

# Notes of a Psychologist Fieldworker\*

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Anthropological findings make interesting reading for clinical psychologists. The accounts of anthropologists who lived for years with "savages" and "primitive people" are read with admiration as testimonies of hardships scientists will undergo to obtain their material. There is a shade of envy mixed with the admiration for the clinical psychologist's research setting is usually the campus of a university, the hospital and the school. His informants are university freshmen, patients and children going to school. Of the anthropologists' methods of gathering data, the psychologists know little except that it must be a difficult procedure.

This paper is an account of a clinical psychologist who, as he continues investigations of methods of thinking, ventures into an isolated and perhaps less primitive culture than his own. The experiment conducted in the field is of no significance to the pursuit of my subject — thoughts and armaments of a psychologist to anthropology in the raw.

Contemplation of field work in a strange culture raised intense feelings of apprehension and displeasure, mitigated only by the writer's enthusiasm for mountain climbing and other outdoor activities. The apprehension was not connected with the prospects of travel and lack of physical comfort; nor was there concern about living with strange people. The misgivings were concentrated around the ignorance of anthropological "know-how." How does one contact strange people? How does one elicit their cooperation? How will they take to being tested? Will their responses make sense? How can one who is relatively ignorant of anthropological techniques undertake a task so inti-

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mately related to it? Training in anthropology instills in the student the notion of eventual field work in a radically different culture. Training in clinical psychology readies the student to work entirely in his own culture.

Anthropologists, friends as well as strangers, were encouraging and extremely helpful. With their cooperation and active support, an area and a tribe were selected: a group of Athabaskan-speaking bush-Indians in the Northwest Territories in Canada. A few months later, the author, his wife and four-year-old son were established at their headquarters. The community contained a Royal Mounted Police Station, a Communication Station, a Hudson Bay store, a Roman Catholic Mission and Hospital, two more missions, and some independent training establishments. Some Indians had settled around these institutions. The services of an interpreter had been secured with the aid of an anthropologist who had worked previously in this area with the same individual. While some resident Indians were interviewed, the bulk of the subjects consisted of individuals who lived in the bush. The experimenter and his interpreter took to the river to accomplish this

The following account, written in the first person, is an excerpt of notes made in the long hours spent on the river in a canoe and represents on the spot impressions rather than orderly thought and mature conclusions.

When visiting the Indian village I was impressed by the many ways in which the Indians were like the people in my home town. Of course, they did not resemble individuals from "the University tribe" of which I am a member, nor did they resemble members of "the patient tribe" with whom I have considerable experience. Rather, the Indians reminded me of that tribal group, the "people from the other side of the tracks" among whom I have some friends. Visiting with friends from "the other side of the tracks" tribe, I am aware of differences. I experience some tension due to these differences. But the likenesses between me and people who live on the other side of the tracks are much greater than the differences between us. I know that they have different values, occasionally different customs and ceremonies; still I can understand the differences, can cope

with them to my satisfaction and theirs. The differences are there but I and they have the equipment to live with them.

The Indians appeared so much like some tribes in my community that I started wondering. I had no reason to doubt that the cultural system reported in the anthropological literature was part of their lives, but it seemed to exert only a small influence in their daily existence. Something akin to the cultural myth of the "pioneering spirit" in my home community — a historically well documented saga, extolled on TV, in children's books and western movies, etc. However, the pioneering myth is hopelessly outdated and survives only in nooks and crannies of the modern industrial West. The important *current* myths governing the behavior of the West are rarely written about, are rarely the subjects on TV shows. In the same fashion, has the anthropologist overemphasized the historical aspects of cultural constructs in the Indians' lives? Do the cultural data represent ineffectual memories experienced at the fringe of existence or do they indicate the presence of a powerfully operating mainspring of on-going behavior? Are the myths, told by older folks, shrugged off by children as pleasant unrealities, or do they serve them as guiding stones when important decisions affecting life and livelihood are to be made? Anthropologists must have encountered great difficulties defining to what degree the ancient cultural determinants represent a focal or peripheral factor in the Indians' present social organizations.

The determination of whether a factor is central or peripheral in the existence of an individual is an important one in clinical psychology. Like the anthropologist, the psychologist obtains two types of data. He learns what an individual thinks and feels his life is like, what his experiences in the past have been, and how he thinks events of the past are reflected in his present life and behavior. The clinical psychologist may or may not agree with the individual's self-evaluation. It is the psychologist's task to relate an individual's history to his present behavior, with the understanding that the focal points of motivation are ever shifting and changing in time. What was most important, most vital to an individual yesterday may be of little motivational impact today.

Psychologists assume that they can evaluate an individual's life pattern and deduce a motivational hierarchy. Whether this claim represents wishful thinking, good guessing or justified theory is an open question. Psychologists act as if they were able to create a hierarchical structure of motivations which indicates what is focal and what is peripheral in an individual's motivational field. It is assumed that this hierarchy is capable of explaining present behavior, as well as predicting future behavior.

Psychologists deal with the problem of assigning quantitative values to a motivational variable primarily in two ways — artistically and theoretically. It is great artistry to sense intuitively the hierarchical relationships of an individual's motivational variables. The theoretical approach draws upon the notions of the orderliness of personality growth and development, upon theories of personality dynamics and the relative strength of drives — social, cultural, physiological and psychological.

An example of a point in theory would be represented by the concepts of sexual identification. It is postulated that with few exceptions all individuals in our culture learn by progressive differentiation, due to social pressure, to assume a sexual role and to derive satisfaction from it. The child accomplishes this goal but slowly and in the course of years. At times, the process of progressive identification is a source of great anxiety, confusion and disturbance. Various sequential steps of the process are postulated. The process is considered terminated when successful identification is said to have taken place. Failure in identification, it is held, will become focal points in an individual's motivational structure. We assume that success and/or failure in sexual identification will occupy a central position in any and all individuals in our culture. Many focal motivational forces will be linked to this development.

Psychological theory is yet inadequate to yield more than tentative answers and is presented here only as a sample of psychologists' thinking in meeting this problem. Like psychology, anthropology has been slow to develop an adequate system by which to assess the significance variables have upon the present

behavior of organisms — be the organism an individual or a society.

It soon became evident that the life of the Indian could not be understood unless one took into consideration those social institutions which exerted considerable influence on his existence. The Hudson Bay Company, the Royal Mounted Police and the Roman Catholic Church, to name only the most important ones, do exert on the Indians a set of conditions which influence profoundly every aspect of their activities. One, therefore, has to understand these social institutions, their histories, as well as present organization and policies, in order to comprehend the world of the Indian.

The clinical psychologist in a similar way needs knowledge of the present social environment of the individual he is dealing with, be it a patient or an experimental subject. This understanding of the social environment is derived from the data and theories of social psychology and/or sociology. The clinical psychologist is often in difficulty when he needs to integrate his own data with data of a sociological nature. There exists at present no consistent body of theory by which the transition from the individual to social data can be accomplished. Clinical psychologists need sociological concepts but they can only be borrowed; there is no way by which they can be assimilated and amalgamated, at present, in clinical psychological theory.

I ask the probably naive question whether cultural anthropology does not face a similar problem in the transition between anthropology and sociology. If it does, it faces, like clinical psychology, the task of unifying principles with ancillary fields. The absence of unifying principles makes communication difficult between professions. The anthropologist centers his interest and convictions around culture and the clinical psychologist his around the individual personality, group interaction, etc. A try at unifying these two principles would be a necessary and possible first step.

When meeting the Indians as an experimenter, my reactions ran the gamut from exhilaration and animosity to exasperation and inertia. They let me wait hours and days without any rhyme

or reason, or so it appeared to me. They were unwilling to part with information, they were often suspicious. At all times I was the one who had to adjust to them, not one of my subjects adjusted to me. All this appears to be a necessary condition of successful field work. There were times when things went smoothly, my work progressed, my curiosity knew no bounds. My notations at the top of the data sheet indicate my feelings at the time of the experiment. It easily can be seen that my data, in some instances, were richer, more exhaustive; my subjects more informative than at other times. At these other times, perhaps, I had spent two days chasing my subjects unsuccessfully. My data sheets would clearly contain an indication of my frustration. The data obtained under these conditions were very biased usually. It helped, in the evaluation of the data, to know the conditions under which they were obtained. Not only I, the experimenter, but my interpreter, too, was a variable instrument. There were days when he was superb; there were others when he was a reluctant and only partially adequate instrument. As time progressed and the end of my experiment approached, field work had worn me thin. I became a "so-so" instrument. I was hungry, filthy, uncomfortable and a bit bored. My feeling for the Indians and their interminable concern for subsistence forced me into psychological distance, as a method of self-protection. The last part of my data must be scrutinized with this in mind. The instrument of the experiment, in this case the experimenter, had deteriorated to the point of doubtful usefulness. The latter data should be either eliminated or reported with a special warning.

Again, clinical psychology and anthropology meet a similar problem. Both in their activities, the one with patients and the other with informants, are not only experimenters, they also are part of the investigating tools. The psychological and anthropological field investigators like all others are not totally objective observers; they are deeply human and carry this quality into their observations. The human error is a limiting factor and a magnificently enriching variable at the same time. How to decrease the variable, human error, while keeping intact the richness of observations born in subjectivity is a source of grave concern in clinical psychology. The best solution known, at present, con-

sists of the examiner possessing considerable self-knowledge. He must know his subjectivity to the last detail in order to supply curbs in one instance (human error) but to let himself go where his intuition leads him using a semiconscious trail or inquiry. Considerable training is given to the clinical student in self-observation and self evaluation. To my knowledge this aspect of training is not stressed in an anthropologist's background. The freedom of controlled subjectivity has little in common with the state of freedom from neurotic pain which can be gained by undergoing an analysis or some form of psychotherapy. The freedom gained in psychoanalysis may, but need not result in free-flowing, controlled subjectivity. The goals of psychoanalysis are determined by the analysand's individual needs which often do not coincide with the personality criteria required of a good anthropological fieldworker. It would represent a fascinating and formidable task to create a special form of analysis designed to alter only those aspects of a worker's personality which come into play in the field work process, while leaving the rest of his personality unchanged.

Short of this improbable type of analysis, other means can be used to insure that the person in the field is suited to his task. Religious groups selecting candidates for missionary work use psychometric and psychological examinations in order to avoid field work failures. Similarly, the budding anthropological student should be guided, with the help of psychological selection procedures, into the most suitable area of his functioning as determined by his needs and capacities. Anthropologists, like all other people, should use their neuroses to best advantage and fight them in their analysis only when their problems become crippling or painful.

An anthropologist with a pleasant and socially approved neurosis, which manifests itself by his need to talk often in a friendly way with a lot of people and thereby to receive many silent demonstrations of affection, perhaps, should not punish himself too hard by doing anthropological field work under conditions of social isolation. Only when his gregarious self is also in need of doing field work under conditions of isolation will he get into emotional difficulties. This unhappy anthropologist may

have two choices. He may wisely decide to become a theoretician or he may decide to undergo an analysis to deal with his conflicts. He may come out of the analytic process with the insight that he has cumbersome, infantile aspirations; therefore, give up field work, or he may emerge with his neurotic gregariousness reduced to the point where he could tolerate field work (which is different from enjoying field work). Any number of other solutions to conflicts may be dealt with successfully in his analysis; he may turn out a much happier man, but still unable to tolerate the rigors of field work. There are many anthropologists who have the "right" kind of neuroses qualifying them to go into field work and to enjoy it. The problem is one of *selecting*, not one of *creating* a right type.

Related to the above point of individual tolerance is the problem of interviewing techniques. At first, when interviewing Indians, I tried to be exact in my formulations, requiring from them exact responses. I believed not unlike an experimenter sitting in an experimental room in the university facing an experimental rat or a college freshman. This search for exactness yielded highly unsatisfactory results; my subjects were "cold," and so was I. Only gradually did I realize that I was behaving contrary to my own training. When examining a non-middle class subject, I know how to make him feel comfortable. I know how to make my subject be free and trusting. I know by his posture when he relaxes; his speech tells me by its hesitation when he has conflicts; his pauses fill in the story; his breathing is smooth when he feels at ease. When I used my tried-and-trusted knowledge of therapeutic interviewing on my informants, the results were startling. I suddenly knew what went on in an interview. From the subject's physical reaction, I often knew whether he liked or disliked a particular question — whether the answer was given with hesitation or in full cooperation. In evaluating the experimental responses, I knew more than the translator's report. I felt I had knowledge as to the reliability of the response.

Anthropologists would benefit, it appeared to me, from training in what is known to the clinical psychologist as non-verbal communications. Perhaps it would be worthwhile to re-



commend opening courses in interviewing techniques to interested anthropologists. The purpose of the introduction should not be to instruct the dynamics of unconscious verbal behavior, but rather to familiarize the student with the telling signs of content above and beyond the spoken word.

As an afterthought it occurred to me that I know of no anthropological field report stating at the beginning the emotional attitude of the fieldworker toward his experiences and physical surroundings. It may be valuable to know how good the instrument was when reading its product.

When interviewing my first subjects, I behaved in the quaint and genteel ways of a middle class professor. I demonstrated my good upbringing by sending my interpreter ahead of me into a tent in order to inquire whether my presence was acceptable. In short, I behaved the same way I would back home — I first inquired whether or not my presence would be disturbing. My translator behaved rather oddly to my experience. He never knocked at the tent; he never coughed, announcing his presence. He usually walked into a tent, took off his shoes and came directly to the point: "A white man-doctor wants to talk to you — ask you questions." After a few days of poor returns in terms of interviews, he took me aside and told me that the Indians thought I was haughty. Then interpreted my "politeness" as an attitude of "they were not good enough." I changed my ways, adapted my interpreter's ways and got used to walking into any tent directly without preliminaries. It worked well. Repeatedly I was told by the Indians that I was the first white man ever to come into their tents. It made my work easier.

From many anthropologist friends I have been told that they carefully refrain from going *after* their information, that they rather wait for their informants to come to them. Thusly, the data yielded by the informants, they assure me, were less subject to bias because they were given voluntarily. Relying only on volunteers introduces a bias into the sample of informants. The informant who is motivated for one reason or another to talk to the white man often represents an exception

to the many who want to have no dealings whatsoever with the white man.

Should one go after informants thus biasing his data; or should he sit back and wait for informants thus biasing his data? Clinical psychologists face a similar problem in dealing with a patient, a source of a great deal of research information. Provided the clinical psychologist uses only data from patients coming voluntarily to him, he will bias his sample. Many individuals demonstrate the same or similar symptoms as our patients do but they are never contacted by psychologists. Some clinical psychologists, recently, have carried their investigations into the community, interviewing individuals in their homes. Such investigations have been difficult because of the subjects' resistance to yield psychological information about themselves. The data, therefore, may represent considerable resistance related bias. Psychology, as well as anthropology, deals with the problem of data and sample bias.

There was one thing I strongly felt: none of the tests that clinical psychologists use are of any value in the investigation of non-westernized cultures and/or individuals. It would lead too far to indicate here but a few of my objections.

It is generally accepted that intelligence tests cannot be applied across cultures. It is equally accepted that the assumptions underlying the concept of personality differ from culture to culture. As all personality tests are based on one personality theory or another, which in turn is validated in our culture only, it follows none of our present personality tests can claim validity outside of our culture. The few attempts to revalidate personality tests in a non-western culture have been fairly successful. To my knowledge no such attempt at revalidation of the Rorschach Test has taken place prior to its use by anthropologists and/or psychologists in their specific field work.

Let us take as an example of revalidation problems the concept or projection itself. When we present an ink blot to a subject in our own culture and we say to him: "Tell me what you see in it, what it reminds you of, what it makes you think of" — we are implying a number of conditions. People in our

culture are used to make-believe, they are trained in making differentiations between real and unreal. When a subject says: "It reminds me of a bat," the examiner and the subject "know" it is not a real bat. Both know that they have in common a frame of make-believe, tacitly assumed by all members of our culture, that something in the ink blot is like a bat which is not a real bat. We must recall that some primitive people reacted at first to a projected movie by throwing objects at the villain on the screen.

It appears as if the assumption — "We know that something is not real, but we both can act as if it were" — cannot be made outside our own culture. Some individuals in our own culture act as if the bat were a real bat. We, therefore, conclude that such a person in our culture is deviant. We infer that the individual fails to deal properly with "as if" conditions. This, in turn, we interpret as ego deficit. If many individuals in our culture respond with bat to a given ink blot, they all do so in reference to an "as if" condition. If many individuals in another culture respond with bat, little inference is permitted until we have established that the "as if" condition is part of their response. If it is not, and there is some evidence to support this belief, the response "bat" means and implies different interpretations than the ones we can make in our own culture. The Rorschach can be used as a projective test if the condition "as if" can be assumed to exist in the testing situation. There are other basic assumptions which must be verified before the Rorschach Test can be used in a different culture.

On its own, we all know that the Rorschach Test is not a reliable instrument. The test is only as good as the individual who interprets it. The good interpreter brings to his task a vast knowledge of noncodifiable Rorschach information. He registers not only the subject's responses, the time delay, but also his comfort, or anxiety while responding. The examiner who has tested many subjects has knowledge of clinical cues, of pathological insertions, of disturbed response sequences and many other indications of why the subject reacted in a given fashion. All the data which the examiner receives and evaluates, which are not part of the Rorschach response proper, are culture laden

to such a degree that they cannot be used as and to interpretations. If these are not used on the other hand, the Rorschach is reduced to being a questionable source of information.

On the danger of being called overconservative, old-fashioned and what is worse, anti-Freudian, I must say that I don't believe that psychology can offer any personality theories which would have meaning outside our own culture. A paper destined for publication in a psychological journal will deal with this problem more exhaustively. Let me present here one sample of my doubts.

I observed that nearly all Indians giggled at the mentioning of the word "angry" — but they had no difficulty describing and discussing angry behaviors. In our own culture, individuals who have deep feelings about "angry" usually don't like to talk about it. Here we had a group of people who do not like to be angry but who, at the same time, do not mind talking about it in a rather relaxed and easy manner. On another occasion, I observed two Indians fighting. Many Indians surrounded them. Assuming that the group experienced repressed fears and anxieties about the overt aggression, according to our personality theories, they should have reacted in a predictable manner. The group should have manifested signs of repulsion, excitations and, consequently, should have attempted to prevent the fighting. Neither or these proved to be the case. The Indians watching the fight took little action. They thought in general that fighting was not a good idea; it disrupted relationships, created bad conditions, but they also recognized that fighting did occur and could not be avoided. What cannot be avoided cannot be condemned, they seemed to reason. I could not help but think that the Indians consciously disliked aggression, saw no sense in it. Nowhere could I find evidence that they denied or repressed their aggression.

Theirs was an adaptation to aggression resulting in genuine indifference to it, something to be avoided if possible. I speculated a bit about this and how it possibly could have come about that some investigators inferred that the Indians' apathy was related to their repression of hostility. Yes, the Indians are apathetic and there are many reasons for it: economic, social and

cultural. The Indians dislike aggression. Aggression and apathy often are related in our own culture.

Let us take a look at this phenomenon in our own society. The problem of aggression is of central importance in our society. (Is it central in Indian culture?) Individuals cope with aggression in a variety of methods, i.e., counter-aggression, flight, retreat, acting out; the aggression can become self-directed and many more coping methods are known. Should an individual for intrapsychic or environmental reasons be unable to deal with his aggression, he presumably represses these feelings. They now emerge as hidden aggression, depression, reaction formations, sublimation and other defensive maneuvers. Apathy is related to depression and thusly often encountered as a symptom in individuals who have problems with aggression. Could it be that observers, having noted few indications of overt aggression among the Indians, having also noted their apathy, concluded that the two symptoms were related to each other, aggression being the common denominator? If so, the observers simply superimposed the dynamics of individuals in our culture upon observations made in another culture. The observers, born and bred in western white culture, cannot grasp that different cultures developed personality dynamics different from their own. They cannot see and accept that in a given Indian culture aggression represents a peripheral phenomenon which can be adjusted to with indifference, avoidance and caution.

To the western white observer the word apathy is the proper descriptive word because it represents an aspect of *his* adjustment armamentarium. In our culture the optimal solution consists of compromise between opposing conditions; few indeed are the situations which are absolutely unalterable in our society. The Indians, on the other hand, are surrounded by what appears to them as unalterable conditions. The possibility of adaptation by accepting the source of the frustration as an unalterable condition, to be lived with, is alien to our way of thinking and, therefore, not adequately stressed. Similarly, it is my personal observation that when Indians refrain from aggression, it does not represent an unconscious process, and it would have to be an unconscious process to permit *interpretation* like regression,

reaction formation and others. It could be equally substantiated that the Indian's attitude toward aggression is a conscious one. The ego may have evaluated aggression as a useless, panic-creating, society-disruptive factor. The ego, therefore, may be formed in such a way as to consciously avoid aggression-laden situations. Other cultures in history held similar views.

What is regarded as a first step in the direction of understanding personality dynamics is the necessity to make observations which are *not* based on any assumption stemming entirely from our own. Perhaps observations should be made by investigators who have had special training in psychological theory and its limitations. Even then it will be difficult to reduce the built-in cultural blinds but intellectual insight can be used to advantage to pierce the blind spots of perception. Field observations made by clinically trained investigators, capable of differentiating between conscious and unconscious processes, between expression and projection, between reaction formation and direct reaction to a situation, will yield a new and different understanding of foreign cultures. It is not too surprising that when western cultured field investigators (untrained clinically) present their collected material to western cultured psychologists (clinically trained), they will end up agreeing that some of our personality dynamics can claim to have universal validity. Whether they do or not is a question we will only answer when differently trained observers will yield less biased results.

Anthropologists and psychologists share the problem of their "boundedness." The anthropologist, in dealing with other cultures, is bound by his own cultural structure; the psychologist, in dealing with individuals, is bound by his own personality determinant. The two disciplines are interdependent in their need for freedom from bias. The psychologist has some tools to make the anthropologist a better instrument for gathering data; the anthropologist, on the other hand, can tell the psychologist about his bias in constructing theories of personality. Unfortunately, this necessity for cooperation is far from accepted and/or practiced.

It was the fact that doubts were created in my own theories that I think was the most fruitful result of my field work ex-

periences. Parenthetically, my data bore out clearly the cultural hypothesis of my experiment.

Working in the field I now know what culture means when an anthropologist talks about it. The power of the subjective experience of this concept, by living it, is overwhelming. All clinical psychologists would profit from having experienced cultural differences. There must be other and less cumbersome ways to experience culture than to live among Indians. This is a hope born out of my personal discomfort in doing field work. Intellectual enjoyment and satisfaction can mitigate but not undo hardship. I salute with respect my anthropological brethren who go out and do field work as a matter of course (without complaining about it in their publications).

Psychologists and anthropologists need to get together, not only because they share interests but because they have common purposes and problems. I do agree to the division of labor: let the anthropologist go out and get the data in the field, have the psychologist keep company with a rat to convert observations into theories. Let both of them have their pleasures and pains — but let them get together in their data and problems.

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