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# New Opportunities and New Responsibilities for the Psychologist[1]

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WE ARE NOW ENTERING the after-dinner phase of psychology in World War II. The hunger-pangs that disturbed us a short time back are now quieted to the point of satiation. The cortical cells are experiencing that pleasantly-toned anoxemia that takes place as digestion exerts its priority over cerebration in the employment of vital bodily fluids. It is time to push back the chairs, to light up cigars, and to reflect on how well we have done by our country, by our profession, and by ourselves in the war years just past.

The reflection becomes all the more pleasing as we are made aware of the contrast between 1941–1942 and 1945. It may not be too much to say that most of us here at this meeting came into the military services through the servants' entrance. We were brought in, in an era of gloom and defeat, under the conviction that things were so bad that any available magic should be tried, even psychology. We have worked four years, more or less. Now we are going out the big front door, labeled as military specialists, while the band plays "Hail to the Psyche." Victory has replaced defeat; concrete realization of what psychologists can do has replaced a vague hope that they might possibly do something; and a warm and cordial acceptance has replaced a suspicious and grudging admission to the military work-place.

It is indeed a time to pass the brandy—at least figuratively. It is a time to make preparation for the moment when we shall stand in front of the assembled regiment or ship's company and hear the general or the admiral recite aloud our virtues. It is a time to lean back in our seats and harmonize on a few stirring verses of "The Cortex, the Cortex, Forever." And, above all, it is a time to look forward to returning to our several campuses as recognized experts.

Yet even as we relish the finer moments of the glowing after-dinner mood, we are uneasily aware that we shall never return to the campuses we left, at least as we knew them. The good, well-rounded world of 1928 and of 1938 is gone forever. It would ultimately have been destroyed, in any event, by the social currents of which the war was only an epiphenomenal symptom. It was already in its last days when we marched off in self-conscious awareness of our new uniforms, four years ago. But it was not allowed to die a natural death. Its final destruction came about through an explosion. An American airplane dropped a bomb no heavier than a week-end suitcase. A small city was destroyed; and with it was destroyed much of the basic framework of the social world we inhabited before the war.

So we sit here uneasily, you and I, pondering the fact that the whole parameter of personal success bulks very small indeed in a world which is earnestly trying to find out whether the human race must necessarily destroy itself. Official kudos, medals, the offer of a better job—all these things seem curiously unimportant as we try to revise our scheme of things to fit a world in which a city may vanish in the flash of an eye and a whole nation may perish between sunrise and sunset.

Instead of the long-anticipated era of congratulation and self-congratulation, then, we find ourselves in a period of searching self-examination. Instead of counting our medals, we are engaged in taking a stock of the primitive tools with which we must hope to master an unending task of incredible difficulty in the years ahead. I should be running vainly counter to the most significant intellectual trend of my professional lifetime, then, if I were to devote this hour to the more or less conventional and expected review of the fine things we have done. Instead, the occasion

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requires that I shall spend the time trying to assay what we have and what we may hope to do with it. The humility that anyone must necessarily feel when confronted with such a task is not decreased by the realization that editorial writers are now inviting the social scientist to enter a game of chance in which the stakes are the survival of the human race.

What I have to say to you about the new opportunities and new responsibilities of the psychologist will be understandable and acceptable in proportion as you are willing to agree to my thesis that psychology is now entering a third phase of its development. The first phase, extending roughly from its founding down to about the 1920's, was the phase of *local* loyalty. The second phase, from the 1920's to the 1940's, has been the phase of broadened *professional* loyalty. The third phase which we are now entering must, of necessity, be the stage of *social* loyalty and *social* responsibility.

Please do not bother to object that phases in the genesis of any science are not as clear cut as that. I know that as well as you do. It is quite true that there are many individuals who fall outside the pattern of these phases. You may be sure, then, that I am not trying to describe any series of universal and all-inclusive temporal groupings. I am trying only to suggest a modal pattern, attempting to locate the movement of the majority of the profession, and content to allow the exceptions to fall where they may.

In that light, let us return to the thesis of the stages. As psychology developed through its first half-century of existence, local loyalties tended to run high. One belonged to a department which possessed the Only True Path; the rest of the field was tenanted by infidels who followed false gods. Wundt, when asked about Stumpf, whose laboratory was only a few miles away, was merely reflecting the spirit of the times when he said that he had never heard of Stumpf. Titchener was wont to dismiss the efforts of some of his extramural colleagues with the statement: "That may be all right, but it isn't psychology." There were not only schools of psychology but local brands of schools. Facts were few; logical constructs were numerous. The whole setting lent itself nicely to fine differences between *mine* and *thine*. The speaker, coming into psychology in the early 1920's, found himself in the midst of this. It was a good lusty era in which you joined up with a psychological team, ordinarily on the basis of a geographical accident, and thenceforth fought lustily to show that your team was right.

Phase Two was a natural evolution from Phase One, as facts accumulated and methods became less particularized. Just about the time that someone thought of getting out a volume on the 'schools of psychology' the schools themselves began to lose their identity and their sharp competitiveness. As early as 1926, one outstanding behaviorist confessed to the writer that a seminar which he had convened to justify behaviorism had convinced him that he was himself not a behaviorist.

Set the dates where you will, the trend remains unmistakable. There is no better proof than to ask you here to look around and determine what 'school' your immediate neighbors belong to. The chances are excellent that you will fail completely at this task. It may not be too much to say that each 'school' has had certain protests to make. It has made these protests, which were then absorbed in the main body of a research psychology, after which the main body moved along. There are few of us today who cannot acknowledge personal debts to Structuralism, Functionalism, Behaviorism, *Gestalttheorie,* and the teachings of the psychoanalysts. Beyond these broad divisions, we have also been stimulated in considerable amount, and with considerable profit, by contact with topology, with operationism, and with emergent evolution. We have gained from our contacts with such varied approaches as nondirective interviewing, projective testing, factor analysis, and the studies of expressive movement—to name only a few influences. But increasingly our identification has been with research psychology as a whole, rather than with any special movement or any particular set of techniques.

World War II has afforded the best demonstration that Phase Two has been a reality. The man from Nebraska has worked alongside the man from Palo Alto without the need of an interpreter. The young officer who had his statistics under Thurstone has found much common meeting ground with the lad who had studied under Kelly. There has been much stimulation in this business of working together and precious little intramural strife. Today, as never before, research psychology is a discipline which is much bigger than local loyalties and much wider-reaching than the confines of any school. And, unless you yearn for the complacent security of the Old School Tie, you will say that this is good.

It is good, I agree; but it is not enough. We must now be ready to enter a third natural phase of development. In Phase One, we demanded that a man belong to the Right School before we would break bread with him. In Phase Two, we ceased to ask him what school he had attended; we asked chiefly that his work should be sound, that he should check his critical ratio, and that his conclusion should not outrun his data. In Phase Three, the satisfaction of the one-per cent level of statistical confidence will not in itself be enough; we shall now have to ask not merely whether a result has *statistical* significance, but also whether it has *social* significance. When that becomes commonplace, we shall have entered Phase Three.

You will readily understand, I am sure, that this is not a matter of choice. If we do not ourselves freely adopt the idea of checking the social significance of our findings, it will be thrust upon us from outside the profession.

Well, you may say, we do all that already. We psychologists have our Society for the Psychological Study of Social Issues. We teach courses in social psychology. In our local communities we have worked with social problems and gone afield to meet social issues. So we are already in Phase Three.

If you can make such an answer, you show that you have failed completely to grasp how broad are the implications of membership in Phase Three. Social responsibility does, of course, include a willingness to meet and deal with social issues. But it goes far beyond that. Social responsibility for the research psychologist touches his professional life at every point. It determines what problems he shall select for his attack. It determines, to a considerable extent, what shall be accepted as methodologically respectable methods of attack upon these problems. It also determines, to some extent, how he shall interpret his findings and how and where he shall publish his interpretation.

It will be quite apparent to you that no one paper can hope to explore the varied implications of social responsibility for the social science investigator. I therefore propose to limit my discussion to what may be, for some of you, the least obvious aspect of the implications. That will mean that I shall attempt to indicate how—as I see it—a continuing sense of social responsibility affects and influences the very problems upon which he chooses to conduct his researches.

Let me begin by stating a postulate which attempts to describe the motivation of the choice of problems for research. Stated in its baldest terms, this runs about as follows:

As long as choice of problems is primarily determined by a feeling of *professional* responsibility, the investigator is most likely to select problems which hold promise of early returns, obtained by conventional methods. In other words, professional responsibility seeks out problems in terms of their promise of methodological neatness.

A sense of lasting *social* responsibility, on the other hand, demands that trained investigators turn their attention to the most pressing social problems. Neatness of result—and its accompanying professional acclaim—must often be sacrificed in order to attack problems which, although not promising early or definitive returns, are of immediate importance to social stability.

Some of you may say that that is an indictment. It implies that applied psychology has dodged some of its social responsibilities. But let us review some of the evidence.

### Psychological Tests

Suppose we begin by looking first at what is perhaps the psychologists' best-known working tool—the psychological test. Psychological testing saw its birth during World War I and reached its greatest peak of usefulness, up to this time, in World War II. Papers presented during this Conference have shown what widespread use was made of psychological tests during the period of the war. Tests were used to select fighting men, to classify them for their best employment, and to determine their best possible usefulness under fire. Certainly not fewer than ten million men in this country alone submitted to some sort of screening by psychological tests; and certainly the tests themselves operated overall at a level of predictive efficiency not hitherto achieved.

This is an accomplishment to which we psychologists may point with great pride. It is possibly the most important single contribution that applied psychology made to the very important and practical business of winning the war. Yet, while it is good, it is also revealing.

Consider the fact that our best-developed tests are probably tests of intelligence. We have made great advances, as a profession, over the intelligence tests used in World War I. A wealth of serious debate during the 1920's set the stage for the factor analysis surveys of the 1930's. We have as a result been able to learn much about the composition of intelligence. The typical intelligence test of the 1940's, partly as a result of such surveys, is a rather well-advanced tool. It contains a large percentage of discriminant items and affords a usefully broad range of scores, and it is characteristically high in reliability. As a result of correlational studies, we know a good deal about the relationship between intelligence test scores and progress in education, in industry, and in the military hierarchy.

Now all of this is good; but it is also significant for my thesis, for intelligence is perhaps the best-behaved, the least troublesome, and possibly the least modifiable of all our behavioral characteristics. It does not represent much of a social problem. The man in the street, without help from the psychologist, has pretty well figured out what to do about differing levels of intelligence. In the main the work of the psychologist has served only to confirm and justify the rule-of-thumb procedures which have been handed down as a part of our common-sense social inheritance. The work on intelligence testing has been well worth doing; but it has been accomplished while other and much more pressing social problems were thrust aside.

### Personality Tests

Let us consider some of the less well-behaved aspects of human behavior. Take that vague and undefined term 'personality.' In our everyday life it bulks as considerably more of a problem and as considerably more subject to modification and change than intelligence. Our ready-made social fabric is much less certain in telling us which individuals are wide of the average in their emotional and social adjustments. Indeed, a recently published best seller suggests that hundreds of thousands of people a year are taking their suspected personality troubles to countless varieties of quacks and charlatans. Clearly the situation demands that the psychologist, as rapidly as possible, shall develop tests which will accurately reflect how any given individual deviates from the social or emotional norm; and it demands that the psychologist shall indicate ways of reducing undesirably large deviations, if, indeed, modification is possible.

The problem has great social significance. What does organized psychology have to offer, in 1945, toward its solution? The plain answer is, very little indeed. Where the intelligence test of 1945 is a recent product, based on hundreds of careful researches, I am informed that the most widely used "personality" test at this time is one published in the early 1930's and based on work completed in the 1920's. Researches on this test have been few; and their outcome has been almost entirely negative. Attempts to validate it against available criteria have, so far as I am able to learn, yielded nothing to encourage us to believe that it gets at clinically identifiable aspects of behavior. Furthermore, such studies as have been made of the traits it purports to measure suggest that these alleged traits are in themselves so unstable as to limit the test to a uselessly low level of reliability.

One may well ask about some of the other attempts to get at personality, such as Murray's work, Shipley's Inventory, the Minnesota Multiphasic, and, above all, the work in projective tests. The answer is simply this: Granted that certain men are exploring promising angles, the vast bulk of American psychology has not advanced in its thinking about personality tests since the 1920's. If we are concerned with large-scale movements in the field, we can feel no elation over the explorations of the few. We shall not have met the social challenge of the need for personality-measuring instruments until psychologists in considerable numbers have sacrificed the professional gains of working neatly on further definitions of intelligence to work upon the unsolved, and possibly unsolvable, problems of defining the place of a given individual in the area of social and emotional adjustment.

### Public Opinion Polls

The example just cited is by no means an isolated one. As you leaf through any year's issue of the *Psychological Abstracts,* you will be struck by the inevitability with which our profession in the main has chosen the neat, rather than the socially significant, in finding problems worthy of its researches. We have, for example, devoted much time and thought to improving the predictive efficiency of our public opinion polls; yet we have pretty consistently dodged the issue of trying to determine what effect the polls themselves may have upon the voters. Alarmed politicians may insist that the polls, by inducing a desire to climb on the band wagon, have seriously altered the political picture. In the main, our researchers have preferred to wave this charge off and to work on the improvement of their polling methods. And, as you review the situation, you may again be struck by the fact that the improvement of predictive efficiency is a neat, quantitatively expressible problem, while the measurement of band-wagon effects is unorthodox, complicated, and difficult of quantitative expression.

### Education

In our public schools, you will find many parallel illustrations. Researches of the past fifty years have brought out a wealth of information as to how to improve teaching. We can now, with some assurance, tell the teacher how to get better attention, how to assure lasting memory, how to motivate his students to work, and, in general, how to induce them to perform well on scientifically improved final examinations. These are all real gains; but they dodge the basic issue of what changes, if any, are produced in our students by their education. We cannot say, with even the slightest degree of scientific assurance, what any given course, or any given curriculum, contributes to the making of a wise and stable citizen. We are totally unable to say which courses make critically important contributions and which make negligible, or even negative, contributions. In other words, we who set high standards of validity for our predictive tests are totally without information as to the validity of any of the elements which make up our modern curricula.

### Alcohol

The exhibit can be expanded endlessly, but here I shall include only a final example. We may begin by pointing out that we know a good deal about the primary effects of alcohol upon human behavior. This is scientifically interesting, and it is useful. Yet the social problem here is pretty well solved by our folkways. Society does not admit the judge to the bench or the teacher to the rostrum while in a primary state of intoxication. It penalizes heavily the drunken driver and the drunken aircraft pilot. In general, society at large recognizes that a man in a primary state of intoxication is incapable of wise and well-coordinated behavior. Our researches, it appears, do little more than confirm and quantify what the man in the street has known since the days of the Old Testament.

Turn from this to the state that *follows* intoxication and you have a different story. Society appears to be puzzled about what to do with the man who has a hangover. He is admitted, possibly with an admiring leer, to the bench or the schoolroom. He sits at the council table in the industrial establishment. He is permitted to drive his car or fly his plane. Yet through all this runs a thread of social uneasiness. We learn that certain commercial airlines take steps to prevent drinking within twenty-four hours of a scheduled flight. We hear of business executives who defer all decisions when they are admittedly recovering from overindulgence. The attitude of society, in other words, appears to be equivocal; it looks to science and technology to supply valid estimates of how, and how badly, a man's resources are reduced by the presence of a hangover.

Here is a socially significant problem, amusing though it may seem at first sight. How much have the research disciplines to say about it? Although there are literally thousands of articles reporting investigations into the primary effects of alcohol, there were, the last time I checked, exactly *three* research articles on the nature of the hangover. The fact that none of these three met the ordinary standards of sound research is irrelevant. The basic fact is that, on the equivocal problem of the hangover, research is silent; it has nothing to offer society. A major reason, I believe, is simply this: The study of the primary effects of drinking is neat; it promises early returns, based upon adequate use of controls, and leading to nicely quantitative results. The hangover is a messy topic. Controls are uncertain, and the outcome is likely to be something less than clear cut. Social scientists have turned, almost without exception, to the study of intoxication itself and have shied away from the socially important, but methodologically unpromising, study of the aftereffects.

If you doubt the fundamental thesis as given here, you may turn to any of the annual indices and make your own tabulation. It can scarcely fail to convince you that the social significance of a problem has been a determinant only for the few; the many have persistently devoted their research efforts to the methodologically neat, with a bland indifference as to whether the outcome met any social need or not.

You may be ready to object that, by raising this issue, I am striking at the very basis of sound research. You may object that the job of the investigator is to investigate and that, in science, no problem is any more important than any other. You may urge that investigating the effects of a one-degree rise of temperature on the maze-solving habits of the tapeworm is, by definition, just as important as the study of why men fight. If you do, it will be a familiar argument, for the speaker was raised on the concept of a science which mercilessly and objectively sought out its facts and let the chips—and the human race—fall wherever they might. He was raised on the stereotype of a scientist who had no social responsibility and who was motivated solely and entirely by the desire to discover the generalizations under which future occurrences might be predicted.

It is a picture not without some considerable appeal. It may have been quite adequate to the world of yesterday; but it has now lost its adequacy. That is not to say that the social scientist should not work upon problems which have no immediate and obvious application. He should and he will. Indeed, it is important that our research people should be left relatively free to work on such problems as their own consciences may direct, for only thus can science and technology progress.

### Applied *vs.* Pure Research

At the same time, let us free our thinking from one error. There has long been in existence a vague belief that something called 'pure science' developed basic methods which it then handed over to 'applied science' for use in practical contexts. I am told that this is at best a partial truth in other sciences. I know that it is rarely true in psychology. A careful review of the last twenty years in applied psychology will show that many, if not most, of the methodological advances in applied psychology were made by men working directly in some practical context. This being the case, researchers can afford to devote much of their time to working on practical problems without any fear that the development of new and basic methodology will thereby cease.

It is encouraging to note that psychology at large has already had four valuable years of practice in working at problems of social significance. It is encouraging to note that this work has been sound enough to evoke the support of hard-headed military realists. One aspect of this situation is worthy of particular note. It is worthy of particular attention that men who were brought in to work upon very specific and highly localized technical problems found themselves increasingly assigned to tasks of greater complexity and greater significance. Men originally assigned to the improvement of selective tests gradually earned the right to work with whole systems of classification for combat assignments. In many cases work with relatively simple criteria based on outcome of training led over into the much more complicated, and more significant, evaluation of actual performance under fire. Psychologists originally assigned to improve the mechanics of day-to-day examinations finally were given a free hand in revising whole programs of training and, indeed, of redefining the whole purpose of the training program. As the detailed history of this war is published, the thoughtful reader will see that here, as never before, psychologists have progressed from an original attack on the neat and simple to a later and more prolonged concern with the militarily important and methodologically complex. It is the speaker's conviction that this will ultimately be regarded by our profession as the most significant development of the war for the future of psychology.

It is of the greatest importance that the lessons of the war shall not be lost. It is of the greatest importance that attitudes and techniques which proved to have real practical significance in attacking the primarily destructive social problems of the war shall be carried over for attacks on the primarily constructive social problems of peacetime. Yet this will be accomplished only if psychologists in general recognize how they have achieved their present military status and if they recognize why they must transfer their efforts and their zeal to the attack upon common social problems.

To this end, careful note must be made of one significant motivating influence of the wartime period. In the main, psychologists were not permitted to remain in their laboratories, or to work upon military problems in the comfortable isolation of their own campuses. Characteristically, they were transported bodily to the military establishment and compelled to live in day-to-day contact with military folk and military problems. As they sweated out tours of duty, they began to work upon certain problems—in a very large number of cases—simply because the problems forced themselves on their attention, day after day. Their problems, if you please, arose from the persistent demands of the environment rather than from the pressure of some systematic conviction or professional nicety. You will readily understand that the voice of the military environment became audible because the trained investigators were there in the military environment itself. If they had remained in their laboratories and in their studies, the voice of the military environment would have been, at best, muffled and not improbably distorted beyond recognition.

You have heard my thesis. I advance it with the greatest humility, not as a revelation of some novel truth but as an effort to formulate what everyone here must surely realize. You may well ask why I bother to state the thesis at such length, if everyone recognizes its existence. It is advanced as the formulation of one hypothesis which may serve to stimulate some proportion of this group to think beyond the hypothesis itself toward the solution of a basic problem. If such thinking should serve to negate the hypothesis I have advanced, well and good. The fate of the hypothesis is of infinitely less importance than that clear thinking should be done by those who are charged with socially significant researches. The war has given the profession of psychology its greatest forward impetus toward the achievement of a place of importance at those council tables where the future of mankind may well be decided. It is by the thinking of such folk as you who are assembled here that the effectiveness of these later councils will be determined. May you think well!

## *Scanning Science—*

At the August meeting of the German Society of Anthropology, at Cassel, the opening address was by Dr. Waldeyer, of Berlin, on "the somatic differences of the two sexes." Its aim was particularly to bring out the contrasts between woman and man, with the purpose of applying the results to the education and "sphere" of woman. He argued that since a wide collation of measurements and statistics proves that she has a smaller brain, has less physical strength, preserves more traits of infancy and childhood in adult life, and has practically in all times and places held a position inferior to the man, that in our schemes of social improvement these undeniable facts should be respected. The efforts of social democrats and society leaders to establish entire equality between the two sexes and to throw open to woman all the avenues of activity enjoyed by man, he intimates, are mistaken, and will prove failures; and quotes with approval the opinion of Bartels, who maintains that the education, physical and mental, of woman, however high it may be, should be always aimed to fit her for the duties of the family circle only. This conclusion will not be in the least acceptable to the "advanced" women of the day, nor to those sociologists who see in woman's present condition, not the model of the future, but a survival from a barbaric past.

—3 *January 1896*