a note just now. I thought Forster was the first. As to Dr. Connor's question: I made mention of a case in these words "a relaxed condition of the tissues peculiar to some people." The negro has a characteristic expression for conditions of this kind, "slack twisted people." Such are persons of low vitality who are placed in unfavorable conditions. Some one has suggested that there should be full correction always under atropin. It is clear to me that if you take the refraction under atropin and then without you find a considerable difference. The eye will be more myopic when the atropin has worn off because there is a tonic action of the ciliary muscle that we can not get rid of. I can not accept that proposition as an absolute truism. I have observed a habit growing among ophthalmologists in speaking of "refracting patients," and I want to put myself on record as opposed to it. A patient may be refractory, but can not be refracted. We should eliminate that expression from our nomenclature.

RESEARCH PROBLEMS OF PHARMACOLOGY.*

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The main object of pharmacology is to supply a basis for rational therapeutics. The entire medical profession has always been in accord as to the desirability of this end, but the methods of pharmacology did not at first secure universal recognition. This prejudice against laboratory experiments has now practically disappeared, partly through the advance of medical education and partly through mutual concessions. The modern physician realizes that rational therapeutics can not be based solely on bedside "experience," with its hastily and imperfectly observed and digested data. The phenomena in question are altogether too complex to be understood by such primitive methods. They need to be studied by the most delicate and profound methods over which modern science disposes. This study constitutes a large part of the research problems of pharmacology.

PRACTICAL PROBLEMS.

These problems, which pharmacology must investigate in regard to every used drug, before rational therapeutics can rest on a sound foundation, are broadly as follows: The immediate and late, the direct and indirect, modifications produced by varying doses of the drug in every structure on which it may act, intentionally or accidentally; these actions as manifested in disease; the cause of these actions; the fate of the drug in the body; the chemical constitution of the drug; the effect of the constitution on the action; the possibility of modifying the constitution of active drugs and with this their action, with consequent improvements in the drug; and, finally, the effect of the simultaneous presence in the body of this drug with others.

Almost every pharmacologic research deals more or less directly with one or another of these questions, and considerable has been accomplished in this direction. The results are, however, altogether too scanty. To realize how far we still are from a satisfactory solution of these problems, you need only examine critically our present knowledge of even the most commonly used drugs, such as strychnin, and you will see that not one of the above questions is answered as fully even as our present experimental means permit. It is well to recall that we do not even know wherein the members of a pharmacologic group—such as the digitalis group, the

bitters, the cathartics—differ from each other. As a further instance of our limited knowledge of very important points, it must be confessed that we know very little of the late changes produced by drugs and of the reactions which must in many cases follow their actions.

PROBLEMS REQUIRING CO-OPERATION WITH OTHER MEDICAL BRANCHES.

To obtain satisfactory solutions of these problems, pharmacology must undertake, in the first place, a more detailed study of the therapeutic and poisonous actions of important remedies, such as aconite, alcohol, antipyretics, antiseptics, arsenic, caffeine, cathartics, cholagogues, dietaries, digestive ferments, digitalis, diuretics, ergot, gelatin (as hemostatic), hydrastis, iodids, iron, mercury, quinin, strychnin, saline infusions, etc. These should be investigated not only by laboratory experiments, but also on carefully diagnosed and observed cases at the bedside. For this a correct diagnosis and understanding of the pathologic condition are prerequisite, and the slow progress of therapeutics is partly due to the deficiencies in this respect, deficiencies which the leaders in these branches are the first to acknowledge. However, it should be possible, even with the present conditions, to so state the problems to be investigated that the answers should be clear and decisive, and that the results of different observers would be comparable.

It is in this connection, in directing and co-ordinating clinical inquiries, that the older, experienced practitioners could do invaluable service in advancing pharmacologic research. Very great benefits are to be expected from the reorganizing hospitals for this purpose, for there are at present very few hospitals in which the experimentalist can co-operate with the clinician. As an instance, there are very few which have the laboratory facilities to make the most ordinary observations on metabolism; nor have they on their staff any men practiced in such work, or even nurses or orderlies who are trained in collecting the material properly. The limits of the thermometer, microscope, hemoglobinometer, hemocytometer and thermostat; and of test tube and burette work, have been pretty well exhausted. To make real advance, the hospitals must have added facilities, and must have a man or a staff who can devote themselves to the scientific work, without distraction by routine duties lying outside of the scientific scope.

This would also give opportunity for applying pharmacology to the investigation of the nature of disease. It must be remembered that both in pharmacology and in pathology we deal with altered cell metabolism. It is often merely a matter of convenience under which science we class a given phenomenon. Toxin-tetanus, diphtheritic paralysis, diabetic coma, and many other forms of disease are strictly intoxications. The methods used in their investigations must be essentially those which have proved so fruitful in the pharmacologic laboratories. All the diseases due to toxins, as also uremia, scurvy, rickets, gout, nephritis, indigestion, etc., need to be investigated in this spirit. This fact is not yet sufficiently appreciated. Natural and acquired tolerance to poisons is another subject in which the pathologist, clinician and pharmacologist can co-operate; but in this field pharmacology is particularly behind. We know less about the mechanism of tolerance in the case of drugs, as morphin, nicotin, alcohol or atropin, than in the case of bacterial toxins—although even here our knowledge is not exhaustive.

* Read at the Fifty-fourth Annual Session of the American Medical Association, in the Section on Materia Medica, Pharmacy and Therapeutics, and approved for publication by the Executive Committee: Drs. S. Solis-Cohen, George F. Butler and N. S. Davis, Jr.

Considerable progress has been made in the way of replacing crude drugs by their active constituents. However, we know but very imperfectly the active constituents of some very important drugs, as digitalis or ergot. Methods should be devised for the practical isolation of these in active and stable form. Other plants, not as yet in common use, may also contain valuable ingredients. The usefulness of active constituents in general could often be greatly increased by modifying undesirable characters, as instability, solubility, rapidity of action, side actions, etc. There can be no doubt that the actions of a drug depend on its constitution, and can be altered with this. Here the chemists do us a great service by modifying the constitution of drugs in almost any manner which we may desire. It is the office of the pharmacologist and of the clinician to ascertain the effect of these modifications. This has been done in many cases to the great advantage of therapeutics. The small book of Fraenkel, "*Arzneimittelsynthese*," gives very interesting proofs of this statement. It does not seem too much to hope that we may eventually be able to dispense entirely with vegetable drugs, and to produce artificially the identical constituents which we now use, and in many cases improvements on these.

This era seems, however, still very distant; we must for some time put up with the disadvantages incident to drugs derived directly from the organic kingdom. But here also great improvements are to be effected, and this offers an alluring field for the scientifically trained pharmaceutical chemist. The present pharmaceutical preparations are, in many cases, far from ideal. They often contain useless and even undesirable constituents, are unsightly, of bad odor and taste, inconvenient and prone to alteration. One of their most unsatisfactory features consists in their uncertain strength. This should be speedily remedied by suitable assay processes, chemical or "physiologic." The subject of physiologic standardization needs thorough investigation to determine the limits of its usefulness, and to establish proper official standards. When reliable assay processes have been elaborated, a great deal can be done in improving crude drugs by cultivation, selecting proper varieties, determining the best season for collecting, etc.—as is now done in the case of cinchona and of the sugar beet. Cultivation should produce a more steady supply of a higher and more uniform quality.

METHODS.

There is no dearth of problems which may be solved by the experimental methods now known to us. However, investigation directed to the improvement of old, or the elaboration of new, research methods constitutes a very useful field of research. Every advance in this direction brings with it large additions to our knowledge, by opening up new problems, or by making research more exact, more easy or less costly.

THEORETICAL QUESTIONS.

I have attempted to lay before you some problems, the solution of which would have an immediate bearing on the questions of everyday practice. Every successful research, in answering some questions, raises others. Every explanation requires further explanation, until we face the question: what is the ultimate nature of the phenomena of life? This, the ultimate question of all biologic sciences, needs to be answered before biologic problems can be said to be thoroughly understood. But just as the science of physics is not rendered useless by the fact that it can not explain the nature of matter or

of force, so the science of medicine is not rendered useless because it has not yet answered its ultimate problems. We can not afford to lay medicine aside until the biologist has finished his investigations. While we are seeking to solve the puzzle of the "ultimate question" we must be content with finding partial solutions to practical questions as they present themselves.

Both lines of investigation are open, and research may be directed either to immediate practical problems, or it may deal more or less directly with the ultimate questions. However, the two are so closely interrelated that every result in one direction aids also in the other. It is, therefore, quite possible to keep both objects in view. This will broaden the work, and will make it more immediately useful. The history of science shows abundantly that achievements in abstract directions are by no means incompatible with the duties of a practicing physician, much less with those of a medical research worker. Pharmacology has taken a prominent part in the investigation of the ultimate vital phenomena, and it holds out peculiar promise in this direction. Its object and scope go to the very root of the matter. It is the experimental chemistry of protoplasm. As ordinary chemistry studies the properties of elements or of simpler molecules by combining them with reagents, so pharmacology studies the properties of the more complex protoplasm (or biogen) molecules by combining them with similar reagents—in other words, by acting on them with chemical or molecular stimuli. The rich variety of stimuli which pharmacology supplies offers a good chance of solving the riddle of the chemical changes which underly stimulation. This requires, however, a more philosophic comprehension of pharmacology than we have at present. Our science consists as yet for the most part of isolated facts without apparent connection. These must be reduced to general laws. We know, in a general way, that drugs act selectively on certain cells. An intimate connection must exist between the constitution of drugs acting on similar protoplasmic structures, and also between the constitution of structures which are affected by a given drug. These connections are becoming dimly perceptible, but require much detail work in the elaboration of a cellular and comparative pharmacology. The modification produced by reagents must be studied by every conceivable method on vital phenomena in their simplest and in their most specialized forms. They are to be studied in their simplest variety, as exhibited in unicellular structures, as the protozoa; and also in collections of cells—in tissues—where the effects are summed up, and consequently larger, but also more complex. They are to be studied in functions of the most primitive type, as in ameba and bacteria, where the functions are simplest in kind, but are complicated by other functions; and they are to be studied in the most specialized form, as in muscle or nerve, where the functions are single in number, but complex in the development which they have reached. We must also study the corresponding modifications in the constituents of protoplasm, particularly in proteids, in organic and inorganic ferments, in toxins, antitoxins, agglutinins, etc.

As these data become known, it will be possible to connect them, and to arrange them into larger and larger groups. We will then have to inquire into the underlying cause of these groups; in other words, into the principles which determine drug action. It is very probable that the grouping will itself throw light on this question. The selective action of drugs may be in part

explained by selective absorption, in part by chemical affinities of the protoplasm. The explanation of both may be largely the same. In either case there is a co-operation of ordinary physical forces, and of other forces which have not so far been reduced to physical terms, and which are, therefore, called vital. The relations of these two sets of forces are interesting problems which find practical application, e. g., in the phenomena of absorption and secretion. We can not hope to make much advance in the study of these vital forces until the part played by the physical forces is understood. When this has been done, the remaining questions will be clearly and simply stated. It may also be expected that the inquiries necessary for this purpose will throw light on the remaining questions, or will at least point out the way for further inquiry.

These are, in broadest outline, the problems which pharmacology has to solve in the matter. It is needless to say that these ultimate problems may and must be also approached by other channels. The chemist must clear up the constitution of proteids, and eventually of living protoplasm; the morphologist must explain structure, the pathologist the phenomena of degeneration and of death, the physiologist must co-ordinate all these data, etc. Indeed, these problems are the meeting place of all the biologic sciences. To insure any conspicuous success in their solution, the investigator must possess a good measure and quality of "scientific imagination" and an extensive acquaintance with the advanced work in the several departments of natural science; he must have a comprehensive, carefully-matured plan of investigation, in which every achievement of research finds a natural place. Very few can have these qualifications. Nevertheless no investigator can be indifferent to these ultimate problems. Every well-ascertained biologic fact, no matter how isolated or insignificant, will contribute to their solution, for these facts constitute the raw material from which generalizations and laws must be built.

PHARMACOLOGIC RESEARCH FOR THE PROFESSION.

My object in laying before you this outlined program of pharmacologic research has been to show you that pharmacology has very many practical and theoretical problems which are capable of solution, and which are worth solving. A physician who wishes earnestly to co-operate to some extent in this research work, will naturally meet some difficulties, and must be prepared to make some sacrifices. But these will not usually be insuperable. There are problems, on some of which I have touched, which demand little except clinical material and accurate—and especially unprejudiced—observation. Here belongs a great deal of preliminary work in comparing the value of drugs supposed to possess equivalent actions, as also observation of idiosyncrasies, of unusual effects of drugs, etc. I may say here that I consider isolated observations of this kind as usually possessing comparatively small value. They acquire importance only when they are collected and examined critically, collated and generalized. I would suggest that the Chairman of the Section appoint expert committees consisting of clinicians and experimentalists to whom such observations could be communicated and who could report yearly on progress. I have already indicated the rôle which hospitals should have in this work. Sooner or later, the intending investigator will recognize the limitations of such empirical observations, and will desire to apply experimental methods. This re-

quires special training, and usually laboratory facilities. Experimental work is, therefore, best begun in an established pharmacologic laboratory, under the guidance of a skilled teacher. This will offer no great difficulty to those physicians who practice in a medical center. One or two hours every day devoted conscientiously to this purpose will suffice for a great deal of research. For those who live at a distance the difficulties are greater, but can be surmounted by spending a few weeks every year in the city. The sacrifice in doing this is perhaps rather apparent than real. In countries in which the public is educated to realize the value of medical research, work of this kind usually redounds in the end to the advantage of the practice of the investigator. I have not sufficient personal experience to say what would be its effect in this direction in this country. But even if we assume that it does somewhat cut down the income, every physician should ask himself whether it is not worth some cost. On undertaking this work the beginner must take care to make haste slowly. A most frequent cause of failure and disappointment is the choice of problems which are too large or too intricate. If the intending investigator has not had any laboratory training, he will do well to either take a laboratory course, if such can be obtained, or to give what aid he can to a more advanced worker, and to pick up some training in this manner. He may then attack some short and simple problem, intended mainly to familiarize him with research methods. The choice of this he should leave to his instructor. He will thus acquire the experience which will enable him to begin on more independent work, preferably along lines suggested by himself; but he should be slow to dispense with the advice and supervision of some more experienced investigator.

The students of the present generation have considerable advantage over the older practitioners, for some amount of laboratory technic is now taught in all the better medical schools. However, the colleges could do a great deal more in this direction of university work by developing the scientific spirit in their students; through the contagious example of research on the part of the instructors; by offering more extensive and more advanced laboratory courses, and by encouraging original work on the part of students. The luxury of laboring at research can not and should not be forced on every student; but the present medical curriculum is so overburdened that even the most willing student can not find time for advanced university work, and is hard put to it to acquire what is strictly necessary to his calling. The only relief seems to be in some system which cuts down the minimum in all branches, but requires advanced elective work along some lines. Such a system is quite simple and feasible in theory; but in practice it is a most expensive method of teaching, since it necessitates a large staff and costly equipment. However, increase of the cost of teaching is a common condition in all progress of medical education. The endowment of medical colleges is one of the most pressing needs of the day, if they are to keep the pace which they have set.

The efficiency of research work could be very greatly increased by more extensive co-operation and organization. Many questions remain unsolved, many good ideas are lost, many good researches come to naught, for the lack of these. The most important and far-reaching problems are too large to be solved by one man, and must be abandoned or only very partially answered,

unless co-operation of investigators exists. In this sense it would be very desirable if there were some central institute or academy which would direct the activity of all the research workers in a given field; which would assign problems and apportion means; which would direct and co-ordinate the work. The difficulties connected with such a plan are, however, so great that there is little chance of its realization in the near future. Nor is the present system, in which every laboratory is an isolated center of research, without some compensating features. It serves to distribute research over a wide field; it encourages originality, and it is a strong stimulus to competition, which is not always detrimental. These features are not, however, incompatible with some degree of co-operation. Every research worker should place himself in communication with other experimenters, or under their guidance. No better means for this purpose can be imagined than the scientific medical societies, local and national, and their sections. These can, by the appointment of committees, by prescribing of questions and by popular lectures, etc., take on themselves advisory and critical functions which would be extremely valuable, and which would lie strictly within their scope.

This concludes all I wished to say to you on the subject of "Research Problems of Pharmacology." I hoped to convince you that work in this field deserves the active support of the entire medical profession, and that this support is practicable. I have tried to give some suggestions as to the ways in which the best results could be assured. I have also ventured to give my conception of the rôles which the professional pharmacologists and the clinicians, the laboratories, the hospitals, the medical and scientific societies should play in this work. These are deep questions, and the views of others will doubtless be of the greatest value, if the paper is so fortunate as to elicit discussion. The solution to this most direct problem—how much every one of us should contribute to this work—is one which each must necessarily answer for himself.

DISCUSSION

ON THE PAPERS OF DRs. CUSHNY, TYRODE, AND SOLLMANN.*

DR. CASWELL A. MAYO, New York—I am much pleased to have the opportunity to say something on behalf of the American Pharmaceutical Association. That association is working on closely similar lines to those being followed by this Section, a fact which is shown by the character of recent communications read before the American Pharmaceutical Association, one of which was the report of the committee on the acquirement of drug habits, of which committee I am a member. This committee made rather an exhaustive study of the subject by correspondence with members of the pharmaceutic calling, and the conditions found show the great need for radical legislative measures restricting the sale of narcotic drugs. I am happy to say that this need is appreciated, and has already been acted on in Ohio and Illinois. One wholesale druggist in Ohio recently informed me that since the enactment of the anti-cocain law in that state—and the American Pharmaceutical Association is primarily responsible for the passage of the law—his sales of cocain had diminished to one-fourth of what they had been. We know that the sale of cocain in Ohio had grown to enormous proportions, but these sales have been materially cut down, and this is due largely to the activity of the American Pharmaceutical Association and the Ohio Board of Pharmacy. I mention this to show that the pharmacist, though much maligned, still has the good of the public at heart, and fully realizes the obligations under which he rests. No doubt you have seen much in the papers about the amount of adulteration of drugs that is carried on in New York City. Un-

fortunately, much of this is true, but the manner in which the facts were presented would convey the idea that all pharmacists are guilty of adulteration. All of you know that this is not true and that some of the pharmacists are reliable, and I want to enter a plea with you for the honest pharmacists. Last year the American Pharmaceutical Association celebrated its fiftieth anniversary; this organization was founded for the prevention of adulteration of drugs. This fact shows that the better element in the drug trade is active in its efforts to prevent adulteration, and the physician can help the reputable pharmacist by giving countenance only to those pharmacists who are accurate and trustworthy.

DR. HENRY BEATES, JR., Philadelphia—It is interesting to contemplate what seems to be the natural disappearance of homeopathy. In an effort to establish a law governing practice in the state of Pennsylvania, where there are three boards of medical examiners, a measure was incorporated in the proposed act which would restrict the licentiate to that system of practice representing the school from which he obtained his diploma. In other words, one licensed by the eclectic board would be restricted to eclectic practice, and so, respectively, with the licentiates of the homeopathic and the regular boards. It is interesting to know that a delegation representing homeopathy appeared before the legislature and protested against the incorporation of such an article in the proposed act. In reference to the general results which have been published and taught as the result of investigations on the physiologic action of drugs, and which investigations were conducted by the administration of galenic preparations, it is self-evident that the natural complexity of a given remedy, because of the various active principles possessed, renders these conclusions erroneous. Our physiologic action, therefore, must be practically rewritten, and such conclusions adopted as are based on the exclusive use of chemically-pure active principles. Therapeutics of a scientific character can only be practiced by the use of chemically-pure active principles. The time has arrived when these crude and unavoidably complex galenic preparations must be almost entirely abandoned.

DR. W. J. ROBINSON, New York—I am the last person in the world to discourage pharmacologic research. I know the benefits derived from this source have been enormous, but I am not as enthusiastic as I was ten years ago. Ten years ago I was a hearty vivisectionist, and was called all sorts of names by our antis; but I must say that while the benefits are great, we are very apt to be led into error. Our Chairman, in his most excellent address, has shown us that life is neither a chemical, nor a physical, nor a purely mechanical force; but I go still further and say that purely animal life is different in many manifestations from human life. In the former, for instance, suggestion plays no part, while we know what an important rôle it plays on the condition of health or disease in human beings. To mention only two examples which the essayist has given us: He said that experimental research has shown that ether in collapse is not to be used. Clinically, we know that it is a good measure when injected hypodermically. Again, pharmacologic research shows that antiseptics are of no benefit because we can not get a sufficient quantity in the blood to kill the bacteria; still, experimentally, we know that intestinal antiseptics do an immense amount of good. How they do it we do not know, but they do it. I am not opposed to pharmacologic research, but I accept laboratory conclusions with hesitation. In this connection I will read a few words written by Sir William Mitchell Banks, who was at one time an enthusiastic animal experimenter, but whose ideas on the subject have undergone a considerable change. He says:

No one for a moment will consider me as holding in contempt either experiments or commissions as such, and what I say I say with regret, but my firm conviction is that neither of them has hitherto been of the least avail in aiding us in our search for the best anesthetic or for the best method of using it. There is no possible comparison between a healthy dog or cat or any other animal, and a diseased human being. They are not in the least under the same conditions, and experiments on them are therefore useless. Take on one side a healthy retriever with its heart and arteries all sound, and without knowledge of what is going to happen to it, consequently without the least fear or anxiety. Take, on the other

* The papers of Drs. Cushny and Tyrode appeared last week.

hand, a middle-aged, or somewhat corpulent woman, with flabby heart and somewhat inelastic arteries. Suppose her to be the victim of some cancerous disease the knowledge of which has worn her out with anxiety and distress, and then let her pass a sleepless night before the operation, going over and over in imagination all the terrible process and making sure that she will die under the anesthetic. Or take your drunken, whisky-sodden dock-laborer, with tissues so steeped in drink that one is almost tempted to believe in the possibility of spontaneous combustion, with cirrhotic liver, fatty heart and albuminous urine. I can not see any parallel whatever between the healthy canine animal and the diseased human animal, nor can I see how experiment on the former will help us with the case of the latter. It is impossible.

Dr. Bodine recently reported two cases of chloroform anesthesia, both patients dying before a drop of chloroform had been administered; simply dying from fear or autosuggestion. The conditions in experimented animals and diseased human beings are widely different. Laboratory research should be continued, but we should accept conclusions with the greatest caution, and whenever bedside experience contradicts the laboratory researches we should follow the former, leaving the latter to take care of itself.

DR. W. W. TOMPKINS, Charleston, W. Va.—I would like to see emphasized that part of the address of Dr. Tyrode which speaks of preparations which should not be excluded merely because the kidneys were not doing their duty. There seems to be an impression among the profession that in Bright's disease particularly morphin should be excluded. My personal observation and experience is that morphin will give a certain degree of comfort. So in pneumonia we are told not to use morphin on account of the depressing effects on the heart. I think that all depends on the quantity that is used. Ordinarily, large quantities of morphin, particularly in pneumonia, are injurious; but, on the other hand, one of the most satisfactory means of whipping up a lagging heart is to use morphin in small quantities, such as a sixteenth to an eighth of a grain frequently repeated. Rheumatism is one of the most unsatisfactory diseases we have to deal with. How much effect the salicylates have on rheumatism I am unable to state; in some cases they seem to act like magic, and in other cases they are most disappointing.

DR. M. VEJUX-TYRODE—I agree with the one who said that conclusions should be drawn very carefully from scientific experiments, not only on this subject, but also in scientific experiments on all subjects. As regards the use of ether, its apparent action can be explained. In the first place, all irritating substances have a reflex stimulating action due to the irritation; thus the local application of ammonia, or acetic acid, or cold baths, momentarily increase circulation; but for a longer period of time it is impossible for them to act. Ether depresses the heart only when given in large quantities. There is never greater efficiency of the heart's action, but the vessels relaxed by ether enable you to feel a pulse more plainly, and you have an apparent stimulation which, however, is not real. In regard to experiments on animals and the value of these experiments, I think the criticism is a little too severe. You will find a certain reaction in animals from certain drugs. Of course, we must admit that this varies, but, as a rule, it is only a matter of degree; and, practically, that which stimulates the muscle of the heart does so not only in the dog but in all animals down to the frog. You may say that morphin produces sleep in the higher animal and excitement in the frog. This is explained very easily. There is a certain action of drugs whereby they act strongest on the best-developed functions. In the human being the brain is more highly developed than the spinal cord, therefore morphin brings about sleep, and the stimulating action on the cord is much less manifest because in the higher animal the brain controls the cord more powerfully. On the other hand, in the lower animal the action of morphin is more pronounced on the cord because there is little cerebrum to be acted on, and thus we get excitement instead of sleep. Still, the action of the drug is not changed; it is only a different manifestation in the different animals due to their degree of development. In drawing conclusions for use in disease we must take into consideration all these different actions on different organs. Dr. Robinson stated that when we have two sources of knowledge for the action of a drug, the purely clinical and the pharmacologic point of view, we

ought to choose the clinical one. I think that is a point which should be taken into profound consideration before drawing any conclusion, for clinicians have differed widely for ages and ages. A drug will be discarded for a long time and then brought into use again, but for an entirely different affection from that in which it was formerly used, and this shows that the clinical basis for action was not substantial. When sufficient experimentation has been done by reliable men, who all agree on the action of a drug, there should be no hesitancy in leaving the clinical basis and changing to the pharmacologic one.

REMOVAL OF THE FAUCIAL TONSIL;

SOME OBSERVATIONS AND A NEW METHOD OF OPERATION.*

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In reviewing the literature¹ on diseases of the tonsils, it will be found that as early as 10 A. D. the medical men of that age recognized this structure as the seat of disease, for at that time the gland was operated on and completely extirpated. In the work of Celsus (10 A. D.) we find tonsillotomy spoken of as a common practice. It was accomplished either by dissecting with a bistoury or by the fingernail of the operator. Not until much later did the profession become aware of the dangers of the operation, which finally came to be considered dangerous from the fact that cases occurred in which death from hemorrhage followed. It was in the fifth century that medical writers first advocated a modified operation for the relief of enlarged glands in this region. Aëtius (490 A. D.) advised the removal of only that part of the gland which protruded beyond the pillars of the fauces. In 750 Paulus Aegineta advised grasping the gland with a tenaculum, drawing it well out of its position and "cutting it out by the root." He limited his operable cases, however, to those in which the tonsils were white and contracted, with a narrow base. He advised against operation on the inflamed tonsil. Albercasis (1120) had the same ideas, showing that the fear of hemorrhage had gained a firm foothold in the minds of medical men of his day.

After this period the operation became obsolete, and in the work of Paré (1509) mention is made of the performance of tracheotomy where there was serious enlargement of the tonsils. He also mentions ligature of the glands, but advised against excision. In 1540 it was suggested to loosen the gland around its edges and then tear it out by grasping it with a vulsellum forceps. As the operation was not easy, and dangerous, great care was enjoined. In 1637, Serverini, during an epidemic of tonsillitis at Naples, removed large portions of the gland, using caustics when they were sessile, a hook and a semi-circular knife when pediculated. In 1672 Dionis advised against operation in any form, as in his opinion the tonsil had a physiologic importance which should not be sacrificed. He did not believe in taking even a small piece away. Many surgeons wrote adversely to the operation. Among these was Heister (1683), who said: "The operation is not only too severe and cruel, but also too difficult in its performance to come into the practice of moderns because of the obscure situation of the tonsils."

* Read at the Fifty-fourth Annual Session of the American Medical Association, in the Section on Laryngology and Otology, and approved for publication by the Executive Committee: Drs. George L. Richards, O. Joachim and G. V. Woollen.

1. The historic part of this paper was taken from Mackenzie's "Diseases of the Throat and Nose," and the anatomic part from Watson Williams' "Diseases of the Upper Respiratory Tract."