Observations

ON

RESEARCH IN MEDICINE: ITS POSITION
AND ITS NEEDS.*

BY

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WITH the greatest activity of medicine, that which ministers to the immediate needs of sick individuals, and which may be called *curative medicine*, we have here no direct concern. Our present interest is directed to that activity the immediate purpose of which is to advance knowledge; and this for convenience will be spoken of as *progressive medicine*. Progress in medicine can be followed historically along two main lines. The first is that traced from Hippocrates through Sydenham; it is the method of observation, and it has given us our descriptions of disease. The capacity of clinical observation to separate and define diseases was greatly enforced by the work of the famous morbid anatomists; the combined observations laid the foundations of modern clinical medicine. Laennec and Skoda, exponents of tested methods of physical examination; Bright, Hodgkin, Corrigan, Addison, who so ably identified and described individual diseases; these names but exemplify that most brilliant period of clinical activity, which was at its height in the earlier years of last century.

The second line of progress has been by the experimental method, using this word in its broadest sense; it is exemplified in the physiological field by the work of the physician Harvey and by Claude Bernard; in the bacteriological field by Pasteur and Koch.

While the methods of pure observation have long since passed their period of greatest activity, the experimental method continues to display amazing progress and growth. The experimental method in its development and in its deeper searchings has found the use of the lower animals increasingly necessary to its study of health and disease, and has led to the foundation of physiology and pathology as independent sciences, and to the great growth of modern laboratories; thus progress in medicine has come to be identified more and more with the work of these independent sciences and with the laboratories. Research, and especially experimental research—using the term experimental as it will be used throughout to designate experiment and not necessarily experiment on animals—tends to be delegated more and more to the laboratories, while clinical investigators have been brought by circumstances to become again content with observation, the ancient, traditional method of medicine. Cleavage along these lines is distinct and is deplorable, since it tends also to split off clinical medicine from invaluable contacts with physiology and pathology, and to render these last too exclusively intent upon their own spheres of work.

The final and practical aim of almost all medical science is to maintain health and to combat disease in mankind. It is in the very nature of things that the study of disease, to be effective, must begin as it must end, with disease itself, and that all knowledge applicable to human disease must owe its inspiration, directly or indirectly, to intimate contact with disease as this exists in living man. If the vitality of medicine is to be increased, it must be clearly recognized that there is a fertile science that deals

primarily with patients, and this must be encouraged to a more vigorous growth. The science might be called "experimental medicine" if this term had not come to convey too strongly the idea of experiment on animals. It would be called clinical pathology, if the latter were not now fenced around by test tube and needle. It may be termed *clinical science*. This science seeks, by observation and otherwise, to define diseases as these occur in man; it attempts to understand these diseases and their many manifestations, and here especially makes frequent use of the experimental method. It makes definite experiments upon disease or watches the effects of experiment conducted by injuries, however these arise; it culls, or actually creates, and uses physiological and pathological knowledge immediately related and applicable to the diseases studied. Its value has been abundantly and frequently displayed in this country by such experimental clinicians as Ferrier, Horsley, Mackenzie, and Head. The very mention of these names is in effect a definition of the science that is in mind. Their work was not work that could be delegated to laboratories; it was inspired and sustained by direct contacts with disease; it was carried through in very large or chief measure by observations on sick people. The chief motive of the present paper is to inquire how this clinical science now stands. Admit that, like other sciences, it can be fostered; admit that, as with these, its development may not be left solely to sporadic enterprise, but can be hastened and guided by the creation of opportunity, and it is not hard to show that it now suffers from neglect.

We may begin to display this neglect by considering the present training and opportunities of the young worker, who, having completed his training in the preliminary sciences, aspires to clinical research. The first part of his course is set. He must become competent to manage sick people. In the past for two, in the future for three, valuable years—and while a contemporary student seeking a career in physiology is finally preparing for, or is actually engaged in, first researches—his attention is largely focused upon fitting himself to recognize a multitude of types of patients, and to tend their immediate needs. Qualification, at the least, is, and must continue to be, inevitable. Under our present-day system qualification is, however, but a preliminary; it is the later stages of preparation, through which the student must now pass inevitably, or almost inevitably, that require our very careful and serious consideration. The governing fact is that no student after qualification can expect to obtain control of clinical material, and at the same time to enjoy freedom to work upon it under adequate facilities, unless he enters upon and completes a well-defined career in a general teaching hospital. This career is of a conservative and most exacting type; it comprises not only house appointments with their valuable and early experiences of responsibility in handling disease, but a series of higher examinations, junior appointments, and heavy routine work, extending over years and arranged from one standpoint—namely, the training of a consulting physician. The man, ardent for research, who enters upon this course begins a long period of intense work, drawing him continually to its natural goal—consulting medicine; for his researches there is no certain prospect, and from this standpoint the ending is almost always barren. The reason is that preparation for success as a consultant and preparation truly suited to a career of clinical research present irreconcilable and deep-seated differences. Because the man who aspires to do research has at present no real option, but must pursue the first form of preparation, whether he desires ultimately to embark upon consulting work or not, it becomes imperative that we should examine this system and point out its defects in so far as it adversely affects a research career. And let it be said that on the matter of the suitability of our hospital system of training as a preparation for curative medicine no opinion is here offered other than to state that in many respects it is admirable for the purpose for which it has been devised; it is, however, a distinct question from the one now considered. The opinion that is put forward is that this system, and the atmosphere surrounding it, is but

* For 25 years engaged almost wholly in research work upon problems arising directly or indirectly out of contact with patients; for many of the earlier years verging upon, or partly in, consulting practice. I have had what is perhaps a unique first-hand experience of the ways of medical research within the walls of a teaching hospital, from the time when there were no facilities or aids worth the name, to more recent times when these have come. Out of this experience, and out of an unusual association with younger workers, there should be something that can usefully be said to help these on their way. What is said is not put down hastily, but is the outcome of long years of experience, thought, and discussion; the views are in fact developed from those expressed (*Morning Post*, April 25th, 1914) by myself sixteen years ago.

I would be deeply concerned if anything written in this article should give rise to the impression that it has been inspired by any personal difficulties in obtaining facilities for work in the past, other than those difficulties that have been inherent in the situation; on the contrary, I recognize and gratefully acknowledge that my work in past years could never have been carried out without the abundant good will and help of my hospital colleagues.

ill suited to the advancement of medicine by research, and reasons for this view will be considered at some length.

An obvious defect in present training is the natural emphasis laid upon diagnosis. Diagnosis makes intense and incessant demands upon a man's time and energy over the early years of his studies. All who frequent the wards of general teaching hospitals realize how dominant is its note and recognize the constant urge of their surroundings towards it. Diagnostic knowledge and ability is the first essential of good practice; it therefore becomes in teaching hospitals the most direct and universal measure of medical learning; it is the stepping-stone to personal success. Its highly competitive character is mainly responsible for the atmosphere of intenseness in these institutions. It demands intimate and long experience of all kinds of diseases and of the manifold variations and incidents that each may display. But, from our present standpoint, it lends an exaggerated importance to knowledge of numerous rare diseases, of newly described phenomena of minor interest and of newly described methods and tests. A desire for encyclopaedic knowledge is fostered and grows; it moves in the direction of detail, for the phenomena studied are of great complexity and diversity, and present few of those connecting links in the form of principle or generalization that would serve to lighten what otherwise becomes a feat of giant memory. The result, and the quick result, of this system is the loss of much that is of value in the earlier training. By unavoidable neglect, the physics and the chemistry of the earlier student days pass into oblivion or become so rusty as to be of no more than occasional service. Even physiology, more recent in the recollection and offering easier points of contact, suffers very seriously; so that few can maintain more than a little part of their learning in it, and none can hope in these years to keep abreast of its continued advance. The physician at an early stage can and does throw much or all of this knowledge to the winds; there is knowledge of far more value to his daily work, and this he assiduously and rightly gathers in.

Again, in the attempt at clinical omniscience, learning drifts into superficialities and is not profound, for there is little time for critical weighing, for sorting, or comparison, and therefore there is little correlation of observation and simplification of ideas. Intensive clinical training, as this exists, leads in general to no real grasp of the problems set by disease or of means of solving them; it is too purely observational for this.

Between the method and outlook of curative medicine on the one hand and of progressive medicine on the other there are definite, inevitable, and sometimes profound divergencies. Consider diagnosis again. A diagnostician goes into his wards with one chief goal in view—it is to place a label correctly, to say that here is a species or a subspecies that someone has described, its name being this or that. It is legitimate to define diagnosis from our present standpoint as an attempt to recognize the known. That is an end-point.

When the clinical researcher goes to his wards his purpose is not to recognize the known, but to face the unknown. Diagnosis to him is but a starting-point, and when made it must be absolute. A diagnosis that is not reached without demonstrable security is unpermissible. Thus, while the interminable daily discussions as to what malady this or that patient suffers form a vital part of the consultant's training and of his work, they possess a very limited value to the research worker. And here it is to be repeated that he who would work with proper facilities in clinical research, works perforce at the present time in institutions in which, owing to its admittedly high practical value, diagnostic acumen is the password to the control of material.

A second divergence that it is important to recognize consists in the fact that curative medicine deals with the individual, while progressive medicine is collective in its outlook. The difference is largely controlled by the factor of time. If a man, by intent or otherwise, goes overboard, his succour comes, not from him who pauses to consider causes and effects, but from him whose hand goes forth instinctively and instantly to grasp and throw a rope. To curative medicine the patient is distinctly and very

properly the very centre point of consideration, but to progressive medicine he is but an incident. To the former the patient is a human being in trouble, and the object of knowledge possessed or sought is the mitigation of this trouble, the comfort and relief of the sick man. To the latter the patient is one who exhibits phenomena of disease, phenomena to be compared and correlated with those displayed by other subjects. Samaritanism, the greatest guiding force of medicine as a whole, is in practice an ever-present and direct, and in progressive medicine a much more distant, and therefore less obtrusive, motive.

When we consider these divergencies we realize that, while curative medicine is essentially observational, it constantly repeats itself; for that reason it is safe, for both the doctor and his patient; for the same reason it tends to be in large part sterile. Progressive medicine to be fertile must from the nature of things be experimental and original. And in this connexion we must note what will be obvious to thoughtful people—namely, that experimental research upon disease may very readily come into conflict with full solicitude for the sick. When such conflict threatens, as it not infrequently does, it is unquestionably right that research should give way unhesitatingly; any other system would be intolerable. Interference giving rise to temporary discomfort, the patient willingly co-operating, may be permissible; interference calculated to bring risk, however slight, to a patient's health must remain unjustifiable. This element of conflict is to be noted first, because it has to be safeguarded under any system of work. No worker can reasonably expect to be relieved of full responsibility in the care of any patient remaining in his charge; but, on the other hand, it is to be realized that no investigator can be successful who allows, or is forced by circumstances to allow, solicitude for his patients to preoccupy his mind.

The divergence between the individualistic and the collective method has been considered here from one relatively simple aspect—namely, from that of the different attitudes that may be adopted towards a patient within the ward of a hospital. It is perhaps almost unnecessary to point out that each method of thought has its practical applications to health, and that these applications, the one considering the health of the individual, the other of the community, often come into direct antagonism. This long-recognized antagonism will not be discussed further than to point out that it really constitutes but one of the issues in the general problem we are considering.

The third and chief divergence between curative and progressive medicine from our present standpoint is, however, intellectual.

In his daily work a practising physician has little or no control over the questions presented to his mind; mainly diagnostic, they are brought to him for solution one after another in great profusion. His solutions are reached in by far the greater number of instances by no process of logical reasoning, but upon the basis of previous experience or upon the basis of what we call intuition, a faculty often remarkably developed. Inevitably his mental activities are chiefly of this kind. The questions are not only profuse, but diverse; the mind is jerked forcibly from one question to another. When problems relating to the mechanism of disease present themselves, as they inevitably do to the more active minds and in the earlier years, then, before a clear line of thought can be established or followed to a satisfactory conclusion, the mind is torn from its problem to be confronted with others. The practice of medicine from its very nature is destructive to consecutive thought; its continued practice weakens the very power to think consecutively, and therefore clearly, upon problems relating to the nature of disease, a matter very vital to progress. The atmosphere of the teaching wards is tense with effort, mental and physical, but it is not an atmosphere of sustained argument. The problems faced by an investigator, properly circumstanced, are under his exclusive control; they are never numerous and rarely disconnected; his observations are made without hurry, his thoughts are equally deliberate, and so his conclusions take shape after long pondering, criticism, and revision.

The physician is forced by his daily work to decide frequently upon an insecure basis of fact and argument.

In his work many of his decisions are frankly opportunist, many are wrong. But if most are right and many of the rest are harmless, then his work is a success, despite frequent mistakes. As a doctor he is sound, for his decisions, like the questions, are independent of one another. An investigator who draws false conclusions early in his research may see the whole structure of the work topple, for here conclusions follow in sequence and an early fallacy affects all; moreover, his work is placed on record and is submitted to close and critical supervision. To a physician, mistakes, if not too frequent, are of little consequence—they are indeed inevitable; they do not shake self-confidence as they do in the case of the research worker. Self-confidence is by general consent one of the essentials to the practice of medicine, for it breeds confidence, faith, and hope. Diffidence, by equally general consent, is an essential quality in investigation, for it breeds inquiry. Here, then, are chief characteristics, each necessary in its own sphere, each unsuited to the other. The two irreconcilables do not stand alone, but find natural companions. A natural companion of confidence is an easy and uncritical acceptance of statements of fact and of hypothesis; it is often coupled with a very wide and diverse acquaintanceship with other men's work and thoughts. The companion of diffidence is scepticism; it tends to be coupled with knowledge less extensive, but derived more from personal experience and analysis, knowledge more precise, and often more fundamental. The very essence of daily observation and thought in scientific work is the continual effort to discriminate as closely as possible between what is true and what is less true or actually false. The standard of truth attained by constant and deliberate cultivation among scientific men in their work greatly transcends that in any other sphere of human thought. It is the standard that allows no statement to pass without full qualification, without full display of its limitations. It is a standard essential to progressive work, but one highly inconvenient, and even obstructive, in the practice of everyday life. The mind of the craftsman dwells on what he knows, and he delights to use and to display this knowledge. The investigator's mind dwells in the main on the unknown and puzzling, and his eagerness is often towards displaying doubts and difficulties. The adoption of a laboratory diffidence by a practitioner would soon ruin his practice. The acceptance of the easy standard of conclusion found in practice would similarly end a research worker's usefulness.

The divergencies between practical and scientific medicine, and especially between their manner of thought and standard of accuracy, are in actual fact profound and far-reaching. They are perhaps as profound as, they are certainly not dissimilar to, those exhibited in the past between orthodox religion and science.

Preliminary consideration has now been given to the training available for the man who desires to devote himself to clinical science. It is a training developed to suit the consultant practitioner rather than the research worker. It has also been shown at length how widely the methods of work and of thought in these two spheres differ. It is the methods of curative medicine that dominate in general hospitals, and it is in this atmosphere that the bulk of investigational work upon patients is now done. Many, if not most, of the difficulties that at present face the clinical investigator arise out of the past and present control, be it direct or indirect, exercised by curative medicine over the facilities for such work and its recording. This control, absolute a few years ago, still predominant to-day, was quite natural, and indeed strikingly beneficial, in its period. If we omit consideration of quite recent times, it is true to say that investigational work in clinical medicine has been undertaken almost exclusively by, or at the exclusive encouragement of, men earning their livelihood in practice, and that practically the whole of our familiarity with the forms of disease as these are found in living human beings, and with the spontaneous manifestations of these diseases, is due to the efforts of such workers. This indispensable knowledge has been gained by the method which in the beginning of all biological science is fertile—namely, by observation

of spontaneous phenomena, the method from the earliest times most natural to, and assiduously cultivated by, physicians. Eventually, however, the fertility of this method greatly declines by a process of exhaustion, and, for those who can read the signs, this time has come in medicine.

There are, no doubt, many important forms of disease yet to recognize, important and spontaneous manifestations yet to note; but the experience of the last few decades has shown this type of work to be decreasing in fertility, while its place is being filled by other and more successful methods of exploration. It is also evident that the continued separation of disease into types can be pressed too far, and that a subdivision which may perhaps have some immediate practical use can help to conceal underlying mechanisms held in common, and thus to hinder progress in studying the nature of disease. It is abundantly clear that present progress in clinical medicine is less and less due to observing what chances to happen in beds filled by the routine demands of unselected sick people and of teaching, and more and more attributable to intensive study of selected cases in which manifestations are deliberately sought or actually provoked, or upon which new and often highly technical methods are employed, which are derived from prolonged and searching studies in laboratories. It is equally clear that successful research work in medicine is becoming more and more difficult for the man engaged in practice to prosecute; it is becoming more and more the province of men who train themselves in technical methods of work and in suitable habits of thought.

It is far from my intention to imply that work of a progressive kind cannot be carried out exceptionally by men engaged in practice; on the contrary, there are some directions—for instance, prognosis—in which investigations can rarely be pursued with equal prospects of success by any other group of workers. There is the intention to imply that in general neither the training nor the daily routine of practice conduces to the type of investigational work which progress in medicine now chiefly demands; and that the practice of general medicine by continually acceding to opportunist and hurried conclusions definitely tends to unfit a man for research. Conversely, the prosecution of research in medicine tends in general to unfit for practice; and the more whole-heartedly and continuously research is undertaken, and the deeper the research goes, the less fitted the worker becomes for the care of unselected patients. This is clearly recognized by workers themselves, and, as long as there is no reasonable prospect of careers in research, will continue to discourage them from developing full research efficiency. It is in part for this reason that research in medicine is most compatible with specialism; the opportunity to undertake progressive work is in fact often the main justification for specialism. It is not possible in research to maintain full familiarity with general medicine, with its ever-changing devices and palliatives, when so much of both time and energy are concerned in studying matters that have no immediate relevancy to practice, and in laboriously and accurately collecting data and critically forming conclusions. The idea, more prevalent perhaps in certain foreign countries than in our own, that research is a suitable introduction to general consulting practice, is not only intrinsically unsound, but has proved itself mischievous. After his qualification a man, by attempting investigational work for a short space, and thus familiarizing himself with methods and especially with the difficulties of gaining new knowledge, will often broaden his views and increase his capacity as a teacher. Ability to undertake research, however, apart from its providing a possible gauge of intelligence, is no criterion of practical capacity; and experience in research, especially on the laboratory side, has little or no value in practice; consequently, when preliminary years are spent in large part in this way, they displace years of more valuable practical experience. The proper sequel to such research is not practice but further research—in short, a career largely concerned with investigation.

The serious aspect of the widespread encouragement of transient research is that, while such work rarely possesses scientific value, yet the reporting of it has acquired a recognized commercial value, and its realization in the

latter way becomes much more often than not the real goal in view. The goal referred to is in certain circumstances a perfectly legitimate one. It is a most desirable and healthy thing that good teachers should use the medical press to inform; it is right that contacts between consultants and general practitioners should become established by this means, and that through periodicals new work of established value should be brought to the knowledge of the profession as a whole. But the present system is obviously capable of, and is actually subject to, grave and frequent abuse; the abuse consists in the redundant publication of articles intended to pass muster as records of serious original research. Now the editors of the journals concerned are for the most part either whole-time editors or are actually engaged in practice. In neither case are they trained by long practical experience of research to deal with such articles critically or to judge them well; their daily contacts are with curative rather than with scientific medicine. Thus it comes about that there is little or no attempt on the part of editors collectively to stem the tide of pseudo-scientific publication; its amount is controlled almost exclusively by the desires of writers, and the tables of our libraries groan under an ever-increasing weight of periodicals. Research is widely exploited for professional ends; records of imperfect and actually unsound work find an almost open path to the printing press; reports of purely repetitive work have reached an extraordinary and most undesirable magnitude; many papers are republished almost in duplicate. The effect of such publication has passed beyond the bounds of menace; it has grown to form a distinct barrier to progress, with which more serious workers recognize they have to contend. The baneful effects are twofold. By its mass it conceals work that has value; by its quality it undermines the general standard of accuracy in observation and thought. Current teaching in the wards and textbooks, current methods of thought in practical daily work, are built in very large part upon the basis of published articles, past and present. The quality of such articles is a matter of vital concern to professional work, both directly and also indirectly, through its influence on progress.

Main functions of workers in progressive medicine are to consolidate old and to explore new ground, to watch the general tendencies of teaching, and to bring the results of their work within the reach of practitioners and thus to practical application. From several of these standpoints there is, in considering the present position of medical research, something more to be said.

In research, one step forward leads to another; researches begin from an accepted foundation and proceed. It behoves the worker to examine closely the foundation from which he starts and to make sure that this ground is solid. In research that directly concerns patients this preliminary work of consolidation is particularly arduous. A physiological worker can in very large measure start from the records left by previous workers; in medicine, as things now stand, this procedure is less safe. The relative unreliability of clinical statement of fact, and especially of conclusion, comes from several sources. Clinicians who record facts and views are not chosen, as for the most part are physiologists, for their ability to use precise methods of observation and of thought; in medicine inaccuracies in statement of fact are, owing to the material used, more difficult to detect, penalties for mistakes are less severe; the standard of truthfulness, in the sense previously indicated, is far less rigid, than is the case in physiology. A very grave defect in medical publication, from the standpoint of progress, is the dearth of systematic statements of evidence. In a very large proportion of instances, satisfactory and recorded evidence for current clinical beliefs is either most difficult to discover or is non-existent. Far too much stress is laid upon suggestion; far too little upon the proof. In this relation it is sufficient to mention therapeutics, but the defect is not confined to this branch.

There is a great work to do in close and critical revision of present beliefs in every branch of clinical medicine. Upon the extent and thoroughness of such consolidation the future rate of progress will in large part depend,

as it has depended in no small measure in the case of cardio-vascular knowledge in the past few decades.

It is essential to concerted progress that there should be more exact definition of terms applied to disease and its manifestations, for close definition would bring increased clarity and would diminish the prevailing confusion of tongues. It is essential to progress that closer attention and study should be given to the precise laws relating to cause and effect; that they are widely misunderstood is illustrated sufficiently by a common tendency to enumerate under "etiological" headlines many and diverse causes of a single effect. It is essential to progress that observational work should be far more widely supplemented by the experimental method and by its manner of thought, and particularly that a full understanding and rigid use of controls and comparisons should develop extensively. Finally, it is essential to draw more closely together the bonds between physiology and pathology on the one hand, and clinical medicine on the other.

These activities, these reforms, have become beyond the power of curative medicine; they can be properly encompassed only by men who are not beset by numerous and diverse daily tasks, who have ample leisure for study and for reflection, who in training have acquired special and suitable experiences and have deliberately cultivated those habits of mind that conduce to successful investigation, who, in brief, make research a life work.

REMEDIES.

The present weaknesses of medicine, regarded from the standpoint of its progress, have been discussed from those points of view that suggest remedies.

The great contribution of curative medicine to knowledge of disease as displayed by mankind has been emphasized. Curative medicine has done more than that. The allied sciences physiology and pathology are its direct offspring, and have ultimately drawn their chief inspiration from it. Not many decades have passed since it became recognized that physiology must assume its own control; within a brief period its progress as a science became remarkable. Pathology has become more recently, though less completely, free, and is making not dissimilar history. These associated sciences are rapidly outstripping clinical medicine in new developments. The fault must be assigned to medicine, which, failing to pull its weight and keep abreast of the rest, threatens to disrupt the team. It is for clinical medicine, the most dependent but the most important of the group, to maintain contact and to draw renewed strength from its more vigorous progeny, and not, as now, to lag preoccupied. The remedy lies in the emancipation of clinical science from the controlling influences and demands of routine practice; for therein lies its chief weakness. Just as it became necessary to the full development of physiology, so it has become necessary to that of progress in clinical science, that its workers should enjoy independence.

The first step to be taken is the training of a group of workers with a view to their devoting their lives primarily to research, making disease as this occurs in man the centre point of their studies. The essence of the worker's training in all the concrete sciences consists in the development of correct methods of work and thought; accuracy in observation and in measurement; in biological science especially, a full understanding of the experimental method; a firm grasp of the nature and use of evidence; to know how to test and how to weigh, to know how to sift out error, to be able to judge the truth or falsity of statements. From the time of qualification the worker we are considering should enjoy full freedom to develop along these broad and recognized lines; he should have the opportunity to acquire such technical methods as may be needed in the pursuit of given problems, and freedom from the recurring fear of failure as an expert and general diagnostician. He should have ample leisure to maintain close contacts with physiology; for, if he becomes a successful worker, he will not only put physiology to good use, but, like his predecessors from Harvey's time down, he will help the growth of physiology, and will stimulate its growth in directions in which from time to time it will require stimulation—namely, into the more biological channels.

A simultaneous step must be the assurance that careers will be provided for those who train themselves successfully. And careers must be assured to individual workers at the earliest possible moment. Young and competent men cannot be expected to hazard the best years of their lives in long and special training without reasonable prospect of corresponding careers before them.

There are at present a very few academic posts, the units, concerned primarily with organized teaching in the schools, appointments of uncertain development and destiny, but offering, it is true, limited possibilities of research. Sure it is that as long as these posts are concerned with the function, important as it is, of teaching general medicine, their capacity to advance medicine by research will remain limited. There are also one or two purely research posts with full facilities; but these are personal appointments, and, being such, cannot be regarded as possessing stability.

It is unreasonable to expect, it would be unwise to advocate, the sudden creation of many posts; for there are not the adequately trained men to fill them. But the vicious circle now existing, whereby there can be no posts till there are trained men, and no trained men till they can see the prospect of posts, must be broken. It can be broken by clear statements of policy that posts will be made for those who render themselves competent for such careers. The posts must be full-time posts, for full-time work can alone ensure the leisure for efficient study, and freedom from that most distracting of thoughts, the imminence of an alternative career. The workers must be secured free and full access to the material they desire to study, and full medical charge of that material; work upon patients in another's charge fails to give sufficient freedom of control and dulls the fine edge of personal responsibility. General hospitals provide the necessary wealth and diversity of material; a teaching element in general hospitals sorts the material and renders it quickly available. The presence of research departments in general teaching hospitals stimulates teaching, and links science and art. For these and other reasons these hospitals are very probably the most suited to house such departments; but if that is the decision these departments should enjoy independence of the more purely curative departments, they must in no sense be subordinated to these, as is the strong and natural present tendency. The need is for *research physicians*, and their departments should include adequate but not abundant beds, and proper laboratory facilities; there should be arrangements for exchange of patients with other departments of the same institution, ensuring by this means supply of material and relief from the care of patients no longer suited to research. Their teaching should be limited to the demonstration of new methods and observations, and to the exposition of new ideas.

The future creation of research posts requires the present promise of financial backing; their creation in teaching institutes would require the early promise of active co-operation by these institutes; the alternative is the foundation of special hospitals or institutes.

The future of medicine lies with the younger and recently qualified generation and with their successors. It is within their power to bring clinical science in this country to a great revival. It is for them to bind themselves into a new association, which by influence and by example may carry the standard of observation and thought to precisely the same level as prevails in allied sciences, and firmly to link up with these on the one hand and with practical medicine on the other. To this end existing associations of physiologists and pathologists will not suffice them. There are problems arising out of contact with patients that are of little or no interest to the associated sciences. It is necessary that clinical science should enjoy its own intimate and untrammelled discussions. It is necessary that it should constitute its own courts of inquiry, of criticism, and of approbation. It is necessary to its full development that it should have its separate scientific societies, should control its own publications, and should be able to confine its original reports to journals restricting themselves exclusively to original matter of proper quality. It should develop its own training ground for new recruits. It should achieve independence and freedom.

EIGHT HUNDRED CONSECUTIVE OPERATIONS FOR APPENDICITIS.

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On returning from France in 1919 I decided to take careful note of all cases of appendicitis that I operated upon, first with a view to more firmly establishing, modifying, or altogether disestablishing the very definite ideas that I held upon the treatment of the disease; and, secondly, in the hope of being of some assistance in the diagnosis of its atypical forms; but I determined to publish no results until the number of cases exceeded 500. When this total was reached I decided to go on over a ten years period. This produced 792 cases, and another month completed the 800. The period covered is from January, 1919, to February, 1929. The total does not include the very many cases in which I have removed the appendix as an adjunct to operations upon the bile passages, stomach, or duodenum, nor those in which I have removed a diseased appendix when I came across it while I was operating in the peritoneal cavity for some other purpose. It is strictly limited to operations performed for appendicitis *per se*, or its results, and it includes all of those cases. An additional point of interest is the fact that in December, 1921, I made a radical change in my operative technique—a change suggested to me by Mr. Upcott of Hull—and its results have been so good that it is now my universal practice.

A glance at my tables will show that 462 cases (that is, 57 per cent. of the whole number) were either interval cases—that is, cases that had weathered at least one acute attack—or they were acute cases that had gone on to perforation. This is a very undesirable state of affairs, and it is especially undesirable when we consider that, apart altogether from the risks of the severe, and possibly fatal, complications that may develop during the course of palliative treatment, early operation offers the quickest and least painful method of recovery from the initial attack, with, of course, the additional advantage that this recovery is final.

In my opinion there are four principal reasons why early operation is not the universal practice.

1. The first, and, as I think, now the commonest, is the fact that patients do not summon the doctor soon enough.
2. The second is the fact that textbook descriptions are frequently misleading, in that the symptoms there described are in reality late symptoms, and consequently practitioners may be tempted to await their appearance before calling in surgical aid.
3. The third is that symptoms are frequently atypical, because the appendix is by no means always in the normal anatomical position.
4. It is extensively held, and, I regret to say, taught, that operation is unnecessary until palliative measures have failed; and that it is inadvisable after the second day of the attack.

My object is to combat these reasons. The first can only be combated by the education of the people, and more particularly by educating them to avoid aperients for abdominal pain. Abdominal pain means that some intraperitoneal structure is inflamed, distended, perforated, or obstructed, and the giving of an aperient in such circumstances is about as sensible as would be the waggling about of a septic finger. I am convinced that castor oil is responsible for more perforated appendices than is any other single thing or act. For the second reason the makers of books must be held responsible, and I hope they will mend their ways; while the third can only be combated by our own experience and acumen, and the fourth will disappear when those of us who are opposed to its teaching are able to convince its holders that better results are to be obtained by its abandonment.

Acute appendicitis is still the commonest of all acute abdominal emergencies, and while it is met with in patients of all ages, by far the greater number of cases occur between the ages of 12 and 25, and consequently the majority of patients are otherwise healthy and vigorous