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THE OUTLOOK FOR PSYCHOLOGY[1]

I WISH to call your attention to certain features of the situation in psychology at present, pointing up, so far as I can, the indications for the future. My summary and my prediction can not be 100 per cent. accurate—perhaps not 75 per cent. accurate. I believe, however, that it is useful to pause in our laboratory work occasionally, and, seated in our arm-chairs if you please, review the results of our endeavor. I shall emphasize the laboratory, its methods, training and products, because the laboratory is the center of true psychological activities, and nothing which is not founded on the laboratory in the fullest degree possible is worthy of scientific consideration in our field. field.

I must emphasize the fact that the laboratory method has justified itself under difficulties during the last twenty years. There is always dissatisfaction with the laboratory method. In the first place, it is "academic." But partly through the results of psychologists, and partly through the achievement of other "academic" men, this term has ceased to be an insult, and is a badge of merit and accomplishment. Let us call the laboratory method "academic"; one of the most striking features of the last two decades is the ephemeral life of movements claiming to be scientific but attempting to get away from a sound academic foundation.

In the second place, the laboratory method is slow. Men outside are impatient, and want quick results. They can not wait for the results of the tedious scientist, pottering in his laboratory. We must be *practical.*

Well, I could point out the fates of some of these practical movements. They are gorgeous while they last, but not beautiful when they burst. They are great opportunities for the charlatan and the sciolist, but unfortunately they have diverted the energies of many really able men.

The laboratory method receives its justification when these "practical" booms burst, having hastened to application when there was nothing to apply, and the shattered legions come limping back to inquire

[1] Address of the retiring vice-chairman before Section I, of the American Association for the Advancement of Science, at the New York meeting, December 29, 1928.

whether the laboratory has yet anything for them to apply in a new campaign. The laboratory method *is* slow, and the laboratory man is exasperating; but somehow, their products have to be awaited. I could draw a striking parallel with medical programs in this regard, but it is too obvious to need elaboration.

A third complaint against the laboratory man is that he is not only aggravatingly slow, but that he is pig-headed, in respect to practical problems. He does not apply himself closely enough to those matters for which an immediate use can be found.

We want the psychologist to find out how to reduce crime, and the exasperating fellow spends his time month after month in putting rats through a maze or in chronicling the rodents' amorous behavior. We want the psychologist to find out the constitutional mental differences between whites and negroes, and he merely amuses himself and wastes university money finding out whether three-year-old children are materially concerned as to whether they wet their beds or not. It would seem imperative that the psychologist should devise means of helping the neurotic person to gain control of himself, and to save him from the exploitation of the commercial practitioner. But the wretch just loafs around his laboratory determining whether a deaf and dumb victim wriggles his fingers or not while he thinks. And so on, and on. We all know these complaints. We know also that from time to time some half-baked psychologist swells up with a practical, non-academic, idea which is going to solve some vast problem, and then spatters the rest of us disagreeably when he blows up.

Unfortunately, where the psychologists have been unwilling to be charlatanistic, others have not been so reluctant. A man may be a charlatan, although he has high-sounding principles, if he is ignorant. He may, on the other hand, not be ignorant, and still be a charlatan. I don't for a moment suppose that all laboratory men are moral, but it is still hard for them to be charlatans.

There is no doubt that we are remiss in not making greater efforts to develop the field of abnormal psychology. I am not unaware of the work of Lyman Wells, Bridges, Franz, and certain others. But on the whole, we have left the field to the psychiatrists, and they, leaving their field of the care of the mentally diseased, have moved boldly into the field of abnormal psychology. But, unfortunately, the psychiatrists have been little interested in acquiring either psychological knowledge or psychological technique. Hence we have a rampant development of theories and nostrums. Out of this has grown the exploitation of mental hygiene, which has already begun to have an unpleasant odor, as must any extensive attempt to apply what none of the appliers knows. There is probably no expanse of fiction as rich in imaginative products as is the mass of material being put before the public as mental hygiene. For this the psychologists must shoulder some of the responsibility, for it is evident that the situation can not be cleared up until laboratory psychologists apply laboratory methods and psychological principles to research in this field. The extension of our work in this direction is not impossible; and although there are many psychiatrists who fear our entry, and oppose it where it seems threatened, there are plenty of sound psychiatrists who are ready to cooperate with us, and who beg us to begin. Right now, the need is for training men for this work, and for providing research jobs for them when they are ready to work. The universities will find it very difficult to do this, until the way has been broken. Some financial assistance from outside the universities is absolutely essential. The adequate ways of applying this are not far to seek.

It is charged, not only against laboratory men but against academic ones in general, that they have not broad human outlooks, or practical abilities. It is true that we have a lot of laboratory lizards, impractical men, who are nevertheless very valuable. But against our group in general this charge has ceased to be even an engaging slander. There is plenty of proof since 1917 that the outlook of the academic man is second to none in breadth; and that if he succeeds academically he has practical ability that he could capitalize more abundantly in business. Even the occasional laboratory man who hasn't a broad outlook is that way not because he is a laboratory man, but because he is just the limited kind of fellow.

In short: the laboratory method has justified itself in spite of great obstacles, in psychology as elsewhere. The mental test movement which made its bid for the solution of all mental problems by a cheap and simple method, short-circuiting the laborious laboratory method, has suffered notable changes. The points which were clear to all of the laboratory men fifteen years ago are now painfully apparent to those serious psychologists who were for the time dazzled; and the others are hastily preparing alibis and seeking cover. The astonishing doctrine that intelligence tests measure mental capacities directly, once broadcasted from centers of authority, is now heard only in the echoes from the more remote places, and mental testers are beginning to understand that only acquisitions are measured. It is true, some workers are still struggling with the attempt to determine whether intelligence, in general, remains constant

from year to year, using in their endeavors age-scales which have been standardized explicitly to prevent their giving such information. But, on the whole, the workers in this field are slowly being educated to an understanding of their materials and an evaluation of their results. The efforts of the laboratory men in this educational work, and their protests against the highly lucrative exploitation of the last ten years are bearing fruit.

Another great movement was spectacular because it occupied a field in which the laboratory has not been ready to make practical contributions. While it is to the lasting credit of laboratories now that they have been unwilling to rush into the field with nostrums, the field has yawned, and it has been filled —temporarily, but with great financial profits. I am referring to psychoanalysis, of course.

The clean-up job resulting from the psychoanalytic movement falls to the psychologist, whether he wants it or not. I speak not alone of the rehabilitation of patients psychoanalyzed until their funds are exhausted or until their symptoms become too serious to play with. I speak of still graver results of the popularization of psychoanalytic speculation.

Although the progress of psychoanalysis has been marked by the development of glittering hypotheses, stated as facts; the abandonment of these; and the substitution of still others; the course of psychoanalysis can not be treated simply as the zigzag path of an inebriate, marked by a series of empty flasks. The hypotheses, abandoned or not, filter down to popular level, and their origin, frequently unrecognized, produce results which are sometimes appalling.

Although the main psychoanalytic movement has burst, one of its accessory bags is still inflated, and curiously enough has fascinated a few laboratory men who ought to know better. For at least three thousand years there has been the illusion that if we could only *classify* men, great social problems would thereby be solved. And so the sheep and the goats have been classified under various more pedantic headings and subheadings. Now the terms are *introvert* and *extravert,* and we have even laboratory methods being devised to sort out the woolly animals from the hairy ones. A survey of this little inebriation episode in the case of the laboratory is most diverting, but not timely here. This movement is already sharpening its knife for the operation of hari-kari—which unfortunately will be carried out on our doorstep. The great validity of the laboratory method lies in the fact that it not only survives these vicissitudes, but looms up more solid, more necessary, more vital and more worthy of respect than ever. I am no fanatic for pure science. I realize that the ultimate justification of any science is in its application. But I must call upon you to rejoice with me that the absolute necessity of pure science is receiving its recognition even in fields where it has been most sorely beset.

It behooves, then, the laboratory men, having upon their shoulders the responsibility not only for the future of psychology but also for the welfare of society, it behooves them, I say, to consider with due earnestness and solemnity the actual situation in laboratory psychology. I do not for the moment suppose that all psychological problems will be solved in the laboratory; but let us admit that the laboratory is the foundation, and that foundation must be conserved and extended. The total group of laboratory men represent many tendencies and preoccupations. Our differences are interesting, and we love them, but it is imperative that we should at times forget these, and consider our common interests and common duties, which I submit are far greater.

The situation in laboratory psychology is not satisfactory. It is not efficient. It is not safe. It is depressing. In the first place, in response to the feverish demands after the war, we have trained too many psychologists, or partly trained them, and there is an excess of teaching and deficiency of research. I am well aware of the fact that the output of research publication has increased, and that in spite of the establishment of new journals, all avenues of publication are congested and an enlargement of these avenues is needed; but whether the volume of research that will be of importance a few years after it has appeared has greatly increased, is another matter into which I shall not at the moment inquire. At any rate, the energies of some of our best young men are engrossed in teaching, and it is a question whether the teaching is worth while in the case of 70 per cent. of the pupils. Leaving out of account the matter of general psychology, which is given to hordes of uninterested youths, the courses in applied psychology from the text-books, and various other sorts of alleged psychology—the laboratory course should be given consideration. I believe that most of the laboratory work given undergraduates is wasted. It is not intense enough to prepare them for research, and as a cultural effort is a total loss. Most of the students subjected to it come out with a scorn for psychology as a trivial subject; nor do I believe that any laboratory work can be given large classes of students which is not a waste of time or worse. I believe psychology would benefit greatly by drastic reduction of laboratory students; a reduction to those who go deeply enough into it to get something definite, and to numbers small enough to make that possible.

As concerns our graduate students, the problem is again a serious one. It is well known that most of them, after doing research for the Ph.D., die on their jobs. They take teaching positions and do no more research. It has been said in this connection that most of these men are not really research men; that they have done research under artificial conditions, working out ideas supplied by the director, and under the personal stimulation of the director, automatically ceasing operation when the extraneous ideas and stimuli are not supplied. This is true in some cases. In other cases it appears that the situation is different. The graduate student, especially in the last year before the doctorate, is in a simple situation. He can give his time to research without distraction. Then he tackles a teaching job, and must put a large part of his time and energy for a year or two into organizing courses; into organizing too many courses. The stimulus to research is lessened; the situation is a new one. If he has held a post-doctorate fellowship, prolonging the time to the critical change, I believe the situation is still more difficult. By the time the teaching is routinized he has forgotten how to do research under difficulties.

This situation has been recognized, and occasionally attempts have been made to meet it by relieving of teaching a man who has had research ability in the past, in order that he may do research. What is the result? He does nothing. Hence college administrators are hard boiled. No one will be relieved of his teaching load unless he will prove that he will do research while carrying the normal load. I believe this is as it should be. What are we doing to encourage the younger men to initiate research along with their teaching? Nothing, except to talk about it, and to stimulate piffling types of investigation requiring little time, energy and development. Is there anything that could be done effectively? I believe there is. Two things are necessary: (1) to set up a definite facilitation of the utilizing of spare time in research of a major character; (2) to hold out definite types of assistance to those who succeed in starting research under those conditions. How shall this be done? I have a suggestion to make a little later.

I come now to one of the larger evils in laboratory psychology: the general instability and impermanence of departments. In theory, and officially, there are a number of departments of psychology in the United States which have had a considerable period of life and growth. To measure them from the official data of establishment of departments is not quite satisfactory from the point of view of laboratory psychology, since some institutions had no laboratories until some years after the departments were established, and in other cases there were laboratories for psychology before there were psychology departments. If we estimate the ages from the dates of actual laboratory installation, we have a fairly impressive list. Really, however, the ages are merely chronological and the continuities are sadly lacking. The characteristic record is of the building up of the staff, the accumulation of apparatus, the development of the body of problems, and then the wreck of the organization with the commencing of a new department into which the first group of problems scarcely enters, and for which a new accumulation of apparatus and the development of new types of technique are necessary. There is, in fact, hardly a laboratory organization in the United States as much as ten years old.

Now, I am not saying that conditions are better in other sciences. I am merely complaining that the conditions in psychology, which are highly unsatisfactory, have contributed to the production of isolated and fragmentary research, and to the retardation of progress in the solution of fundamental problems. Obviously, the causes of these conditions are largely beyond our control, lying primarily in the short-sighted policies of university administrators in general. The rapid development of laboratories, leading to the frequent shifting of personnel, is partly responsible; but this shifting would not be possible if there were any fundamental stability in the older places. Instead of the developments taking place in the institutions where the best men are, they take place in new institutions in order to attract these men away from their establishments. One of the signs of inefficiency due, I believe, in part, to this instability and lack of progressive utilization of resources, is in the fact that the destruction of files of our journals would not be a great loss. The loss would be principally in some of the newer developments of psychology, such as animal behavior. Certainly an impartial censoring of what is contained in the volumes would be an unmixed blessing.

How this instability may be reduced is a perplexing question. The establishment of institutes affiliated with universities, but under relative autonomous control, may help, and is an experiment worth while, but it introduces new and serious evils, and I do not believe it will be a panacea. In the meantime, it is most expedient to consider whether there is a possibility of increasing the stability of our laboratory work as a whole in spite of the instability of departments. For it is increasingly evident that there are fundamental problems of pure science which must be solved for the benefit of other problems which are

rooted in them, and that these problems can not be solved in a short time, or by the solitary efforts of one or two men. Furthermore, some of these problems require expensive installations of a magnitude which make the provision wasteful and vicious unless their use can be extended over a long period of time in the hands of a series of competent persons interested in these problems.

Let me return now to one of my main points of dissatisfaction with laboratory work and suggest a remedy. The salvaging of researchers and the stimulation of better research may, I think, be forwarded very much if two provisions can be made: (1) for summer research fellowships, and (2) for the aid of going research at critical points. Let me elaborate the first suggestion somewhat.

The young instructor, struggling to organize his teaching work, theoretically has his summer free. Practically, he is forced by financial considerations to seek a summer-school job, or else some non-academic type of employment, in order to support his family. If he teaches in a summer school, he labors with a group of tired-out schoolma'ams, case-hardened superintendents, college students of inferior grade who have flunked their courses in regular session, and an assortment of high-school sheiks and flappers. He accomplishes nothing of importance, and he comes back to his fall work fatigued and unprepared for it. Yet he has few alternatives. It is all right to say that university salaries should be more adequate, so that instructors would be able to spend their summers more profitably; but university salaries are not adequate for younger men, and they will not be. Moreover, I am not certain that more adequate salaries would turn the trick. The incentive to a better scale of living or to a trip to Europe would be too great. Now, I am not belittling either of these goods. But sacrifice must be made, and it is usually necessary to establish conditions such that the proper sacrifice will be chosen.

If a young instructor were offered the alternative of a summer-school job at $600 or a research fellowship at $450, he would choose the latter, or else show that he had not the stuff in him. In the summer, if not distracted, he could start research and thus get back into the harness. During his academic year, even, he can start research if he can see the possibility of effectuating it in the summer. The plan is well worth trying, and does not require any vast amount of funds. The objection will be raised that there are few places where summer research can be carried on. Few institutions of high rank maintain a summer quarter, and from the institutions which maintain the flapper summer schools, the good men on the staff flee during the summer as from the pestilence. A psychological Woods Hole is not possible. Psychology is not that kind of a subject. Moreover, although Woods Hole and La Jolla and Friday Harbor are doing for the young biologists something of that which I am proposing to do for psychologists by summer research fellowships, I am not completely convinced that it is the most satisfactory solution of the problem even for the biologists, and I suspect that if the scheme I propose were established for psychology, not only the physical sciences but even biology would follow in our wake.

Let us grant that few places at present are prepared to take summer researchers. This is true, and there would be few fellowships at first. But the stimulus thus furnished to other institutions would be great, and I believe that there would be an effort on the part of the universities to offer attractions to summer researchers. There might even be a reduction in the summer schools of the present type, and this would be a great blessing in itself.

In this connection, I should like to point out that the national laboratory I have proposed would be of material advantage. A certain number of summer workers could be provided for on certain topics. This would, however, not fill the whole bill, or even a large part of it.

I may point out, finally, that this summer research fellowship plan is not a mere extension of the present pre-doctoral and post-doctoral fellowship experiment, but is rather something designed to offset the evil effects of these.

Now as for funds for the aiding of research. This is a ticklish subject. In the first place, the administration of such funds offers a serious problem. The selection of really worthy research and the aiding of it in a way which shall be effective, and which will not relieve universities of their responsibilities, is apparently too difficult a job. Two years ago I should have said that there is no agency competent to do this. The National Research Council seems at first incompetent because of the constant flux of the divisions, but it is now impressed upon me that the capacities of the council have not been fully utilized. A changing division may have permanent committees, and a permanent committee can do this very thing, inefficiently at first, but with a steadily increasing efficiency. Certain of our committees have in the last few years demonstrated the fact that they can fulfil functions of this type.

Certain principles of award are capable of being outlined. No grant must be made except to "going" research. Projects are a snare and a delusion. The man whose research is to be aided must have shown that he has a vital problem, that the problem is capable of solution in his hands and that he has the guts

to see it through. Again, it is not a question of financing men. Financing men sounds well but doesn't get results. Research, not projects; work, not men—these are worth assisting. This plan would establish a situation quite different from the present one, under which it is easier for the man to get funds for something he proposes to do than for something he is doing. In fact, that he has research actually going now counts against him. Hence, instead of furnishing a stimulus to the best men to get research started on their own, discouragement is produced. Men say: "I would like to do research if I could find the time, or if I could get assistance." What we must show them is that the man who can get research started with no time and no assistance will be helped to finish it. The importance of this stimulation I believe to be very great.

As regards the effects on university budgets, I believe the plan can be so handled as to stimulate the universities to more generous provisions, instead of weakening their efforts. Aside from the fact that university cooperation would be demanded in awards, there is the fact that the difficulty in university provision is in regard to elasticity. No department needs the funds for vital research in one year that it needs in another. Yet, departmental budgets can not be very elastic, and the natural tendency is toward the minimal, not the maximal provision. More generous provision in many cases means actual waste and the production of rotten research. Universities themselves recognize this, and attempts are made to provide elasticity through the setting up of general university research funds to be applied as needed each year. This scheme is useful but has painful drawbacks. Politics is by no means absent from universities, and departments can often enforce claims to consideration against other departments. No one in a university except the psychologist knows whether a certain psychological project for research is valuable or not. Hence, the tendency to dole out in accordance with the importunities, and to consider whether the department has been getting its share of the booty or not. For its effect on the administration of university funds an expert national committee is much needed; but such a committee would have no standing with institutions unless it, itself, had some funds to administer.

Let me give an example. The situation of the psychogalvanic experiments, so-called, is a scandal, and a stench to the nostrils. In a number of universities the galvanometric method is being used, but is apparently getting nowhere. Individual experimenters publish results which can not be duplicated by other experimenters. Yet a number of our most able young men are working in this field. The trouble is not in lack of physical technique, for these same experimenters have shown their ability to take galvanometers which other departments were unable to operate, set them up, and get them going in excellent style. I think we may readily discern that the trouble lies in the complicated psychological conditions, and in the lack of development of psychological technique applicable to these conditions, and which can be made a matter of accurate record. At present it is impossible to ascertain what any of the experimenters really did obtain. Their results, no doubt, are valid for the conditions of their work, but what were the conditions? Nobody knows. I am speaking here of the careful workers, oriented in the history of the method and in the techniques of preceding investigators, in so far as it is possible to know what these preceding workers did. I am not speaking of the hasty and ignorant persons who have discovered wonderful things by appalling technique.

The committee on research on emotions in the Division of Anthropology and Psychology of the National Research Council thought it possible to forward research in this vexed field by providing for a three-year research in the hands of one man. No funds were obtained for the research. Although I was a member of the committee, I am now convinced that this abortion was not unfortunate. If the research had been financed, the results would have been merely one more set of data which no one else would have accepted.

We have among us six or eight young men who are as well versed in the galvanometric technique as is possible under present circumstances. What are the possibilities of pooling their abilities and training? Not any, under present conditions. What university could undertake the work of cooperation and correlation? None, so far as I know. What would be the cost of an installation competent to provide for the various aspects of the problems which, it is plain, must be taken care of during a period of twenty years? My present estimate is $100,000, assuming that a suitable site and building were available. There is, however, no such edifice at present. Spending such sums of money under present conditions would be mere folly. Suppose you were to put the installation in Siwash University, which may at the present time have a laboratory director interested and competent in problems of this type. In five years' time, the department at Siwash might be remodeled, the director dead or gone to some better place, and the new director trained in and interested only in the study of the putative ancestry of emotionally unstable children.

The installation necessary for the psychogalvanic work is also useful for other lines of work, after the

psychogalvanic cycle shall be completed, and to a certain extent while it is being carried on. The galvanometers will be required for fundamental attacks on problems such as those of speech and thinking; and various accessory apparatus will also be essential or accessory for certain other problems of a lengthy and fundamental nature. What I really am proposing, therefore, is a national psychological laboratory, similar in some of its functions to the Bureau of Standards, but not under federal control. Such a laboratory can be under relatively permanent direction and can undertake programs of research too lengthy, too expensive and too complicated for other institutions. In such a laboratory truly cooperative results of the highest value can be obtained. Men working in other laboratories on details of the problems undertaken in the national laboratory could make arrangements to transfer their work there during a year's leave of absence, or in summers. Work done here can be subjected to criticism while in progress, instead of afterwards, and the cooperative method can insure greater certainty as to conditions. Such a national laboratory, I believe, could be of really inestimable advantage to psychology, not only because of its availability for the solution of problems unwieldy elsewhere, but because through it standards of research may be elevated.

The day of the isolated experimenter and of fragmentary problems is passing. Unless we find means of shaping our problems into coherent plans of larger unity; unless we find means of carrying out vital research and postponing the merely interesting; unless we can pool our constructive and critical abilities, we shall be out of step with the advance of scientific method.

Another of the troubles of laboratory psychology, not its last, but the last I will mention, concerns publication of research results. Publication at present is in an unsatisfactory position. Costs to authors for monographs are too high. Subscription prices to journals are too high. There are not enough journals, yet there are more than we can support. Under such conditions the tendency is to suppress data which might be valuable, and to publish conclusions which are usually worthless. Stuff is published which ought to be burned, and research which ought to be published sleeps in pigeonholes. Publication is on an unsatisfactory basis both commercially and scientifically. Publications of a sound type should be more generally supported by the company of psychologists individually. They can not be so supported until prices are reduced, and prices can not be reduced until wider support is given. Here we have the old problem of the irresistible force and the immovable body.

The problem of abstracting is closely allied with that of original publication. It is openly admitted that the present abstracting experiments in psychology and biology may not give the results which were hoped. Nor can we reasonably expect satisfactory results on the basis of amateur work. Unless we have a group of well-paid abstractors, with at least doctoral training in the fields they cover and giving their whole time to the work, the abstracting business will be a disappointment.

Now I am fully aware that my proposals are open to many objections. In the first place, they are dangerous. They seem to tend toward centralization of power which can dictate to institutions and individuals. Centralization, maybe; but dictation—fiddlesticks! On the contrary, the committees which should administer such trusts would be extremely unpopular, and would do as much good by stirring up attempts to do without their aid as they would through their aid. The administration would be incompetent, certainly; but it would not dare be as incompetent as our departments are. The greatest objection of all is this—the schemes are financially Utopian. There isn't that much money. Well, money is being spent at present on far less definite and less vital projects. We may not be able to get money, but if we see clearly the needs, and the benefits that would accrue from these plans, or from still better plans which you may suggest, then we are slackers in our duties if we do not present our case. That is all that I am interested in at the present moment.

KNIGHT DUNLAP
THE JOHNS HOPKINS UNIVERSITY

FREDERICK CHEEVER SHATTUCK[1]

A SHREWD and kindly judge of human nature, a whimsically humorous commentator on men and affairs, a wise physician rich in the learning derived from large experience, an impressive and stimulating clinical teacher, a far-sighted, enterprising and generous supporter of important new developments in medicine—these are some of the lasting impressions of Dr. Shattuck which lie deep in the memory of his colleagues in the Harvard Medical School.

His association with the Medical School was long and distinguished. Six years after his graduation in 1873, he was appointed clinical instructor in auscultation and percussion, and as instructor in those subjects and later in theory and practice he continued for nine years, until 1888. He then became Jackson professor of clinical medicine, a title which he honored

[1] Minute placed on the records of the Faculty of Medicine, Harvard University, at the meeting of February 1, 1929.