sible, and in promoting the solidarity among biologists which will make cooperation feasible.

J. PLAYFAIR MCMURRICH

COOPERATION IN BIOLOGICAL RESEARCH[1]

THE idea of cooperation in science is not new; our universities, learned societies and publications represent or involve forms of cooperation that are well established and have demonstrated their usefulness in the progress of science. Without them, progress would be painfully slow. They are, in fact, the very framework and supporting skeleton of science, without which there might be life indeed, but at most aimless amœboid movement, no dignified or effective progress.

I suppose it was not intended that the present discussion should concern itself with such old established organizations, but rather that it should deal with needs that have arisen as a result of recent growth of science and its increasing specialization, and which are not adequately met. Organization must keep pace with specialization, if the true objects of specializing are to be attained.

The last decade has witnessed the origin or farther development of institutions planned to meet the specific needs of the present, and organized to anticipate the growing demands of the future. I name various departments of the national government, the Carnegie Institution of Washington, the Wistar Institute of Anatomy and Biology, the Rockefeller Institute for Medical Research and the McCormick Institute for the Study of Infectious Diseases. These institutions recognize the fundamental importance of research for the well-being, nay, for the very life, of the commonwealth, and they also recognize cooperation as the vital principle in the conduct of research. The institution that inbreeds, that does not seek for the original and productive investigator, and that does not lend its own cooperation and secure his is on the high road to ineffectiveness.

I believe, however, that the full conception of cooperation in scientific research is not usually grasped and that the logical outcome of the principle is, therefore, not really understood: An organization may be formed that proposes to make cooperation with scientific men and institutions its main business; it may propose to seek out the original investigator wherever he may be found and to support his work in every possible way; it may welcome every new branch of scientific investigation and propose to favor it according to its importance and its needs; and yet such an institution may not be fully cooperative. It may be privately controlled; if so, its impulses are primarily benevolent and not free, guided by tradition and charter and not by the native interests of the governing body, and for these reasons apt to fail to profit to the fullest extent by the fertilizing influences of new conceptions.

The fundamental idea of cooperative organization is a free association of individuals that proposes definite ends and effects an organization to attain them. The members of the organization are at the same time the court of last resort; they may elect representatives as a board of management, or as officers of the organization; but the representatives are responsible to the organization for the conduct of affairs. The functions of such an organization are not benevolent, but free, for the members are vitally interested in the conduct of its affairs and they are themselves the governing body. The organization is plastic, responding to new ideas, so long as membership in it is determined by broad prin-

[1] Discussion before the American Society of Naturalists, December 31, 1907.

ciples and not merely by personal considerations.

Our scientific societies are the type of such organizations in science. The members make their own laws, elect new members, appoint officers in rotation from the membership, read their own papers and pay their own expenses. In turn, groups of members take on the duties of hosts to the others, and it is a genuine and deep pleasure, for are they not returning the numerous hospitalities that they have received? Such meetings are among the best events of the year in science, the most stimulating, the most fraternal; and the impetus to the progress of science is incalculable.

The Marine Biological Laboratory is another example of a free cooperative organization in biology. I believe I need not apologize for using this organization to illustrate my remaining remarks, because it belongs to the Naturalists of America and it illustrates better than any other institution with which I am thoroughly familiar certain working principles of cooperation in biology.

The government of the laboratory is vested in a corporation consisting of 64 life members and about 125 annual members, and a board of trustees of 31 members (at present, full number 35), of whom three are *ex officio*. The corporation meets annually to elect a clerk, treasurer and eight trustees to serve four years. Membership in the corporation is by election by the board of trustees. To the latter body is delegated the administration of laboratory affairs, and they appoint all officers and agents. Of the 189 members of the corporation, 129 are professional biologists; of the 31 members of the board of trustees, 25 are of the same class. The corporation has the power to modify the policy of the laboratory in any respect by virtue of its control of the membership of the board of trustees.

The government and ownership of the laboratory thus rest absolutely in the hands of American biologists, and this is the first and fundamental cooperative feature of the organization, viz., cooperation of those interested in affecting an organization for research. I believe that much of the usefulness and effectiveness of the institution rests on this basis. It establishes a thorough democracy of sentiment, which is more nearly like the sentiment of our scientific societies than that of any other institution that I know. Out of this grows a feeling of loyalty that is shared in greater or less degree by nearly all who have worked at the institution. These now represent practically every university and college of the country, most of which are represented either in the board of trustees or in the corporation.

This fundamental cooperative principle was established at the foundation of the laboratory, though the proportion of professional biologists in the membership, both of the corporation and of the board of trustees, was less then than now.

A second important form of cooperation exists between the laboratory and other institutions. Many of our institutions require the facilities of a marine station for the research work of their biological departments. Some have sought to supply these facilities independently, but I believe I am correct in saying that this method has not usually proved permanently successful, partly because of the expense of the undertaking, and partly because such an individual undertaking lacks the inspiration that comes from the presence of representatives of other institutions, who bring in new ideas and establish a congenial community.

In the year 1891, after four years of ex-

perience in conducting the affairs of the laboratory, a committee of the board of trustees prepared and sent out a circular letter requesting the cooperation of the biological departments of our colleges and universities in the maintenance of the Marine Biological Laboratory. The form of cooperation suggested was that of subscription for students' tables or research rooms, the former at $50 each and the latter at $100 each for the season. In this way the laboratory anticipated the need of biological departments for marine facilities both in instruction and in research.

The response was unexpectedly general and prompt; the following institutions subscribed at once: Columbia, Brown, The Missouri Botanical Garden, Williams, Chicago, Massachusetts Institute of Technology, Rochester, Bryn Mawr, Mount Holyoke, Vassar, Wellesley, Cincinnati, Miami and Northwestern. The majority of these institutions have continued their subscriptions up to the present time. Others have since come in, some sporadically, others continuously. Last year the number of subscribing institutions was 17. There has not, however, been considerable growth in this respect; and this is perhaps partly due to the fact that the laboratory has rarely refused a free working place to competent applicants, unless space was lacking.

It is clear that an institution may secure for its investigators by this form of cooperation the best of facilities for marine work at a cost many times less than would be required on an independent basis, with the added advantage of association with representative investigators from other laboratories. At the same time such an institution is aiding to support an organization that supplies one of the most general needs of American biology.

Special forms of cooperation with institutions are entered into from time to time. Relations with the Woods Holl Station of the U. S. Bureau of Fisheries have always been mutually helpful. The laboratory also exchanges investigators' tables with the biological stations of Canada and desires to enter into similar relations with other marine laboratories. For three years the laboratory furnished working places for twenty appointees of the Carnegie Institution and this relation was a great aid to the Marine Biological Laboratory at a critical juncture of affairs, and furnished a permanent stimulus and incentive to the prosecution of its work.

It is very desirable that such cooperative relations with other institutions should be extended and strengthened; and it is probable that this will take place in the near future. The maintenance of such relations implies that they shall be mutually advantageous. I believe that this has been demonstrated, and that, in proportion as this is realized and the spirit of research increases in our institutions, such cooperative relations are bound to grow. The principle of cooperation does not mean that all shall do the same amount, but that all shall enter into it in the same spirit and do according to their means and opportunities.

A third fundamental form of cooperation is that of and with the various subdivisions of biological inquiry. Four departments are formally organized in the Marine Biological Laboratory; in the order of their establishment they are: zoology, botany, embryology and physiology. These are, of course, broad divisions and their organization as departments does not mean that other subjects are excluded; indeed, the laboratory welcomes any biologist with a problem in the solution of which the facilities or fellowship of the organization may aid. There has been a good deal of

research done both in pathology and in psychology at the laboratory, and it is hoped that it will grow; the laboratory is ready when the time is ripe to organize departments in these subjects.

The problems in the various departments of biological research are so intricately interrelated that each department is sure to be of aid to others in many ways often curious and unexpected. This is not a matter for surprise. It only emphasizes the necessity of the broadest organization of our work if any subject is to march forward with the least degree of impediment.

Indeed, I question very much if the work can logically be confined to the subject-matter of biology. Our physiological chemists are already urging a chemical laboratory. This seems necessary for the physiologist who has chemical problems, and it may prove attractive to the chemist who has biological problems. A department of chemistry in connection with the Marine Biological Laboratory would be of distinct advantage to every other department; even the morphologist has his chemical problems, particularly in connection with the complex matter of stains.

An institution organized in the manner outlined is in a position to develop in harmony with its environment, and should be in little danger of outliving its usefulness, because it is national, representative and plastic. It should thus represent at any time the best traditions and methods of research so long as it maintains the fundamental forms of cooperation: its free organization, its free cooperation with scientific societies and men, and the free cooperation of the biological sciences within the common organization. If these are maintained, it must inevitably keep pace with the increasing subdivision of biological specialties and be guided by the community of ideas and problems in different fields.

An important result of the cooperation of biologists in the maintenance of the Marine Biological Laboratory is the direction given to research. It is true, of course, that every productive investigator aids in giving direction to research; he has students and he has imitators. Where a number of such investigators come together for considerable periods of time and problems are discussed, there tend to be a clearing of ideas and sharper definition of problems. This effects a larger circle, and the influence spreads. The best results of this sort can be attained only if people discuss their work freely, and do not keep it locked up until after publication. I believe that a considerable effect has been thus produced in direction of research, particularly in cytology, experimental embryology and physiology at the Marine Biological Laboratory. Organized efforts to direct the research of others, excepting one's own students, are apt to be futile, especially on a large scale, and in a large subject such as biology, where fundamental problems are dimly perceived or perhaps yet unsuspected. For this reason and others even more important, the institution has never prescribed or limited subjects of research.

I have spoken thus for the most part about the work of the Marine Biological Laboratory, because it illustrates in a practical working model various possible forms of cooperation; and because such a demonstration is worth much more than a merely academic exposition of the nature and advantages of cooperation.

Frank R. Lillie

Especially when only a few minutes can be devoted to a very large subject, definition of terms is of much importance.

Cooperation in education or research may be compared with efforts as dissimilar as those of an army, a swarm of bees, an ant-hill after enumerating all of its denizens, or the cells or cell-complexes of that unit that we biologists know as an individual animal or plant.

Every professor having to do with graduate instruction exemplifies a simple type of cooperative alliance in his relations with students: sometimes stating a problem, encouraging them with a nod of appreciation, or setting them to thinking by a suggestive question in the Sprechstunde to which association with them is limited, while they are left to fight out their own salvation at other times; sometimes being brain and ganglion to their muscle, and himself doing all but the mechanical parts of their thesis majors; occasionally, perhaps, drawing equally facts, inspiration and reputation from their surpassing initiative, energy and success—and then possibly being even more than an incubus.

There may be good administrative reasons why a research department should not show seeming narrowness of vision and purpose; but there is a great chance, in a department blessed with armies of capable graduates, to distribute between them the details of a broad study, the blocking out and accomplishment of which marks its architect as a master in the truest sense.

May I suggest that cooperation—the mention of which instinctively sets us to thinking of enlisting for our own purposes the effort of remote workers—may sometimes at least, like charity, begin at home; and that many of the good theses which now appear to the average critic as dissociated from one another, and without obvious environmental relation, can be given thus an excellent ecological meaning?

People with a capacity for business organization see that this simple type of cooperation might reach much farther if the various graduate schools of biology were further to differentiate and coordinate their respective effort. It is safe to say that the students of a given department, in which they stay for only a few years, can effect such a coordinated and cooperative attack on the several parts of a large and divisible problem only by chance or a miracle; but the result is quite within the power of the permanent heads of departments, if they are willing to take it up and desire to do so.

Popular interest in biology to-day centers about the plant or animal as a mechanism, the plant or animal in action, rather than as illustrating that abstract concept called, sometimes sarcastically and usually vaguely, a species. Illustrations of intra-departmental cooperation are afforded by a few of the great morphological and physiological laboratories, and in the studies which Bateson is having made in genetics and Pearson in biometrics. Who can doubt that we who admire the great men who edit *Pringsheim's Jahrbücher* are really able to characterize its editors, Strasburger and Pfeffer, as leaders in their profession almost as much because of the correlated contributory studies of their pupils as from their own great investigations?

For interdepartmental cooperation, which I understand is admirably exemplified in current astronomical work, illustrations in our own field may be taken from the now unpopular—but fundamentally indispensable—systematic branch of botany. The master mind in this field to-day, Engler, is exemplifying in a large way, by the publication of "Das Pflanzenreich," what united effort may accomplish; and our own incipient "North American Flora," under Britton's editorship, has been launched on the same lines—which have long been successfully followed in the much simpler preparation of encyclopedic matter.

There appeared at one time a possibility that Mr. Carnegie's great research foundation might knit together and unite the workers of our country into a net by which the depths of science should be dragged, but there is a bottom to every purse, and, large as its resources are, the Carnegie Institution has found the requirements of investigators to be still larger, and its policy seems to be crystallizing wisely into ample support of relatively few definitely controlled studies rather than a broadcast dissipation of its resources without such control. There is perhaps nowhere a better illustration of efficient, self-centered organization on a large working basis, but our national Geological Survey offers an equally good illustration of this type.

When the agricultural experiment stations were established, with national support and under the supervision of a national bureau, some persons thought that cooperation between the stations might be secured through the latter. Whatever the purpose of the law may have been, as conceived, its provisions, as embodied in legislation, have given to the central office little authority beyond financial supervision. Perhaps it is best that this should be so—every question is many-sided: but it is generally conceded theoretically that larger strides might have been made in agricultural science by greater concentration and correlation of the effort of the stations. The newer addition made to their equipment for investigation by the Adams act more clearly provides for this, and promises adequate results; but it is already bringing prominently to the front other cooperative needs, the most important of these referring to the channels of scientific publication.

As a matter of fact, the Carnegie Institution and United States Geological Survey do not illustrate cooperation in the sense in which I understand the word to be used this afternoon. Each is really an aggregation of workers whose tenure of office as well as their scientific activities are more or less definitely under the control of a recognized chief. Their effort, because of this control, is as certain to be productive of desired results, under good leadership, as is that in a well-managed factory—subject always to the greater difficulty of directing the activity of educated men with wishes of their own as to the application of their talent. The—not always popular and variously successful—efforts of such an organization to enlist extraneous volunteer cooperation are beset by peculiar difficulties that are not to be disposed of in a word; one of the greatest of these perhaps lying in unexpressed and unaccepted but no less real punitive power at the central desk. The ultimate coordination of experiment station activities, if effected, can only add another illustration of good administration of a self-contained organization which pays its constituents for the effort that it therefore controls.

Real cooperation, though it will always have to direct its aims with reference to those of such powerful endowed aggregates as may exist, can hardly be looked for through the latter. Its units must be the scattered men of science who constitute the university and museum forces of the country—one or a few in a place. The difficulties of effecting and maintaining such cooperation are identical in part with those underlying good government, and can be met, apparently, only in the way in which municipal problems are met successfully.

Is such cooperation desired? The native Mexican never hails the coming of the civilizer after he has once understood him and his ways; more work and greater competition are what he sees as his own portion. His question, Is greater progress worth its cost? is worthy of consideration. If it be granted that it is, it is hardly

necessary to await the coming of a leader in order to effect organization through which it may be reached. The cry for a king is answered, in the proverb of our profession, by misfortune. Our tastes and institutions are democratic. Our greatest achievement of cooperative scientific organization promises to come through the methods that we know and like. Initiative lies at the door of a few universities whose graduate departments are the home of the larger part of the biological research of the day. We really can not be so poor in men as not to be able to find an executive of ability and tact, if we desire him and search for him. Not impossibly, when found, he may prove to be so conscientious in "pottering" over his new task that his own hand will lose its cunning in technique, and opportunity for mental concentration in his own chosen field may be sacrificed to the new duty. If so, and the duty be performed, need we begrudge him the recognition that, if successful, he must win as the coordinator of our research?

The decision to form an effective research organization must be made by us if it is to be made; action on such a decision is equally ours; responsibility for cooperative success must depend in equal measure on investigators and executive. There is no strong reason to doubt that such success is attainable; but the purest spirit of democratic government, dominated by that love of advanced scholarship which makes and marks the investigator, seems essential for its permanence.

WILLIAM TRELEASE

FROM the history of biology, it would be easy to show that the idea of cooperation had not been always with us. Indeed, so late as the founding of the Naples Station, when Dohrn sought the approval and support of the venerable Eulenberg, that worthy refused to aid him, on the ground that Dohrn's plan would exhaust all zoological problems within twenty years.

Eulenberg's fears have proved unwarranted, and we no longer regard the supply of problems as dangerously limited; in fact, it is the very opposite condition that is most in evidence. The attempt has been made to meet this superabundance of opportunities by an increasing division of labor, and it is pleasant to note that the workings of specialization in the field of biology, impress us with the fact that specialization and cooperation are but two aspects of the same process.

This idea is so familiar, however, that I do not need to expand it, yet despite the general acceptance of the broad fact, the intimate nature of the relation between cooperation and specialization is often rather vaguely felt, and the present discussion should assist us to intensify our consciousness of this intimacy, and so make clearer how we may, and ought to act.

In its immediate and simple form, cooperation hardly requires to be discussed. We are familiar with such examples of cooperative work as Keibel's "Normentafeln," or the biological investigations undertaken in behalf of the alcohol commission. Matters like these arrange themselves. On the other hand, even without previous agreement, we get similar and in some ways better results, when a number of investigators independently direct their attention to the same problem, as has occurred in the study of Mendelian inheritance, or of the auto-regeneration of nerve fibers.

In attempting combinations for the solution of large problems it must be kept in mind that any arrangement which suppresses or eliminates the pleasures and excitements of the hunt for truth, or which cramps the cooperator, is in so far faulty. Against this we must be always on guard, for it is agreed, I believe, that the solution

of problems and the answering of questions is of most worth as a means for developing the workers themselves, and good plans for cooperation must be in harmony with this idea.

Recognizing these limitations, by which cooperative endeavors are necessarily restricted, and at the same time recognizing the existence of relations which might be improved by a more vivid appreciation of the cooperative spirit which underlies biological activity, my colleague, Dr. Greenman, with the assistance of his advisers, has undertaken to utilize the Wistar Institute for the purpose of such improvement.

Permit me to mention two things which have been done, and which are distinctly promising. They constitute my special contribution at this time.

On looking over the field a year ago, it appeared that American biologists lacked suitable facilities for the publication of papers which were extensive and required ample illustrations. Most journals did not feel justified in devoting to such long single communications the space and money which are demanded for them. Nevertheless, papers of this character mark a step in advance, for they result from the effort to be more critical and thorough, and plainly they deserve encouragement. It appeared, therefore, that we could advantageously cooperate with investigators in this country, by arranging for the publication of such researches, and through the appreciative generosity of a colleague it has been possible to do this by reviving the *Journal of Morphology* under most favorable conditions.

In your presence it is hardly necessary to enlarge on the significance of this step, but I have ventured to call attention to it, as one example of cooperative activity.

My second example is from quite a different field and relates to the collection and preservation of material, in order to make it accessible to investigators at large. The endeavor to do this has grown out of a movement started and fostered by His. Some years ago His persuaded the Associated Academies to appoint commissions for embryology and for brain study, in order to develop cooperative work in both these fields. His knew from personal experience that studies in these departments had reached such a stage of elaboration, and were so largely comparative, that the labor of preparing the material for a given research often overtaxed the powers of even the most industrious, and the hour had come to assist investigators by gathering and storing at central stations series of sections and related material, which should be available for all.

This idea naturally appealed to us, and we are now endeavoring to develop it, laying emphasis at present on the material which illustrates the structure of the nervous system. The plan gives to the museum of the institute something of the functions of a library, with this difference, that it handles specimens instead of books.

Such specimens are derived from two sources: from the laboratories of the institute itself, and from elsewhere. In connection with the latter source, permit a passing comment.

We all have occasion to lament the fact that many of our promising scientific youths, after an encouraging start, turn aside from pure science to follow the primrose path of some more remunerative calling. For their loss and ours, we must always grieve, but there is comfort in the thought that in a measure it may be possible to save the pieces. The youth is gone, his special skill diverted, but his preparations may still be rescued for the benefit of others.

Too often these preparations find their last resting place in some forgotten trunk

or lofty laboratory shelf, from which they are never recovered. Such loss should be prevented, and at this point the museum comes forward and offers to care properly for these materials.

To do this effectively, however, there must be genuine cooperation on the part of the investigators. Such material is not worth storage or arrangement unless accompanied by descriptive notes, notes so complete that by the aid of them the material may become really useful to a second person. The museum can offer the opportunity, but the acceptance of this by the investigator implies also the obligation we have named, and it will be of interest for us, in the course of years, to observe how far the obligation will be met. Such is my second example, and it completes the instances which I desired to present.

Before closing, however, allow me to reenforce the general statement that aside from the obvious occasions for simple cooperation, which in a measure take care of themselves, there also exist between investigators more complex relations which offer special opportunities for cooperative treatment. The examples given have been selected as illustrations of such opportunities, and indicate how some of these may be utilized not only for the general advancement of biological research, but also for the stimulation and assistance of the individual biologist.

HENRY H. DONALDSON

AGITATION in favor of cooperation and coordination in scientific research has been noticeable in recent years in many departments of science. It seems quite possible that the importance of cooperation and combination of effort has been borne in upon us by recent demonstrations of the effectiveness of such movements in industrial enterprises. However this may be, it is safe to assert that the general idea has come to the front repeatedly of late years, and it is probable that as an outcome of the discussions aroused some definite attempts will be made to utilize this principle more fully than has been done heretofore in the advancement of science. Indications of such an effort may be seen in astronomy in the organization effected for the preparation of an astrographic chart, and in the successful establishment of the International Union for Cooperation in Solar Research. As a matter of history, we are all aware that the idea is not a new one in science. This fact has been especially emphasized by Merz in his valuable book upon "The History of European Thought in the Nineteenth Century." He states that cooperation formed the underlying principle upon which the great academies and scientific societies of Europe were based. In the organization of the Académie des Sciences, particularly, the idea was kept clearly in mind, and, indeed, was most successfully applied in various important pieces of work, such as the measurement of arcs of the meridian and the determination of the variations of gravity in different latitudes. We may believe, in fact, that the recognition of the value to be derived from combined effort on the part of those interested in a common pursuit explains the existence of such societies as this which we are attending to-day. No one probably is disposed to doubt the importance of cooperation when the term is interpreted in a broad way, but if we give it a narrower connotation in the sense of an intensive combination of the scattered energies of many workers there is, perhaps, room for differences of opinion in regard to its value. The point for discussion, as it presents itself to my mind, is whether or not it is desirable and feasible in the actual work of investigation to seek for an intelligent coordination of the activities of numerous individuals, and to attempt to

focus this combined effort upon specific problems. Speaking in general terms, it is evident that the bulk of the investigation going on at present is not being conducted on this principle. The productive investigators in the various laboratories of the world are working independently. The problems that engage their attention are determined by personal interests or accidents of equipment or opportunity, and their researches are not correlated except in so far as certain problems come to the front from time to time, and by the general interest which they excite attract for a period numerous workers to a common line of study. The fortunate investigator who unearths a new idea, or devises a new method of importance, is sure to have many followers, and there results for a while a certain kind of cooperation, which is lacking, however, in the element of intelligent coordination; so that oftentimes there is an apparent waste of energy and material, due to the fact that the individual investigator is unable or unwilling to make full use of the results obtained by his coworkers. The method of independent investigations needs no apology or defense, and we should be careful not to minimize its importance. The competition that it implies encourages originality and carries with it all the benefits that accrue from differences in point of view. It is to this kind of investigation that we must look for our epoch-making discoveries, so at least we may infer from the past history of science. No one surely has any wish other than to see this kind of research grow in volume and importance in this country. Nevertheless we may ask whether it is not possible that in some ways better and quicker results would be obtained by directed cooperation. In the accumulation of reliable data, for example, by the concentrated application of approved methods of work. It needs no argument, I am sure, to convince any experienced worker in science that eventually such accumulated knowledge will cause of itself the destruction of false theories and the development of newer and truer points of view. In my own subject, at least, it is undoubtedly a fact that brilliant discoveries have come, as a rule, not as a bolt from the blue, but from a slow accumulation of diverse facts and theories which, eventually, in the mind of some one gifted worker, when the time was ripe, have burst forth as a new conception. Our individual workers of genius must be supplied with raw material in the way of facts and theories in order that their talent may be productive of real good, and it is in the accumulation of this raw material that most of us make our contributions to the advancement of science. It is in this direction also, as well as in the utilitarian application of scientific knowledge, that cooperative work, as defined above, might be depended upon to greatly accelerate the rate of progress. From the point of view here adopted the success of cooperation in scientific investigation must depend chiefly upon the possibility of devising an efficient organization for carrying it on, and obviously two essential requirements of such an organization are, first, that it shall possess sufficient dignity and authority to make its direction respected, and second, that it shall have at its disposal sufficient funds to pay for the expenses of the work. Several possible ways may be suggested for developing such a mechanism for cooperative research. In the first place it is quite possible that any body of scientific men may cooperate by a series of conferences and some sort of a voluntary compact. A notable example of an important effort of this kind is found in the International Union for Cooperation in Solar Research, already referred to. Quite

recently also the National Academy of Sciences, through a special committee, has taken steps to organize a definite plan for cooperative research upon the equilibrium conditions of chemical reactions. A similar plan might be followed with advantage by any or all of our special societies. Speaking for my own subject, there is no reason why the American Physiological Society should not, through its council or by means of special committees, plan out work of a general character and enlist the cooperation of selected investigators. There are a number of questions in physiology which bear upon public health or social conditions which might be studied systematically in this way. There is an important field also in the determination of physiological constants and the standardization of methods and apparatus which might be worked better by this method than by the accidental cooperation of individual investigators. There can be no doubt that such an effort would be well worth making even if it fell short of the full measure of success hoped for. Some data of fundamental importance would be obtained with a degree of completeness and certainty which could hardly be reached by any other method. There is another consideration of subsidiary importance which is worthy of passing notice in this connection. It is, I believe, a matter of common knowledge that in every department of science there are many able workers who remain unproductive because of a certain lack of initiative, or because they waste their time and opportunities in ill-directed efforts. Quite often these workers are the very ones who have had the most careful training in technique and are the best qualified to accomplish difficult research work. If under the influence of some central organizing force they could be enlisted in a systematic campaign of work, their training would be utilized for the benefit of science and to their own best interests. There is another class of workers, to be sure, who are so constructed temperamentally that they never accomplish their best work except as free lances—for them cooperation would be irksome and deadening. In any such plan of work as that contemplated some discretion in the selection of workers would have to be exercised by those charged with the general direction. I am convinced, however, that an earnest persistent effort to organize cooperative work is well worth making on the part of all of our scientific societies. It goes without saying that a voluntary cooperation of this character would meet with many partial failures; much that was initiated might fail to run a completed course, owing mainly to the lack of a compelling sense of obligation on the part of those entrusted with the details of the work, but on the principle that half a loaf is better than none I believe that we should all do well to follow the example set us by the astronomers. Another source to which we might look for aid in developing and testing the cooperative method is found in those large scientific bodies which have a certain amount of money at their disposal for the encouragement of research. In some cases the money controlled by these societies has been given for specific purposes and would, therefore, be difficult to administer in the way here suggested. More frequently, however, the funds are available for the promotion of scientific knowledge in general by means of investigations. As a rule such funds are disbursed on the principle of competition rather than of cooperation. They are used to subsidize individual researches, and the work accomplished, however good it may be in the single piece, is scattered over a wide field and lacks the effectiveness which might be obtained by intelligent super-

vision. The method of subsidizing is a method fitted to encourage or perhaps to discover the individual worker of talent, rather than to promote an increase in knowledge. While it has much more to commend it than the wasteful and almost useless system of granting prizes, we must admit that in its actual working it is haphazard; a blind sowing of seed, the harvest from which depends largely upon chance and circumstances. One may be allowed to question, therefore, whether it might not be more productive of good, if societies with funds entrusted to their keeping, such as the National Academy of Sciences, would make an effort to dispose of their funds in the systematic investigation of fundamental problems. The society mentioned has at hand, in its own membership, men who are abundantly qualified to select the right problems and to direct and coordinate the work of those entrusted with the several investigations. Whether such a use of its funds is permissible I can not say, but if such is the case one can scarcely doubt that by organizing systematic research of a cooperative character the National Academy could make itself a living and stimulating force in the scientific activity of this country. But among the agencies to which we may look for help in the matter of cooperative work, the two which seem best adapted for this variety of research are the laboratories supported by the government and the specially endowed institutions of the type of the Carnegie, Rockefeller, Wistar, etc. In regard to the governmental laboratories it is natural to suppose that the problems to which their resources might be applied most appropriately are those possessing an immediate economic importance. Individual scientists in the service of the government have without doubt contributed many investigations of the first importance, as they would have done under any circumstances which offered them equal facilities for work. But the specific function to which these departments are best adapted would seem to be the prosecution of investigations bearing more or less directly upon the health and wealth of the citizens of the country. I do not mean to say that it is inappropriate for the government to give its support to investigations of the more fundamental and theoretical problems of science, but at present, at least, funds from this source can probably be obtained with least opposition when the work undertaken gives promise of a more or less immediate application to the needs of life. In following out such investigations the laboratories of the government are peculiarly fitted by their organization to effect a coordination of the labors of their individual workers. On the contrary, the specially endowed institutions have a freer hand in the disposition of their resources and are less hampered by the necessity of adopting a utilitarian policy. With large means at their command and with a centralized authority, fitted to direct and control the investigations made by their scientific staffs, these institutions constitute ideal mechanisms for testing the effectiveness of cooperative research—it would seem, indeed, that in this field there lies for them an especial opportunity. The laboratories of our universities form training schools wherein young men and women must be taught to use the appliances of research, and it is almost a necessity of the case that the work shall be large and varied. The whole range of a given science should be presented and exemplified as far as possible. In these laboratories also the opportunities for individual research should be made as wide as possible—therein lies their special mission, and as a matter of fact this condition prevails at present, and has prevailed from the beginning of scientific

laboratories connected with academic institutions. If our specially endowed institutions simply follow the same general plan they will add nothing distinctive to the character of the scientific activity of the country. It will be as though one or more new universities had been organized, and the present opportunities and methods had been somewhat extended—a chance for a few more investigators to try their powers under conditions not materially different from those already existing in many laboratories. If, on the contrary, the energies and appliances of these institutions were directed toward a cooperative concentration of effort, then indeed, they would fill a need not now efficiently met by any of our existing scientific foundations. There seems to be no reason why the directors in such institutions should not exercise the power of planning a campaign of work in which all the talent and training of the workers under their control should be brought to bear upon a systematic continuous investigation from several sides of problems of importance. The policy that seems to have been adopted by the Carnegie Institution, of applying its funds to the creation and maintenance of special laboratories, such as the laboratory of nutrition and the Desert Botanical Laboratory is a welcome step in this direction. Well equipped and well directed, they will accumulate data of the greatest importance and will fulfill a function which our teaching laboratories, by their organization, are unfitted to exercise. Laboratories of this character so organized that their forces can be coordinated now upon one problem and again upon another constitute a kind of machinery which is at present lacking in our scientific workshop and from which results of the greatest value may reasonably be expected.

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THE topic before us for discussion permits of a great variety of interpretation. It would be feasible and interesting to discuss the possibilities of cooperative endeavor on the part of university departments of psychology on the one hand, and the medical departments of mental and nervous pathology on the other. Such cooperative enterprises have already been given a trial in one form or another both in this country and abroad, and so far as I am aware, with general satisfaction to all concerned. A much further development, however, is practicable, and one of the first steps in this direction, already taken by the more progressive medical schools, consists in the requirement that medical students should familiarize themselves with the rudiments at least of modern psychology. In my judgment this movement is but an expression of the most obvious common sense and I welcome it as such; but I am sure that much remains for the psychologist to do even on this level of cooperation in supplying the medical student with a selected material peculiarly appropriate to his needs.

We might also discuss the possibilities of cooperation between the departments of neurology and psychology. This is a hobby which I am glad to ride at any time. Again we might with advantage consider the possibilities of cooperative division of the field of study dealing with animal behavior as between the zoologists and the animal psychologists. But it is clear that within the limits of time at my disposal any such discussions would monopolize my part of this program and at the end find us with only one of many equally important groups of relations examined. I have therefore chosen to devote myself to a consideration of the special demands which psychology has to make upon several of the biological sciences and to a brief statement

of some of the contributions which it is in position to offer in return for favors received. I choose this course in the hope of furthering among the distinguished naturalists here present a fuller understanding and appreciation of the ideals, aims, and necessities of modern psychology.

In emphasizing the *demands* psychology makes I am by no means oblivious to the inestimable services already rendered her by the natural sciences, but I may safely assume that the more important of these services are familiar to you and I prefer to attempt to make vivid the future favors for which we look.

It should be remembered at the outset that historically psychology arose out of philosophy, and it still retains a large measure of intimate filial relations with its first parent. In recent years, however, it has been increasingly adopting the manners and point of view of its brothers and sisters, the natural sciences. This has been particularly true since the introduction into psychology of experimental methods. Although there are not at present any sharp lines of division between what may be called philosophical psychology and psychology as a natural science, the distinction in emphasis is none the less real. I shall, for the present purpose, disregard the more philosophical branches of psychology and confine myself to those of a more scientific character whose relations to the biological sciences are necessarily most intimate.

It should perhaps be added that the psychology of this stripe conceives its business as the study of the organization and operations of mind; broadly and metaphorically, this field may be called the anatomy and physiology of mind. Moreover, it regards the mind not as a remote, abstract metaphysical entity, but as a concrete vital function bound up in the most intimate connection with physiological processes which must be taken into account before it can be properly and fully understood.

Perhaps the most persistent and important levies which psychology undertakes to make are at the expense of neurology, using this term to designate the scientific study of both the anatomy and physiology of the nerves, it being understood that this includes certain phases of physiological physics and chemistry. The situation here is so obvious and so familiar as to require little elaboration. For modern psychology, the hypothesis that the mind is functionally dependent upon the nervous system has become substantially a postulate. Whatever we can learn, therefore, about the nervous system, is clear gain to us in our efforts to disentangle the complexity of mental life and to apprehend its principles of organization. We wish to know everything which the neurologists can tell us regarding the ways in which nervous currents act, how they reinforce one another, how they are inhibited, and what are the conditions of their original arousal. We wish to know also, more completely than we now do, what the great typical pathways are through the nervous system and what junction arrangements these pathways have with one another. These facts we wish for the light which they may throw upon certain recognized peculiarities of mental process. What explanation, for example, can the neurologist offer us for such peculiar facts as are disclosed by the synæsthesias, in which the stimulation of one sense organ like the ear is immediately followed by sensations not only of sound but of color or taste? What explanation can he offer for the fact that the color threshold is lowered by the simultaneous stimulation of the ear?

We should like to know what occurs as

regards neurone transmission when an act previously carried on with painstaking, conscious effort, becomes so habitual and automatic that conscious control can be, and often is practically, done away with. Does this involve the use of pathways below the level of the cerebral cortex, or does it simply mean decreased resistance in cortical neurone systems, consciousness being the expression of such resistance? Our basal conception of the relation of consciousness to the nervous system will be determined in no small measure by a conclusive answer to this question. Despite much dogmatic assertion and certain very interesting recent experimental investigations, the problem is still unsolved.

We wish also and particularly to know what portions of the nervous system, or what modes of nervous action are primarily responsible for the great subdivisions of our mental life. In a rough way we have already learned something in reply to such questions regarding the portions of our brain responsible for certain of our sensations and movements and even for certain of our ideational activities. But the details are still very hazy. We should be glad for a more definite knowledge of the differences in cortical action which distinguish sensations and perceptions from ideas and images. Here again dogmatism runs far in advance of well-organized and demonstrable fact. We should also welcome most enthusiastically any fundamental illumination as to the physiological basis of memory.

We want to know much more than the specialist can now tell us about the structure and function of sense organs. Behavior is simply a generic term for muscular movements made originally in response to sensory stimulations of one kind or another. It is obvious that we must know the characteristics of these sensory excitations before we can adequately understand the reactions which are made to them. This is peculiarly true of comparative psychology, with its interest in the mental processes of animals, but it is equally true at bottom in the case of human psychology. We want, for example, to know the structure of the retina in the lower animals and especially those whose vision is not binocularly unitary, and we also want to know the facts about the visual conduction pathways in the central nervous system in the case of such animals. This knowledge we desire, not only to enable us more exactly to interpret the behavior of such animals but also, and particularly, for the light which it may throw upon human visual processes. That animals have eyes might seem to imply that they see colors and yet, as is well known to you, evidence is rapidly accumulating to render it fairly certain that many animals supposedly possessed of color vision, in reality are sensitive only to differences in brightness or luminosity. A careful examination of the retinæ of such animals may well give us our long needed clue wherewith to untangle the puzzles of the human color sense. In the case of man it seems not unlikely that a completely adequate color theory must await researches in physiological chemistry as yet unmade. Nevertheless, we may get our start from such investigations as these just suggested upon the animal retina.

I need hardly add that a correct interpretation of animal behavior depends upon the solution of problems such as these: for example, much evidence is now at hand indicating that animals may possess developed sense organs of which they make little or no actual use under normal conditions. It is wholly problematic whether certain birds make any actual use of smell as a sense process, and yet the evidence suggests that anatomically they are equipped to respond to odors. Similarly,

certain varieties of rats make under many conditions little or no use of their eyes. For such animals these senses are almost as much of a luxury as the vermiform appendix is for man. The naïve observer of these animals, unfamiliar with these peculiar facts about the non-use of these sense-organs, must inevitably go astray in interpreting their reactions.

We can not hope for a thoroughly satisfactory theory of auditory consciousness until we learn more adequately the anatomical and physiological facts about the cochlea. Of late the widely accepted Helmholtzian theory of sympathetic resonance by the basilar membrane fibers, has received some almost fatal wounds and none of the substitutes as yet proposed is wholly convincing. I might unfold a similar tale of defective present-day knowledge in the case of each of the senses, and the psychologist stands ready and eager to appropriate with gratitude whatever can be given him here. Are there without doubt special end-organs for the temperature sensations, and for the several forms of contact? What is the implication as to our bodily sensations of Head's recent experiments on sensory nerve regeneration and the return of sensitivity after nerve section? Are there specialized end-organs for the four elementary tastes whose psychological and physiological distinctness seem so certain? Is there no differentiation in the olfactory end-organ comparable with the bewildering profusion of olfactory sense qualities? The answer to these and to dozens of other similar questions must be obtained before psychology can be satisfied with the finality of its analyses and explanations of sensory consciousness.

From comparative anatomy and physiology, as well as from embryology, we look for much helpful light on the circumstances surrounding the appearance and growth of intelligence. Time fails me, however, to attempt to specify details.

Modern psychology gladly acknowledges a great debt of gratitude to the alienist and the pathologist. The study of insanity and nervous diseases, chaotic as are the present conditions in those branches of medicine, has been of indisputable moment to psychologists. Moreover, we recognize that the studies of the neurologists and the pathologists are mutually indispensable to one another and that answers to many of our questions already formulated must come from both these scientists in order to be complete. The study of alternating personalities, of hypnotic phenomena, of somnambulism, and the positive insanities, to mention only these, has let in a flood of light upon the complexities of organization in the mental machinery which could not otherwise have been attained. The questions which the psychologist still has to put to these colleagues of his are so numerous as wholly to baffle summary. They can only be illustrated.

Is the disintegration of the self found in the so-called alternating personalities simply an exaggeration of normal conditions, or is it wholly pathological in the sense in which scarlet fever is? Already intimations of the final answer to this question are looming large. What is the origin and inner character of the so-called "phobias," so characteristic of our day? This man is a neurotic hypochondriac; that man a neurotic recluse. Have these diseases their foundation in specific lesions of one or another kind; are they expressions of hypertrophy of normal physiological functions, or are they purely psychic? What is the physiological basis of suggestion often employed in treating such conditions? Perhaps, if a satisfactory reply could be obtained to this last question, our medical friends would be less generally willing to hand over to Christian

Science and Faith Cure healers the undeniable therapeutic values of this process. Medicine, as well as psychology, could therefore profit by the answer. What is the physiological foundation of hypnosis? Answers to questions such as these would set us far on our way to a better understanding of the mind and its connection with the body. With due modesty I may as a psychologist say that the issues raised here concern matters about which our present knowledge is almost exclusively psychological.

I trust that in this brief sketch I have made it clear that psychologists are watching with utmost eagerness a wide range of neighboring scientific territory from which they will purloin anything of value to them if not prevented, and I hope I have also shown that their needs are many and genuine and definite. But what has the psychologist to offer in return for the blessings of natural science past and future?

I shall make my reply very brief and confine it to a few words dealing first with the general advantages which psychology offers and second, to the specification of a few more concrete details of service.

One very obvious and simple service which the psychologist would be glad to render his scientific colleagues is the tender of a knowledge of a few simple psychological distinctions and a reasonably satisfactory terminology in which to clothe them. It is depressing to the psychologist to find his brethren still using ideas and terms which were becoming obsolete in psychology at the beginning of the nineteenth century. It may readily be granted that the terminology created for strictly psychological purposes may be found unsatisfactory in some particulars when employed in psychiatry or neurology. But the correct alternative to choose in the face of this difficulty would not seem to be the naïve creation of a new terminology, nor the utilizing of one already outworn, but rather the modification of the best one in vogue. I would not seem to imply that psychological terminology is a finished and satisfactory product. Quite the reverse is true, but it has some relatively stable features to offer and some good reasons for offering them. Moreover, there are certain elementary psychological ideas and principles which are quite firmly established, and should be familiar to every scientist whose work requires him now and again to indulge in psychological statements. Such terms as sensation, perception, imagination, memory, attention, association, conception, reasoning, emotion, and volition have sufficiently fixed and definite meanings attached to them to render their use perfectly practicable. Without such knowledge it is hardly possible to make any extended statement about mental facts without becoming involved in needless terminological difficulties.

I trust my attitude will not be misunderstood. I speak in sadness, not in irritation; in sorrow, not in anger. Open the standard anatomies of the nervous system and you will not infrequently find diagrams of the cerebral cortex with one set of areas marked "sensory" and another set marked "psychic," as though sensations were not psychic and as though psychic meant anything in particular anyhow. What kind of psychic? Emotional psychic; ideational psychic; volitional psychic? To classify the functions of a region as psychic is much like classifying the people of the United States as human. It may be true but it is not illuminating; and if the term psychic is employed as significant simply of something not immediately sensory in character it is an unpardonably vague term for which good substitutes are easily available in psychology. If it is used as a cloak for ignor-

ance, then the term *"unknown"* should be substituted for it in the regions concerned.

There are many general advantages of a similar character to be gained by the scientist from a slight acquaintance with psychology, and not the least of these is perhaps the more vivid appreciation on his part of the elaborate technique which modern psychology has worked out to meet her needs and the substantial foundation which now underlies modern psychological doctrines. In so enlightened a body of scientists as this which I now have the honor to address, there is undoubtedly no such shallow misconception of the attainments of modern psychology, but there are many who still dwell in the darkness of intellectual night so far as concerns this matter.

I shall select simply a point or two to illustrate the more specific and particular ways in which psychology may contribute to the natural sciences. The contemporary naturalist often has occasion to make use of the psychological principle of association and I would urge on his thoughtful consideration the psychological analyses of this feature of mental life. The bland naiveté with which he often uses this principle makes one gasp who has ever faced its multitudinous complexities. It is a safe surmise that Aristotle had forgotten more about the principle of association than certain modern naturalists have ever known. It is respectfully submitted that it is not good common sense in the use of a principle like this wholly to disregard the elaborate analyses of generations of previous workers. Again, it is out of the question for the neurologist, for instance, studying the function of the auditory end-organ apparatus to go far or safely without a knowledge of such generally unfamiliar phenomena as those of combination tones with their many varieties. Similarly the physiology of the visual processes must remain lamentably incomplete in the hands of an investigator unfamiliar with the important facts of color vision: for example, the peculiarities of such vision under dark and light adaptations respectively, the phenomena of contrast, peripheral retinal color deficiency, the peculiarities of peripheral and foveal space impressions, and so on. In other words, psychology is in a position to furnish a systematized statement of vast ranges of mental phenomena which not only *may* be taken into account by the neurologist, but which *must* be taken into account before his science can approach completion, because these phenomena constitute many of the concrete facts which it is his business to explain. In other words, psychology—or some other science doing her work—sets many of the most important problems for the other biological sciences. Facts which she finds, they must take account of and, if possible, explain.

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SCIENTIFIC BOOKS

The Value of Science. By H. Poincaré, Member of the Institute of France. Authorized Translation with an Introduction, by George Bruce Halsted, Ph.D., F.R.A.S. With a Special Prefatory Essay. Pp. iv + 147. New York, The Science Press. 1907.

In calling attention to M. Poincaré's masterly little book, I propose—these columns being what they are—to consider rather its general significance than to traverse the technical problems of logic and epistemology which it raises. For scientific workers at large, the *tendency* of the monograph happens to be the most important thing about it. It adds another to the numerous contemporary evidences that scientific investigation, when subjected to reflection, and viewed with regard to its methodology and intellectual presuppositions, leads unavoidably to difficulties that belong in the field of philosophy. No doubt, I may incline to exaggerate this view, but, as