KNOWLEDGE

Last and least of these four qualifications comes knowledge. But it is last only because of the high valuation of the other three, not because it is unimportant. The obtaining of new knowledge is the chief aim of research, and the accumulated knowledge of the past is its working capital. Moreover, the pursuit of knowledge furnishes the training ground on which the qualities of character and attitude are forged.

Science faculties to-day are giving a great deal of thought to the question: "What kind of knowledge is required for industrial research?" I shall attempt to answer this question.

From the standpoint of the laboratory with a long-range research program, the answer is: First and most important, fundamentals—as broad a range as possible. The reason for this is that the research man must be prepared to tackle more than one kind of problem. Research horizons expand, giving birth to new projects and making those of yesterday obsolete. In addition to this broad training, a man must master some one subject and become an expert in it, as part of his training, but *it matters little what that subject is*. For example, men trained in physical chemistry have made excellent physicists, and have become expert in such fields as high frequency electronics within a few months. Similarly, specialization in nuclear physics is a satisfactory training for industrial physics, provided it is combined with broad fundamental training.

For short-range research projects, the requirements are different. Here a man is hired to solve a particular problem or work on a particular type of development, with less emphasis on long-range usefulness. The requirements in this case are *training and expert knowledge in this particular field.* Broad fundamental training still is desired, however. The difference, therefore, is essentially only in the subject chosen for specialization; in the case of preparation for short-range research, the subject should be one for which there is a current demand.

SUMMARY

To summarize, I have tried to suggest that knowledge, valuable as it is, is not considered the most important qualification for industrial research. Character, aptitude and attitudes are more important. Should they not be rated so in the college educational program? Aptitude, though it can not be trained, can and should be screened by the college more than is done to-day, to avoid the lifelong disappointments of misplacement. Character and attitudes actually are molded by college influences, for better or worse, and therefore are products of college life and are the responsibilities of the college, just as much as scholarship. The fact that the molding influence is the student body, more than the faculty, makes the problem different from that of scholarship, and a different method must be found for dealing with it. Could a method be found if sought with sufficient effort? To-day we give it low priority, devoting only a small fraction of college effort to it. Why?

A STIMULUS-EXPECTANCY NEED-CATHEXIS PSYCHOLOGY[1]

By Professor EDWARD CHACE TOLMAN
UNIVERSITY OF CALIFORNIA, BERKELEY

AT the time I sent in the peculiar title of this paper obviously I was suffering from mental obfuscation. However, in the intervening four or five months my mind has had time, I hope, to clear. In fact, I now seem actually to have discovered what the paper is to be about and to have invented a quite normal title for it, to wit: "The Contribution of Rats to Human Psychology."

In other words, what I really want to talk about is the simple, though somewhat hackneyed, subject of the contribution of rat experiments to the understanding of human behavior. It would seem that the ultimate goal of all psychologists (even of rat psychologists) is the explanation of the behavior of human beings. It appears further, however, that those of you, among us, who have concentrated primarily on human beings have become increasingly aware (as the psychologists of thirty or even twenty years ago were not) that human behavior takes place only in social contexts. You human-oriented psychologists have begun reading (and perhaps even *inventing*) bits of anthropology and sociology. As a result, you have finally become convinced that men are not born, like Athena, full-grown and all armored but, rather, as naked babes who begin acquiring their armor at their mothers' breasts, in the alarms and excursions of toilet training and in the give and take of sibling rivalries.

Now this has produced a real revolution in all our thinking—even in that of us rat psychologists. But, unfortunately, it seems also to have led to some tendency (or perhaps I am merely over-sensitive) on the part of you human psychologists now to look

[1] Address of the vice-president of Section I, American Association for the Advancement of Science, Cleveland, Ohio, September 12, 1944.

down, or rather up, your noses at us poor animal psychologists whom you seem to descry as still left hanging uncomfortably out on the limb—that is, on the good old philogenetic limb. And we animal psychologists (being very suggestible) actually begin to feel quite hang-dog about it all ourselves and we too begin trying to look snootily down (or up) our own noses at our own selves hanging by our own tails out on that same good old limb. But this I, for one, am finding a very uncomfortable position. Hence, the main purpose of this paper is to try to justify that limb to myself and to try to get myself right side up on it as rapidly, and with as much circumspection, as possible.

To return, now, to the new insight that human behavior is always behavior in a social, or culture, context, what have we animal psychologists left to do? The primates (that is, the chimpanzees and monkeys among us) have not been too hard hit. They have rapidly shifted their attention from problems of learning and insight to such problems as social dominance and submission; cooperation in work-activities; a pair of chimpanzees mutually picking off one another's fleas; the sexual cycle of the female ape and how the different stages of her genital swelling affect the food-generosity or the food-stinginess of her accompanying male; chimpanzees raised by human parents, and human babies raised by chimpanzee parents. Oh, no, I forgot. This last has not yet been tried. But it undoubtedly will be (although the folklore seems to prefer the she-wolf as the mother-image) by intention, or by default, if the world continues killing off human mothers at its present rate.

So much, then, for the primate psychologists; but what are we poor rodents to do? What can we contribute to the problems of human behavior, always enmeshed (as the latter is now so clearly seen to be) in the situations set by specific cultures and by specific social groupings? Our only comeback would seem to be that, whereas rats must be admitted to have very little social life and absolutely no culture, there are certain basic laws and principles which can still be studied more conveniently and with just as much validity in rats as in men.

But, first, let us consider further the nature of the new insights as to human behavior. Basically, these are merely the realization that every adequate description and every quantified rating of any aspect of human behavior must always involve and refer to a particular cultural milieu. For example, psychologists no longer conceive of I.Q.'s *per se* and abstracted from given cultural set-ups. Rather, they now realize that any given intelligence rating is always derived from performance with respect to the particular goals of a particular culture—goals with respect to which the individual in question has been reared and relative to which he now has to perform. Similarly, psychologists no longer think (or so I hope) of motivation *per se* but always of motivation with respect to those particular types of means-end lay-out provided by a particular society. Or, to take a final example, psychologists no longer consider emotional stability or its inverse emotional instability as something purely biological but rather as an entity which has final meaning only when defined by the norms and values of a going social group. For a man is emotionally stable or unstable not in a vacuum but by reference to the specific values which his culture prescribes. Thus, to have cataleptic-like trances was (we are told by Ruth Benedict) highly *de rigeur* on the part of women who would become shamans among the Shasta Indians in California. By having such seizures they then become counsellors of great power and importance. To quote: "It is clear that, far from regarding cataleptic seizures as blots upon the family escutcheon and as evidences of dreaded disease, cultural approval had zeized upon them and made them the pathway to authority over one's fellows. They were the outstanding characteristic of the most respected social type, the type which functioned with most honor and reward in the community."[2] But though cataleptic trances were thus highly approved among the Shasta Indians, obviously they would be rated as signs of emotional instability to-day in Cambridge, Mass. (or would they?).

But does this line of reasoning mean that we are now left with nothing but relativity? Do the words intelligence, motivation and emotional instability, taken apart from specific culture contexts, have no meaning? No, I do not intend to go as far as that. I would admit that there do still remain certain generalized and useful formal meanings for these terms. For, even though the actual material content and the accompanying quantified ratings of intelligence, motivation and emotional stability will not necessarily carry over from one culture to another—for example, from California to Boston, to the Trobriand Islands or to Cleveland—it will still be true that these terms are useful and necessary as generalized frames of reference. They are frames whereby we can compare behaviors of different cultures and the behaviors of different individuals in any one culture. Thus, as such formal frames of reference, what we mean by intelligence is probability of success in reaching goals; by motivation, probability of persistence in striving towards goals; and by emotional stability, probable tendencies not to exhibit unacceptable divagations in the pursuance of such goals. Intelligence is tendency to succeed, motivation is tendency to persist, and emotional stability is tendency not to exhibit unacceptable divagations.

[2] Ruth Benedict, "Patterns of Culture," Boston: Houghton Mifflin Company, 1934, p. 267.

On the other hand, it still remains true that we can not meaningfully rate the actual intelligence, the actual motivation or the actual stability of an individual raised in one culture, say, a Trobriand Islander, if set down in another culture, say, in New York (and having to react to a New Yorker's goals) or, similarly, those of a New Yorker set down in the Trobriand Islands (and having to react to the Trobriand Islander's goals). And there is still the further question as to in how far a New Yorker's intelligence, motivation and emotional stability, as rated in reaching one New York goal, will really carry over to, and are the same as, his intelligence, motivation and emotional stability in reaching other New York goals.

Let us consider this latter question further and let us begin with intelligence. Our first answer may be drawn from the dictates of common sense. Common sense undoubtedly would point out that some New Yorkers seem to be generally bright (successful) *re* all things New Yorkian, whereas others seem to be generally dumb (unsuccessful) *re* most things New Yorkian. This common-sense observation tends to lead, then further, to the notion of a single entity to be called "intelligence" or at any rate New York intelligence. And Spearman, as we all know, entertained this notion and sought to give it statistical validity in London and ended up with *g*.[3] However, as has already been argued, it is quite obvious that this London (or New York) *g* would not carry over, as such, to the Trobriand Islands. The man of high general intelligence in New York (or London) might well prove astonishingly dumb on the Trobriand Islands and, *vice versa,* the man generally dumb in New York (or London)—witness the Admirable Crichton—might prove surprisingly bright (that is, successful) on the Trobriand Islands. And so we return again to our original question. Is New York intelligence or London intelligence really unitary in spite of Spearman?

As a next step in the argument let us now shift our attention, from New York or London to Chicago. For in Chicago, as we all know, much brilliant statistical analysis has gone into showing that, at least along the shores of Lake Michigan, there are some seven, or is it nine, major subvarieties of intelligence. That is, Thurstone[4] and his students have demonstrated that Chicago intelligence seems to have at least the following major, and mutually independent, components: intelligence in words, in numbers, in spatial relations, in quickly perceiving visual or verbal meanings, in remembering relatively rote material, in inducing a general principle from presented particular data and in deducing particulars from presented general principles. That is, it has been found in Chicago that there are few interconnections between successes in reaching goals lying in the seven different directions of words, numbers, space, perceptually presented particulars, remembered particulars, induced relations and deduced particulars. Thus, for a man to be good (or poor) verbally does not mean any necessary concomitant tendency for him to be good (or poor) numerically, or spatially, or the rest. But, if he is good in reaching one goal in primarily verbal terms, he will also have a tendency to be good in reaching other goals which also lie primarily in the verbal direction (provided, of course, all the problems continue to be couched in good Chicagoese.).

Is this, then, our conclusion? Are there seven or perhaps nine basic kinds of intelligence capable of being universally generalized? At first sight, it might seem so. For it takes no great stretch of the imagination to suppose that what Thurstone found for Chicago would also hold for New York, London, for Cleveland or even (though I hate to admit it) for San Francisco. But consider again the Trobriand Islands. If Trobriand Islanders were raised in Chicago (and especially if they went to the University of Chicago) obviously there would be found the same outcome with them. But would we find these same seven "vectors of the mind" in the Trobriand Islands themselves? One doubts it. If a factor analysis were made of abilities on those islands, one might well find not seven but three or ten or fifteen dimensions of intelligence among which there might well prove to be some, to us, quite funny ones—such, say, as an ability to influence others by sorcery, which would correspond to no basic factor to be discovered in Chicago, Cleveland, London or San Francisco, though such a factor or ability might, perhaps, be found in Los Angeles.

My belief is, then, that that which Thurstone and his students have found are not seven biologically laid down aspects of intelligence but rather certain major directions of success prescribed by our Western culture-complex. We learn to perceive, to use words, to use numbers, to deal with certain types of spatial relations, to memorize nonsense materials (such as telephone numbers), to induce and to deduce. And each of these major learnings, whatever it may be based upon in the way of innate abilities, gets developed by the arrangements and accidents of our Western bringing up, more or less independently of each of the others. I do not pretend that such a hypothesis has really been proven, or that I have grounded it in actual data. I present it, however, as an *a priori* possibility and one which is, at least, worthy of examination. Moreover, it also makes sense when we return now, at long last, to rats.

For, it has been found that in all the intelligence

[3] C. E. Spearman, "The Abilities of Man," London: Macmillan and Company, 1927.

[4] L. L. Thurstone, "Primary Mental Abilities," Chicago: The University of Chicago Press, 1938.

problems which have been tried with rats there are extraordinarily small correlations between tendencies to succeed in any two different problems. One maze tends to correlate but little with another and mazes do not correlate at all with discrimination-boxes, or with puzzle-boxes, and the rest.[5] It appears, in short, that where culture does not operate (as in rats, thank God, it does not) intelligences (*i.e.*, tendencies to succeed) turn out to be very specific and almost unrelated to one another. This appears both in intercorrelation studies and from the further tests which, I understand, have been made on the Tryon and the Heron bright and dull strains.[6, 7] In short, I shall take very great biological specificity as my basic assumption. This notion I originally learned from Tryon.[8] I now believe with him (unless he has changed his mind in the meantime) that the biological and hereditary bases of intelligence are multitudinous, relatively narrow and very specific. Assortative mating, as it occurs in human societies, plus the coercive effects of our educational systems, have, however, the tendency to weld these multitudinous possibilities into a finite number (Thurstone's seven) culturally defined directions. The advantages (or disadvantages) of our systems of mating and of education tend to produce some human individuals who are good in most of the seven directions and others who are poor in most of them. But they also produce individuals who are good in some of the seven though poor in others. But again, I wish to contend that the particular seven categories, that we find, are not so many genetically segregating units but the product of a given culture. For once again, I would assert that the Trobriand Islander living on his islands might well be found by Thurstone to have not seven but say, fifteen or perhaps only three "vectors of the mind." And few, if any, of these fifteen or three might be found to coincide with the seven found in Chicago.

So much for intelligence. Consider now the second of our three sample variables—motivation. No good factor-analyses have yet, so far as I know, been made of human motivation in this country, although Spearman did claim to find in London a single generalized motivation factor, *w*.[9] However, though we do not have good correlation matrices for motivation, in human beings, we do have one for rats. E. E. Anderson[10] measured the motivation of 51 male albino rats in a variety of exploratory-driven, hunger-driven, thirst-driven and sex-driven problems. What did he find? First, there were no correlations from one drive to another. Secondly, in cases of the hunger and the thirst drives, he also found little or no correlations between the different measures of each of these drives by itself. That is, the rat who performed well as compared with the others, when all were under the influence of hunger, in one getting-to-food test did not necessarily perform well as compared with the others, when all were again under the influence of hunger, in another getting-to-food test. And a similar lack of intercorrelations was found between different measures of the thirst drive. Thirdly, in the cases of the exploratory and the sex drives, however, he did find evidences of something which carried over from one test to another. The rats who were highly exploratory in one apparatus (which encouraged exploration) did show some tendency also to be the ones who were highly exploratory in other apparatuses, which likewise encouraged exploration. Similarly, he found intercorrelations between different measures of the sex drive. The rats who copulated most frequently when a receptive female was present also tended to be the ones who dug most rapidly through sand to get to such a female; and the like. In short, some motivations, such as hunger and thirst, when measured (as in rats) outside the grouping effects of a culture appeared very specific and contingent upon the features of the particular situation. Others, however, such as exploration and sex appeared more general and less tied to specific situations.

When now we turn to human beings, although the corresponding studies have not been made with them, it seems probable that much evidence of generalized and more culturally determined drives would also be found. Thus, for example, some men in our culture would appear to be highly motivated in the whole area of scholarly pursuits but poorly motivated in those of sports or business. Others would appear to find great motivation in the general area of being good husbands and fathers while others would show but little of that generalized drive and would exhibit, rather, strong drives for writing poetry, painting pictures or for an exaggerated night life. But here, again, though we might well find such an appearance of "vectors of motivation," it would seem obvious that as in the case of the "vectors of intelligence," they would be mostly the products of a particular civilization and not of human biology. There are certain major goals which our culture sets up. And, growing up in this culture, some of us, due no doubt in part to our special inheritances but probably much more as a result of the accidents of early training and experience, tend to pick up some of these goals and others of us tend to pick up others of them. If, to use the vernacular, an individual has been "raised right," he may acquire most of them, but, if he has not been "raised right,"

[5] C. L. Vaughn, *Comp. Psychol. Monogr.*, 14: No. 3, 1937.

[6] R. C. Tryon, *39th Yearbook. National Society for the Study of Education*, 1940, Part I, 111–119.

[7] W. T. Heron, *Jour. Comp. Psychol.*, 19: 77–89, 1935.

[8] R. C. Tryon, *Jour. Comp. Psychol.*, 30: 283–336, 1940.

[9] *Op. cit.*

[10] E. E. Anderson, *Comp. Psychol. Monogr.*, 14, No. 6, 1938.

he may acquire but few—or only those of lesser repute. And an individual raised in a totally different culture might well acquire almost none of our major motivations. Again the doctrine, I am contending for, is that of an as yet perhaps unknown set of basic biological drives upon which given cultures then build their own smaller or greater number of culturally defined and specified motivational directions.

In fact, it would seem that Freudian psychology and its offshoots and derivatives considered (not as therapy) but as explanatory principles are no more and no less than some stimulating hypotheses as to how a given culture, working through the early family set-up and the early training procedures characteristic of the family and of the larger culture in which the family is immersed, may operate to emphasize in given individuals certain of the major motivational directions of the given society and in other individuals, others of those major directions.

Finally, turn to the last of our three variables which I have suggested—merely by way of example—that of emotional stability. This is, undoubtedly, the as yet least clearly conceived of the three. I defined it above as the tendency of the individual not to exhibit irrelevant and unacceptable divagations in the pursuance of a given goal. Let us turn now once again to rats. Let us consider Hall's pioneer studies.[11] You will remember that his rats were deposited one by one in an open field and records were kept of how much they moved about, how much they defecated and how much they urinated. And he found that the individuals who defecated and urinated most also tended to be the ones who did the least moving about. Such animals tended to freeze in one spot. Furthermore, he and his students have also found that two different strains can be bred—one a strain high in defecation, urination and immobility and one a strain low in these propensities.

The question which arises next is then, how general (or how specific) is this complex of responses? I confess to not being too clear on the point. But, for the sake of argument and to bring the discussion in line with what I have contended for the other two variables, I shall again adopt a similar position. I shall assert that these tendencies, which Hall and his students have bred for, constitute a relatively specific biological entity. And I shall contend further that these tendencies to defecate, urinate and freeze into immobility, taken by themselves, can not, as such, be evaluated as either emotionally stable or unstable. They are biological substrata upon which a culture may or may not build If the rats themselves had a culture, they might either disdain or promote to the status of seers and prophets the individuals who showed such propensities. In the first case, such propensities would be said to be symptoms of instability; in the second case, symptoms of genius and of greatness. I suspect, however, that rats, if they had a culture, would, like men, be more apt to find such propensities bad. They might well feel about them as we feel about bed-wetting beyond a certain age. Though in a strictly neutral sense bed-wetting may, for all I know, be a nice outlet for certain deep-lying motivational conflicts.

In short, my argument is that emotional stability, like intelligence and motivation, is, in the last analysis, an evaluative and cultural concept. It depends upon the rules of the given culture which behaviors are to be defined as unacceptable divagations and which are to be designated, rather, as incidental, colorful (and perhaps desirable) accompaniments of the carrying out of prescribed goals. Holding to this point of view, all I now wish to emphasize about such researches as those of Hall and his students is that they are important contributions to the problem of the uniqueness and the inheritability of certain funny types of propensity which cultures may then either utilize (or condemn) and weld into either what they call stability or into what they call instability.

This concludes my purely descriptive evaluation of the three basic psychological concepts—intelligence, motivation and emotional stability. I have emphasized that I believe all three to be *von Grund aus* cultural concepts. They can not be given specific contents divorced from the particular cultures in which they operate. And the significance of the work with rats was that it proved in each case that the inherited bases may be relatively specific and may have no simple one-to-one relationships with the finally molded culturally defined variable.

Finally, however, there is one further point which I, as a rat psychologist, must raise. For if, as I have been arguing, there are no unitary intelligence, motivation or stability functions necessarily common to all men in all cultures and still less common to men (who operate in cultures) and to rats (who operate outside of cultures), what, it may be asked, is the significance of most of our rat studies other than those on heredity? What about the thousands of studies on learning and on motivation and the smaller but pioneer number of studies on conflicts in rats? My answer would be to assert that, although rats have no culture, still the formal laws about the causation and development of intelligence, motivation and instability are universal in character and can be examined in rats just as well as, and far more conveniently than, in men.

And this brings me back once again to my original abortive title—"A Stimulus-Expectancy Need-Cathexis Psychology." And now I must add still a third neologism; namely, Conflict-Instability, so the com-

[11] C. S. Hall, *Psychol. Bull.*, 38: 909–943, 1941.

plete title would read: "A Stimulus-Expectancy, Need-Cathexis, Conflict-Instability Psychology. In other words, there are, I believe, three basic types of causal determiner (to wit, stimuli, needs and conflicts) which may be thought to be the respective primary causes of our three variables. And the equations involving these determiners and other factors such as numbers of repetitions, primacy, recency and the like, which connect these determiners to the final three variables, can be better studied in rats than in men.

The basic laws of intelligence concern the fact that successive re-presentations of arrays of environmental stimuli arouse in an organism "sign-gestalt-expectations" (as I originally called them) or what Hilgard and Marquis[12] have called, more simply, "expectancies." I am grateful for and shall accept their shorter term (although it is possible that I may use it in ways they didn't intend). Thus an intelligence functioning (that is, a success functioning in the reaching of a goal) is, as I see it, an expectancy on the part of the organism, aroused by that part of the stimulus lay-out which is immediately presented, to the effect that such and such performances or behaviors (if carried out) would be successful in reaching such and such a goal. These expectancies fundamentally are merely sets in the nervous system aroused by environmental stimuli. In the case of human beings such neurally-based expectancies are (as we know) often accompanied by consciousness; but they need not be. And, in any case, their definition does not involve the question as to whether or not they are conscious. It is pointer-reading behaviors which operationally define them. To sum up, the total causal factors underlying such expectancies are, as I see it: (1) the presented environmental stimuli; (2) the hereditary determinants of ability, whatever they finally turn out to be; and (3) the laws of learning (*i.e.,* sign-gestalt, or expectancy, formations). And the operation of all these basic factors and laws can be as well studied in rats as in men—even though (as I have insisted, probably by this time *ad nauseam*) the particular expectancies which get built up in men are determined and guided by particular cultural set-ups and even though, also, the amounts and kinds of repeated presentations (and the span of environmental entities, offered in any one presentation) are likewise, in the case of men, also culturally determined. The basic shape and equations of the learning curve can still be determined by the study of rats, and far more conveniently than by the study of men.

Turn, next, to motivation. The basic problems of motivation I have tried to epitomize by the hyphenation—need-cathexis. That is, motivations are derived basically from the arousal of needs plus the added fact that (through heredity and/or through training) certain types of goal-objects get cathected by a given need. Such cathected goal-objects, when reached, relieve the need. It now appears, further, that the basic laws concerning the arousal of needs—especially in the case of the simple viscerogenic needs—may likewise be successfully studied in rats. And the basic laws of cathexis whereby particular goal-objects get cathected by particular needs can also be studied in rats. Some important beginnings have, in fact, already been made by P. T. Young in his studies of food-preferences.[13] But the further (and humanly more interesting) problem of how, on the basis of the simple viscerogenic needs, the more complicated psychogenic ones (to use Murray's dichotomy)[14] get built up, probably can not (I must admit) be studied in rats—although it probably can be, and in large part is being, studied in chimpanzees. But problems such as the list of major psychogenic needs and of the concrete types of goal which get cathected by them has obviously to be investigated separately in each culture. In other words, although possibly we can study in chimpanzees the basic laws of what I have elsewhere called "drive-conversions,"[15] that is, the conversion of the libido of the viscerogenic needs into the libido of various psychogenic needs, we can not, I fear, approach the study of the actual goal-aims of the psychogenic needs of men in various actual cultures except by studies within those cultures.

Turn now, finally, to our third sample variable—emotional stability. Here we have as yet very few general principles (whether in rats or in men). But, as I have already indicated, I believe the appropriate neologism would be conflict-instability. That is, I am supposing that it is conflicts between two or more needs which are the basic causal determiner of those kinds of behavior which a given culture will declare to be symptoms of emotional instability. Those particular irrelevances and divagations (such, for example, as bed-wetting, nail-biting, stammering, flushing, cataleptic trances, visions and hallucinations) which a given culture may either disdain or capitalize upon, result primarily, when two needs conflict with one another. Along side of some major overt need some second covert need is at work and interferes with the attaining of the major goal. The individual is having to handle two (or it may be more) needs at once and it is this which causes the "funny" behaviors. Hall's rats would seem, for example, to

[12] E. R. Hilgard and D. G. Marquis, "Conditioning and Learning." New York: D. Appleton-Century Company, 1940.

[13] P. T. Young, *Jour. Comp. Psychol.,* 1932, 14, 297–319; *Jour. Comp. Psychol.,* 15: 149–165, 1933.

[14] H. A. Murray, "Explorations in Personality." New York: Oxford University Press, 1938.

[15] E. C. Tolman, "Drives toward War." New York: D. Appleton-Century Company, 1942.

have been interrupted in their exploring by their coincident fear. But as to the basic laws which made some of the individual rats more susceptible to such interference than others we as yet know practically nothing. We do not know whether Hall's more stable rats (in calling them more stable we are, of course, evaluating them as if they were human beings living in our culture) were so because they had inherited little fear or because they had inherited better "inner walls" for keeping their different need-compartments separated (to borrow Lewin's figure).[16] And, if we did know this for rats, we certainly do not know it for men. Is the emotionally stable man in our culture one who has no conflicting needs or is he rather one whose tough compartmentalized make-up keeps his competing needs from interfering? Or is he perhaps, quite oppositely, one whose needs do interfere but in such a way that the culture considers him a leader or a genius? We do not know. In any case, however, it is clear that, while it will be desirable to work out more of the basic principles of need-conflict with rats, it also has to be confessed that special studies with men in their own actual cultural set-ups likewise will be necessary. For, again let me emphasize that the "funny" behaviors which are termed instability in one culture may be called genius or at least a peculiar delightfulness and richness of coloring in another.

But enough. What, by way of summary, can we now say as to the contributions of us rodent psychologists to human behavior? What is it that we rat runners still have to contribute to the understanding of the deeds and the misdeeds, the absurdities and the tragedies of our friend, and our enemy—*homo sapiens?* The answer is that, whereas man's successes, persistences and socially unacceptable divagations—that is, his intelligences, his motivations and his instabilities—are all ultimately shaped and materialized by specific cultures, it is still true that most of the formal underlying laws of intelligence, motivation and instability can still be studied in rats as well as, and more easily than, in men.

And, as a final peroration, let it be noted that rats live in cages; they do not go on binges the night before one has planned an experiment; they do not kill each other off in wars; they do not invent engines of destruction, and, if they did, they would not be so dumb about controlling such engines; they do not go in for either class conflicts or race conflicts; they avoid politics, economics and papers on psychology. They are marvelous, pure and delightful. And, as soon as I possibly can, I am going to climb back again out on that good old philogenetic limb and sit there, this time right side up and unashamed, wiggling my whiskers at all the dumb, yet at the same time far too complicated, specimens of *homo sapiens,* whom I shall see strutting and fighting and messing things up, down there on the ground below me.

[16] K. Lewin, "A Dynamic Theory of Personality." New York: McGraw-Hill Book Company, 1935.

OBITUARY

DEATHS OF RUSSIAN BOTANISTS

A FEW names have to be added to the long list of Russian botanists—the victims of total war (see SCIENCE, 100: 43–44, 1944).

The most irreparable of these losses is the passing on April 19, 1942, of Professor Aleksandr Aleksandrovich Elenkin (1873–1942), one of the most prominent authorities on cryptogams. He was born on September 4, 1873, at Warsaw and educated in the university of the same city. After serving one year as an assistant in botany at the University of Warsaw, he was appointed a conservator of the St. Petersburg Botanical Garden and since then was always associated with that institution, which later was incorporated into the Academy of Sciences as its institute of botany.

Elenkin was active in all fields of cryptogamic botany. His first works were on the lichenology of Russia. He made several exploring and collecting trips to Finland (1898–1909), Caucasus and Crimea (1899), eastern Siberia and Mongolia (1902) and Central Russia (1903, 1907, 1910). On the basis of these collections he published his classical work, "Flora of Lichens of Central Russia," in four parts (1906–11). His studies of mosses are represented by another important work—"Mosses of Central Russia" (1909). In 1906 he was appointed director of the phytopathological station of the Department of Agriculture, and this turned his attention to mycology and phytopathology. He edited the journal *Boliezni Rastenij* ("Morbi plantarum") and contributed many papers to it. In 1910 he was given the task of describing the algae of the Kamchatka expedition of F. N. Riabushinsky (1908–09) and four years later published "Die Süsswasseralgen Kamtschatka's" and "Die Meersalgen Kamtschatka's" (In "Expédition à Kamtchatka" 2: 1–448, 1914). This was the beginning of his thirty years' study of the algae of Russia; in this field he became an undisputed authority in the Soviet Union and culminated his life's work by a masterpiece, "Monographia algarum cyanophycearum aquidulcium et terrestrium in finibus URSS inventarum" (1936–38), two volumes of which are published up to date and two others will be issued after the war. Besides this, he was the author of several papers on Darwinism and the philosophy of