*THE SUMMER LABORATORY AS AN INSTRUMENT OF BIOLOGICAL RESEARCH.**

THERE are three kinds of summer biological stations:

I. Laboratories devoted to instruction alone. Of such a laboratory it may be said that it has no real reason for its existence. Its work can be done better, as a rule, in the existing college and university laboratories.

II. In the second place, there are some summer laboratories which are devoted to research exclusively.

III. The third class will include those combining research with instruction. Here belong our best and most flourishing institutions. The combination is good for both sides, and it is absolutely necessary to the instruction if it is to be maintained at a high standard of efficiency. The instruction may be, and in general should be, different in method and usually in subject matter from that given in the schools. In biology it is generally the natural history or ecological side which receives special emphasis, and rightly so, since these are subjects which can hardly be pursued under other conditions than those by which we are here surrounded.

I shall speak to-day chiefly of the research function of the laboratory. I do so the more cheerfully knowing the excellent record which this station has behind it in the matter of research, and having all confidence that it has a much wider future from this day onward.

The necessary conditions for research in biology fall into two great groups:

1. Material conditions—suitable buildings, apparatus, etc., a rich and diversified fauna and flora conveniently near and an environment sufficiently varied to promote ecological and experimental researches in biology. All of these conditions are obviously abundantly fulfilled in this laboratory; but I shall not attempt to enlarge upon them, as that would be to trespass on a theme better treated by our director himself.

2. There are, in the second place, certain conditions of successful research which we may term provisionally the subjective conditions. These are really of far greater importance than the material conditions, but more difficult to control. I propose that we consider some aspects of these conditions for a few minutes in the hope of gaining thereby a better insight to the real purposes of a lake-side laboratory of natural history and thus securing greater efficiency in our work here.

It is the investigator's own self—his bodily and mental organization—which is the most important instrument of research with which we have to do. Our problem, then, is nothing other than how best to bring this apparatus up to the highest state of efficiency. This laboratory, whose guests we are to-day, is clearly provided with all the material facilities for a great research center. It will make the most of these opportunities and enlarge these facilities, without doubt. Its staff is composed of tested and approved research workers. Their efficiency too is doubtless capable of further development. I ask, then, what may we expect from the laboratory in this far more difficult, yet vitally important, field of activity—the culture of the investigator himself?

Before we attempt to formulate our aims, our ideals, of what a laboratory should do for its investigators, we may properly inquire into the fundamental nature of research in general.

A few weeks ago a young man whom we may call Linton (I hasten to explain that he is not a disciple of mine) came to

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the door of my laboratory with a parcel under his arm and asked if he might have the use of my best compound microscope for a few minutes. I invited him in and he proceeded to unwrap a dead crow which he spread out on a table and with a sharp pocket knife dissected rapidly until he had exposed certain tendons running from the bases of the primary wing feathers to the joints of the wing and thence to the sternum. Then calling attention to these tendons, he asked for his microscope. I suggested that it is not customary to use the compound microscope for that class of work and inquired whether a dissector would not be better adapted to his purpose. To this he assented and forthwith asked for my best dissecting microscope. I supplied his wants and left him. Returning in the course of half an hour I found him still examining carefully the whole length of these tendons and their attachments. Looking up he asked if I knew of any force other than that of the atmosphere and the bird's muscular movement which keeps the bird suspended in the air. Upon my replying in the negative, he proceeded to elaborate an electric theory of flight, the sternum being conceived as the generator (he reports that he has occasionally received slight electric shocks from the breast bone of freshly killed birds), the tendons shown by his dissections being the conductors and the entire surface of the spread wing being thus highly charged with electricity. At this point I ventured to inquire how the electric charge acts to keep the bird afloat. This he did not know, but thought it ought to do so.

Now, here is a bright man, full of ideas and enthusiasm, willing to do much hard labor to work out his conceptions. Why are his researches abortive, their only value being to afford a hearty laugh and a few minutes of interesting diversion in the midst of his daily grind to a jaded college professor? Here is valuable energy—for the man is no fool, he is simply untrained—going to waste merely for lack of correlation. The man is unable to put his own ideas into relation with the great body of science, and hence he is still in blissful ignorance of the fact that he has succeeded only in making a laughing-stock of himself. The most elementary knowledge of electricity would of course have shown him the absurdity of his proposition. And when we investigators of mature experience publish scientific vagaries or fantastic theories (as we all do sooner or later if we do much really creative work), our failure is traceable in the end to the same defective correlation as is Linton's electric theory of flight. So soon as the facts are all in, such excrescences of scientific fancy are trimmed off automatically.

If now we attempt to interpret research in terms of the current dynamic conceptions of consciousness and of things in general, we may conveniently subdivide the history of any given special investigation somewhat as follows:

I. We have first the preliminary steps which we may characterize, to borrow a medical term, as the prodromic stages. These include the investigator's whole previous life, the sum-total of his experience so far as it has affected his mode of thought and his fund of ideas. His consciousness may be conceived as for the moment in a state of relatively stable equilibrium. This equilibrium, however, is not perfect. His fund of knowledge is not complete, nor even symmetrically incomplete, but it is full of gaps.

To some men these gaps are not significant. Their phenomenal world is completely filled (no matter how contracted their horizon), as one fills out automatically and unconsciously the blind spot on his own retina. Such people never become investigators.

To some other people, on the other hand, every break in the continuity of our knowledge is a distressing thing; merely to state a problem is to call into being a center of unrest. Our investigator manifestly is of this type. He looks at the world as in a broken mirror, full of irregular gaps and distortions, every one of which is to him more or less painful. We can not therefore speak of the resting stage of his consciousness as in perfectly stable equilibrium.

II. In this condition he receives some impression—let us say, to take an illustration from my own recent experience, a peculiar movement of the catfish hitherto unobserved—which serves to direct attention to some one of these tender spots of consciousness, in other words, to some biological problem. The known facts bearing on that problem become focalized in the investigator's mind, and that which was before a consciousness in relatively stable equilibrium becomes a tensional system in very unstable equilibrium—it becomes, in fact, at-tention.

Our student has now selected his problem, let us say the function of certain cutaneous sense organs upon the barblets of the catfish, the stimulation of which he supposes may have occasioned the peculiar movement above referred to.

We are in the habit of saying that the 'research' now begins; as a matter of fact the research is already well under way. Well begun may be more than half done in this case. The meaning of the problem and the value of its solution will be determined by the character of the initial tensional system, and this in turn rests upon the investigator's wealth of mental content in what I have called the prodromic stage—in common parlance, upon his preparation for research. The observed fact in the beginning would have suggested no problem unless his previous fund of experience had permitted a prophetic insight into its meaning—'in-sight' as distinguished from 'at-sight,' or the meaningless gaze of the untutored mind, to use a happy antithesis of Dr. Paul Carus.*

III. Now, tension means dis-harmony. The solution of the problem involves the release of this particular tension in consciousness and the return to equilibrium, or the correlation of the new fact observed with the preexisting body of fact. The probability is that this can not be done directly; they do not fit together. The student now proceeds to accumulate new facts suggested by those already known, with the expectation of being able finally to complete the system, effect the correlation, and so relieve the tension. This is observation and experiment.

His fish, to return to our illustration, is placed under experimental conditions and its reactions noted under a great variety of modes of stimulation, the observer's mind being always alert for an observation which correlates with facts previously noted.

IV. This process involves the dissociation so far as possible of fact and meaning, of observation and interpretation, and this dissociation in consciousness of things which in nature actually belong together *is* the tension. The facts must be objectified, personal equation must be eliminated or allowed for, prejudice avoided, and when the series of facts as thus objectified is sufficiently complete so that fact hangs with fact and the whole forms a natural unit, then the interpretative series (which in the first place set the direction of the research) again becomes dominant, fact and interpretation fit to-

* In the 'Primer of Philosophy.' Cf. the 'intuition' *vs.* 'at-tuition' of Laurie.

gether, the tension is relieved, correlation is effected, the problem is solved.

The student finds, for example, that the observed reaction of the catfish under consideration is produced by the chemical stimulation of certain taste buds on the barblets; in other words, that the fish tastes with the barblets as well as with the tongue. The steps in the observational series are these: The movement made when the barblets touch food is to turn and snap up the morsel and swallow it. It is a sensori-motor reaction similar to those from the eye and nose. This is our first correlation. It is not a tactile reaction, because it does not occur after contact with a tasteless object such as gelatin. In the same way the participation of other sense organs may be experimentally eliminated until chemical stimulation (taste) is left as the only possibility. It is a fact of observation that there are sense organs on the barblets which resemble in structure the taste buds in the mouth; the two groups of sense organs are known to be supplied by similar nerves both of which are connected with the gustatory centers within the brain. Experiment now shows that the two sets of sense organs have a similar function, and the coordination of our factual series with the interpretative series is complete—the reaction is a gustatory reaction.

The working hypothesis in the research from this point of view is an anticipation of the 'meaning' of the factual series in advance of the discovery of all the facts. The value of the hypothesis is simply to point the direction for the accumulation of further facts. If a coherent series of facts can be found which coincides with the provisional explanatory series, the solution is found; if not, another provisional explanation must be produced.

In all these cases the test of fitness is the coordination of fact with fact, and fact with explanation. That done, the gap in our knowledge is filled, our world of knowledge becomes by so much larger, and the painful tension of disconnected experience gives place to the satisfaction of correlation and integration of the new with the old and consequent broadening out of the meanings of life, which is really creative work. In proportion as the newly acquired facts and interpretations can be broadly correlated with the existing fund of knowledge, in just so far is the research really great.

Now the ability to make this correlation at the end is the same kind of ability as that which is necessary at the beginning to get the good 'point of view' from which to choose a fruitful problem and shape efficient working hypotheses. No liberality of financial endowment, no profusion of material equipment, no wealth of faunal and floral environment, will compensate for its absence. Nothing can come out of the place which is not inherent within its men. Our first aim must, then, be to safeguard the investigator himself.

To return, then, to our theme, there are very practical ways in which the summer laboratory may contribute, as no other can, to the culture of the investigator himself, and it is this function which I conceive to be the most important justification for its existence.

Most of us who go from home to these biological stations leave well-equipped laboratories of our own, and not infrequently the particular research in hand can actually be done more conveniently at home than abroad. It has been my custom for years, at the beginning of the summer, to pack up my slides and other research materials and carry them at considerable expense of time and money to a seaside laboratory where two or three months are spent, not in the study of marine types, but in the elaboration of my collections made in Ohio and for which my own laboratory furnishes the necessary equipment better than those to which I have made my annual migrations. Nevertheless, I have always felt that the time and money so spent were profitably invested—indeed the expenditure has actually been necessary to the highest efficiency of the research.

The apparent paradox is fully explained when it is remembered that the man can put into the research no more than he has in himself. The investigation is no external thing which he makes; *it is his life,* and the man must first be broadened, deepened and rounded out, if this manifestation of his inner life is to have real power, is to become an efficient factor in the world's work. This the summer laboratory can do, and can do with small material outlay, if only we go at the thing in the proper way.

Most of us are closely shut up during the nine months of the school year in the confined atmosphere of our own laboratories, often in almost total isolation from our confrères with whom we are (or should be) collaborating in the prosecution of our researches. In such a case one naturally slip into ruts, and sooner or later, unless the situation is relieved, his work will give evidence of a mildew, or perchance a dry rot will permeate his best work, so that freshness, originality and virility gradually give place to dreary routine or to grotesque fantasy.

That this is not the inevitable outcome of the sort of solitary confinement to which many investigators, particularly those in the smaller colleges, are of necessity condemned is clear from the history of many of our most illustrious research workers whose most valuable contributions have been produced under just these conditions. But the fact remains, that just in proportion as our work will prove of real and permanent value, we must keep ourselves in touch by some means or other with the outer world, with the current tendencies and advance movements of research in cognate lines. This can of course be done in great measure through the medium of the press, but nothing can take the place of real, vital contact with other investigators in the flesh. Here we get not only inspiration and stimulus in general, but often items of direct value to our own work years in advance of their publication.

The summer laboratory should be a clearing-house of scientific ideas, not merely a hotbed or forcing house for budding researches. To meet this need it is evident that the greater the diversity in personnel and range of interests represented the better. That which the university student prizes most is the intimate daily contact in the lecture room and laboratory with his instructors. In the properly organized summer biological station every worker comes into that same sort of relation with every other worker, and this, I take it, is the best that the station can offer to its patrons. To attain the highest efficiency there must, therefore, be sufficient flexibility of organization and diversity of interests represented to correct the tendencies toward intellectual in-breeding which we find in most of our university and college laboratories and to secure a sort of cross-fertilization of scientific organizations.

Regardless of the individual investigator's problem and method, he can well afford to utilize such opportunities; indeed he can not afford, except in unusual cases, to neglect them for long periods, if he would retain his intellectual tone and elasticity. The station, in short, is an exceptionally favorable aid in effecting that breadth of view and perfection of coordination which we have seen to be the keystone in the arch of scientific achievement.

It is a source of congratulation to us, the members of this laboratory, that these liberal principles are clearly at the foundation of our present organization. Our director has made it very plain, not only by word of mouth, but much more forcibly in practical ways, that it is to be the policy of our laboratory to secure the widest cooperation among all the men of science of our state. To this, as the representative of organized science in Ohio, I have pleasure in responding with equal cordiality that it will be our purpose to share in the great work here established to the full extent of our ability, by attendance when possible, and by sympathetic interest at all times. While we are the gainers by this liberal hospitality offered by the laboratory, it is certain that the laboratory in thus casting its bread upon the waters will find it again after many days.

Permit me in closing to quote a paragraph from the article recently published in the special Christmas number of *Mind!* entitled, 'Specimens of the Critique of Pure Rot, from the remains of a Philosopher, by I. Cant.' "Let us begin by inquiring into the possibility of Rot in general. That Rot exists you may take my word. And there are two kinds of it: damp rot and dry rot, besides certain fungoid growths." To which of these categories this effort of mine belongs, I leave you to judge—whether it is damp rot or dry rot, or merely a relatively innocuous fungoid growth which will deliquesce with the rise of to-morrow's sun.

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THE INTERNATIONAL CATALOGUE OF SCIENTIFIC LITERATURE.*

It may be well to briefly outline the history of the International Catalogue of Scientific Literature before recounting the condition of the work at present.

The original suggestion of an international catalogue came from Professor Joseph Henry, first secretary of the Smithsonian Institution, who in 1855 called the attention of the British Association for the Advancement of Science to the great need of a work of this kind. The idea was in advance of the times, and not until 1867 did it bear fruit in the publication by the Royal Society of the well known 'Catalogue of Scientific Papers.' In this publication Professor Henry was given due credit as the originator of the idea, but the work itself was only in part the realization of his plan, dealing as it did with serial publications only and indexing them by authors' names alone. However, with this start the plan lived and progressed until 1894, when the Royal Society, feeling that the time had come to improve the plan of their catalogue, and assured that this could be effected only by international cooperation, addressed a circular to the learned societies of the world, bringing the matter to their notice. By the advice of the societies responding to this circular the Royal Society through the British Foreign Office invited the governments of the world to send delegates to a conference to be held in London in 1896. At this and the two following conferences of 1898 and of 1900 the plan took shape and it was decided to start the work with a classified subject and author catalogue of all original scientific literature beginning with January 1, 1901. The following named sciences were to be included within the scope of the catalogue, one volume a year being devoted to each of

* Read before the Bibliographical Society of Chicago.