his entire good faith in pressing these conclusions home to his hearers.

"Many of you may probably have heard much said about my rough practice in pneumonia, and the eagerness with which I urge copious detraction of blood. This may be, so far, true. But you will observe that I am seldom called in, except in the very worst cases, *when every practitioner would be aware that nothing but free venesection can be of utility*. If I get any patient in favourable circumstances, I by no means carry it to such an extent. There are many who go along with me in this practice, and there are several very eminent practitioners who carry it to a greater length than I ever do."¹⁵

Setting aside differences of individual opinion, I have no doubt that you will all agree with me that the most striking fact about these extracts, considered as an historical specimen of authoritative teaching, is not even the unhesitating faith in blood-letting as a remedy, but the belief in it as the remedy—the only possible remedy—for a severe case of pneumonia. It is as clear as anything can well be, and I may add that the passages I have suppressed for the sake of brevity only bring this out more clearly, that Gregory believed with all his heart, with a strength of conviction apparently derived from experience as well as tradition, that a case of this kind, if not bled, and bled largely, must of necessity prove fatal. This conviction, and no other, will account for his fanaticism, as we must now esteem it—the fanaticism, however, of a powerful and vigorous mind, misled only by the fact that, under the influence of a traditional orthodoxy, he had never allowed himself to think or to try otherwise. For we now do not think, but we know, most absolutely and positively, that it is quite otherwise; that the great majority even of severe cases of pneumonia will get well under a much simpler and less spoliative treatment. We also know full well that under such treatment the mortality is much less now than it was then or since, under a free and a systematic treatment by blood-letting. But the great, because the permanent, step in advance has been in reaching the conviction, founded on experience, that a simple, almost a hygienic, treatment of the most acute diseases may also be one of a very moderate or low mortality. And this conviction, otherwise expressed as one of the self-healing tendencies of acute disease—although probably embodied in a phrase attributed to the Father of Greek medicine *νόσων φύσις ἥπρτο*¹⁶—has required more than two scores of centuries, and especially the last half-century, to stamp it down as a current medical doctrine; to convert it, moreover, into a fundamental rule or principle of medical practice in the present day.

3. *Epidemic Fevers: Alcoholic Stimulation*.—I am tempted at this point to allude to another instance of changed practice occurring within our own day, and applied, or applicable, over a very wide area of disease. I do not know if in this instance I can count upon the assent of all of you to the revolution being a *fait accompli* even now, nor will I venture to affirm that the terms in which it must be stated are final; but as the Public Orator of the University of Dublin furnished some of us only the other day with an epigrammatic statement of it in the words *lacte non vino*,¹⁷ perhaps a very few minutes may be devoted to the consideration of it, as briefly and as impersonally as is possible in this place.

¹⁵ I will venture here to express again what has been elsewhere noted with respect to these valuable manuscripts, interesting to every historical student of the art of medicine, "a wish that some selections from them could be published even now, as a monument of one who has left no printed record of himself in the department of practical medicine, in which he was so long an acknowledged leader."—*Christison's Life*, vol. ii, p. 118.

¹⁶ See *Insanity: Modern Views as to its Nature and Treatment*, p. 29.

¹⁷ The following sentences from Dr. Webb's oration at the honorary graduations on June 30th, though subject to correction in at least one point of detail, may be permitted here as a humorous statement of the case, to be preserved in connection with the present address: "Febris sanum, ut memorie tradidit Cicerone, in Palatinum quondam stetit—inter colles Glasguenses nunc simile stat sanum. Hospes noster, quem nunc salutamus, ejus est Antistes (?) et quod mirum, lacte non vino numen illud deestatum se placuisse gloriatur."

All of you who have read (as who has not?) the late Dr. Murchison's classical volume on *Fevers* must have been struck with the historical summary he gives, with such minute accuracy, of the confusion arising out of the names of the continued fevers adopted by Cullen—synocha, synochus, and typhus; the first, corresponding with the fever of high reaction, or so-called inflammatory fever (perhaps the *καύρος* of the Greeks); the last with the low nervous fever, or fever of debility, and partly with the malignant or putrid fevers of some of the older authorities. Now, as a general rule, it is hardly too much to say that while the employment of blood-letting and other evacuant methods was at one time universal, or nearly so, in the synocha, recognised as such; the treatment of typhus, when frankly admitted as typical, and as tending to debility and nervous exhaustion, has leaned as a rule, during at least the last seventy years, in the direction of alcoholic stimulation. Synochus, as a merely intermediate or indeterminate form, may be here excluded from consideration; but it is important to remark that, up to the time to which my own recollections of current medical doctrine extend, the idea prevalent was that epidemic continued fevers oscillated between synocha and typhus, in cycles of years; and that the treatment, in order to keep pace with the constitution then existing, had to be regulated according to the "epidemic type" which, judged by the experience of those who had been watching the progress of events, could be said to represent the general tendency, according to the all but universally received doctrine of the "change of type." This doctrine, which may be traced back, though not under the same name, to Sydenham, was strongly held in Edinburgh when I was a student; and when the relapsing fever of 1843 (a true synocha according to the old definitions) somewhat suddenly appeared, it was held to be a "nova febris," quite distinct from the mixture of typhus and synochus which had held the field for many years before. Dr. Christison alone recognised the fever of 1843 as identical with the synocha observed in 1819, and at that time treated (as Dr. Murchison has clearly shown) by copious venesection, with the supposed result of cutting it short at the close of the first week; a strictly normal crisis, however, of the fever itself (as observed in 1843), and quite erroneously attributed to the remedy.

But it is not with the depleting practices in epidemic fevers that we have to do, in the few minutes more that I claim your attention; but rather with the reaction against these, which in Edinburgh we always associated with the venerated name of Alison, and which in Dublin was no less characteristically set forth by a man of very different genius, but equally earnest, and equally devoted to his art—that great Irish physician who has left us his testimony in three notable words which, he said, might be inscribed upon his tombstone,—"*He fed fevers.*" It is not in this city that the practice of Dr. Graves, especially in typhus fever, requires to be explained or commented on, or even brought to mind as an historical fact; but I think this famous phrase was perhaps the first distinct recognition in Ireland of what is now one of the common-places of medical doctrine—that febrile disease of all kinds, being essentially a wasting process, requires to be met by such treatment as will preserve, rather than exhaust, what remains of the powers of life, and especially the greatly weakened functions of alimentation, digestion, and textural nutrition. Nor is it necessary to suppose that either Graves or Alison had anticipated, what came about in course of time, the extension of this general doctrine to nearly the whole field of disease, and the disappearance, under its influence, of a host of depleting and debilitating practices. But what is quite clear is that in typhus fever, as it occurred epidemically some time between 1819 and (say) 1829,¹⁸ the idea first took root in some of the most prominent minds in the medical profession, that alimentation, not depletion, was the proper practice to be pursued in most of the epidemic fevers. In Dr. Alison's case this idea was practically determined, to some extent, by his favourite notion of a "change of type;" and when "synocha" reappeared in 1843, it might, perhaps, have been expected, according to his theory, that he would have reverted to the old practices; but I have no evidence and no recollection of his having done so. In typhus, however, his practice was undoubtedly, and emphatically, one of stimulation; leaning, perhaps (though I am by no means so sure of this), more to the side of alcoholic stimulants, and less to that of food, than the practice of Dr. Graves. But in any case, the practice of both was guided by the same essential principle; that of supporting the strength, and so warding off the tendency to death by asthenia. This, too, he used

¹⁸ The former date is that of the epidemic chronicled in Dr. Welsh's book: the almost indiscriminate use of the lancet being the leading feature of the practice as described at that time. The latter is the period usually assigned by Dr. Alison himself as the limit in time to the appearance, within his own experience, of fevers that would bear depletion; and the probable date, therefore, of the more decided adoption of the opposite practice.

himself to expound to us as really quite in accordance with the Cullenian tradition; inasmuch as Cullen's principle was, in fevers, to "obviate the tendency to death," rather than to adopt any violent or perturbing treatment.

But while the general plan or idea of supporting the strength in epidemic fevers may be said to have had its origin thus early, the modes in detail of carrying out that principle must have varied almost indefinitely; and it is probably quite safe to say that the general principle of Dr. Graves, to "feed fevers," became gradually merged, with some, in a process of high alcoholic stimulation as a substitute for feeding. It is very difficult to say how far individuals were responsible for the advance and development of this tendency in London, Dublin, or Edinburgh respectively; but the name of one very eminent Irishman who had attained a reputation of the highest kind in London, began, twenty years later, to attract attention as representing a kind and degree of stimulation, not only in fevers, but in almost all acute diseases, which had previously been very unusual, and had probably never before been advocated so ably or in such seductive and uncompromising language. You all know well the name of this most able compatriot of Graves and Stokes; but as I have been accused by one at least of his countrymen of disrespect for his memory in submitting his opinions to professional criticism, even in the most carefully guarded language, I will avoid as far as possible any needless allusions to him in this address, which may possibly find its way into other than medical hands. The theory adopted, however, was that alcohol is not only a real food, but that it is the food of all others—perhaps, indeed, the only food—to be relied upon for supplying the material of repair in fevers; thus compensating the waste and averting the functional exhaustion characteristic of most acute diseases. The practice associated with this theory was the subordination of all other nourishment (with the possible exception of beef-tea) to alcoholic liquors, and especially to brandy given in high doses continuously from half-hour to half-hour all day and all night, during the currency of severe cases of acute disease, and almost without any exceptions. So little, indeed, did the previous habits of the patient, the age, or the sex, count in this administration of alcohol, that a nursemaid, aged 18, had forty-eight ounces of brandy *per diem* at the height of her disease, and between four and five gallons in all; a rheumatic girl had six drachms of brandy every hour; and another girl, aged 14, far gone in organic disease, had several gallons of brandy in the course of a few weeks. These practices were, no doubt, conscientiously pursued and conscientiously defended; but the practical result of them was such a widespread and apparently irresistible professional bias towards the prescription of alcoholic liquors, especially in England and in the metropolis, that some of the leading physicians both in London and in the provinces, some years afterwards, found it necessary by a distinct manifesto,¹⁹ which was largely signed, to dissociate themselves formally from an excess which in their deliberate opinion tended very decidedly both to moral and to physical evil, and made the profession at large appear to be partakers in the blame justly due to those who needlessly or recklessly foster habits of intemperance.

More than seven years before this point had been reached, however, in the opinion of these eminent English physicians, it had been shown in a Scottish hospital that in the one principal disease in which the entire medical tradition was unequivocally in favour of stimulation—namely, typhus fever—success was to be attained in treatment, not by increasing, but by very considerably reducing, the amount of alcoholic stimulants administered; and this, as compared with an amount which, even at its maximum, fell far short of that which was habitually prescribed in London. This inquiry was brought about, in the first instance, not by any *a priori* considerations, and still less by any fixed opinion adverse to the medicinal use of alcoholic stimulants, but by the fact that the author of it felt himself under an obligation to justify to one of his senior colleagues the mode of employing these stimulants at which he had arrived as the result of personal experience, and, in doing so, was compelled to investigate as carefully as possible, and as far as possible numerically, the whole of the data available in that particular epidemic and in that particular hospital. The results of this inquiry were communicated to the *Lancet* in March, 1864;²⁰ and this was followed up by several other papers, and

particularly by one on "The Limits of Alcoholic Stimulation in Acute Disease," read to this Association at Oxford in August, 1868.²¹ It would be vain to attempt here, at the close of this long address, even the most imperfect statement of these results. The one thing about them is that they have been accepted, without having ever been seriously assailed. It may now be regarded as a position established by evidence, that in typhus fever, and therefore, *a fortiori*, in other fevers and in all diseases attended by febrile debility, alcoholic stimulants are not the only nor yet the best form of alimentation; that their proper use is as medicines, and this within limits; and that, as a rule, and also within such limits as are suggested by reason and easily attainable in practice, milk (which during the alcoholic fanaticism had been nearly lost sight of) is in reality the food which is for the most part more easily digested and more conducive to success than almost any other. And thus the epigrammatic motto of your University Orator, "Lacte, non vino," has received, I trust, at once its seal and its justification.

I have thus endeavoured to show you, by a few instances only, in what manner, and on what lines, the art of medicine has been advancing towards stability, as I affirmed in an earlier part of this address. The last example is curious, because the Brunonian system, while theoretically very widely different, was characterised in some respects by a like excess in the administration of alcoholic stimulants to that which was current in London at the time just referred to. But no heresy like that of John Brown, crude and devoid of all foundation in real science, could have held the field for a single year in the middle of the nineteenth century. What has been doing in all these years has not been in vain. A vast army of earnest, unprejudiced, and disciplined workers has been engaged upon all the unsolved problems connected with disease. Diagnosis and pathology have made immense strides in advance during nearly the whole of the present century. Operative surgery has achieved the most brilliant and unquestionable triumphs of any period in the long course of medical history, and this in almost every department. Hygiene and sanitation, with their reflected influence on curative medicine, have advanced by leaps and bounds; and, even in the application of strictly therapeutic methods, the changes, though not so easily stated or so palpably appealing to the judgment of public opinion (inasmuch as quackery always is and always will be more self-asserting than truth) are all in the right direction. Polypharmacy, in its old and irrational forms, is now nearly gone. Hardly anyone of intelligence supposes nowadays that a disease is to be slain by a mere prescription or Latin formula. A science of pharmacology,²² or of the uses and actions of medicines, is being built up upon a foundation that was never even possible before the present day, and which, beyond all question, will render this, the least satisfactory department of the science of healing, far more secure in the future. The mere simplification in the forms of remedial agents, and the mitigation accordingly of the old, inexpressibly repulsive, and nauseous manner of prescribing them wholesale (which incurred the ridicule of Molière two centuries ago, and yet lived on into almost our own day) represents an advance by no means devoid of importance as regards the comfort of the individual patient, and the peace of our families, and especially of our nurseries. Nor is the immense relief to human suffering by the use of anaesthetics, rightfully employed, and even of narcotics and anodynes in greatly improved forms of administration, to be lost sight of, even in the most brief allusion to the modern use of drugs.

All that removal, moreover, of the old traditional dogmatisms to which I have referred has been merely a clearing of the ground, so to speak, on which a modern science of healing, not by any means forgetful of its past history, but entirely unshackled by its old errors and prejudices, is being slowly but surely built up. And the fervid youth of the present generation, if they remain true to the spirit and not to the mere letter of the Hippocratic medicine, may well be grateful to their fathers and their grandfathers, were it for nothing else than this important and necessary process, whereby a soil has been prepared, at much cost of individual labour and sacrifice, on which they are permitted and privileged to sow the seed, and to reap more or less fruitful crops of good for humanity at large.

¹⁹ The history of this famous Declaration, as well as the substance of it, and some of the signatures to it in detail, will be found in the *JOURNAL* for December 23rd, 1871, p. 737; and a more complete list of the signatures is given in the *Medical Times and Gazette* of the same date, p. 778. The opposite side of the question, professionally, in so far as it appears in the remonstrances of some of those who objected to this Declaration, will be found in the *JOURNAL* for December 30th, p. 788; *Lancet* of same date, p. 931; *Medical Times and Gazette*, December 23rd (all of 1871), p. 779.

²⁰ See *Lancet*, March 12th, 1864, p. 91. "Facts and Conclusions as to the use of Alcoholic Stimulants in Typhus Fever;" and also leading article, March 26th, 1864, p. 357. The other papers here referred to in the *Lancet* are as follows:—

"Clinical Observations on the Course of Typhus Fever, and especially on the Phenomena of the Crisis," January 21st, 1865, p. 57 (remarks on Milk as a Food, p. 58), and "On the Treatment of the Acute Diseases of the Chest," January 18th, 1866, p. 29, and January 20th, p. 59 (especially the latter paper).

²¹ *Glasgow Medical Journal*, November, 1868, p. 15.

²² See, on this subject, the account of the unfulfilled aspirations of the late Sir Robert Christison, *Life*, vol. II, p. 127, *et seq.* Also his *Address in Therapeutics*, read before this Association in 1858, and reported at length at the time in the *JOURNAL*.