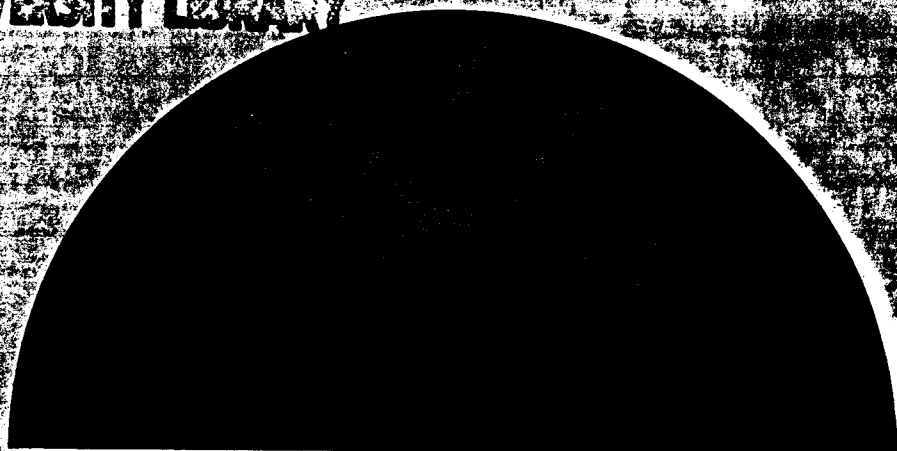


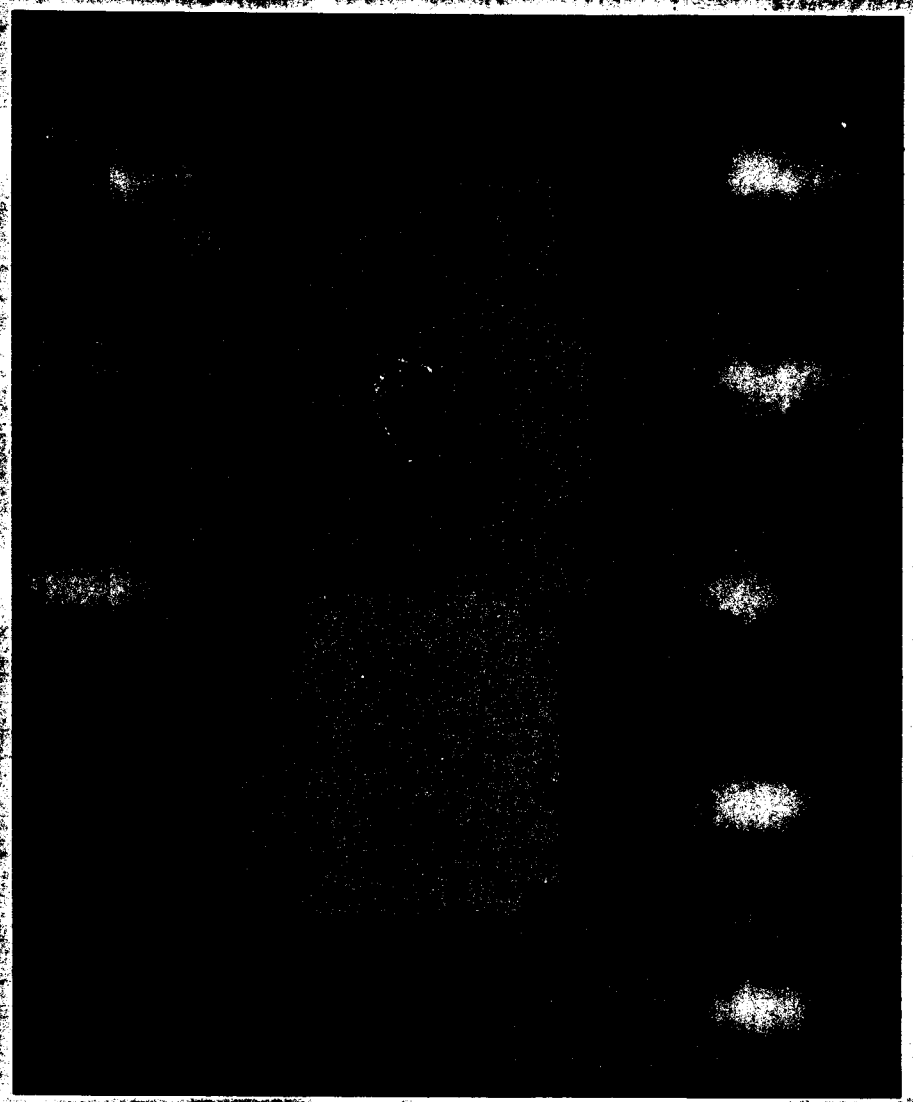
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the problems of fieldwork. The real problem of fieldwork lies in the fact that our professional subculture does not help us to come to terms with the merging of strongly emotional and strongly instrumental aspects of field relationships.

Meanwhile the following recommendations may help to reduce common feelings of guilt. Realize that instrumentality is a common aspect of relationships, also in the host society. Keep promises made in the field, e.g., as to writing government recommendations, or popularized and locally available accounts of the topics studied; or as to keeping in touch. Produce scholarly work that although not immediately meaningful to your informants, is yet of such quality that it does justice to the intensity of the fieldwork experience. Try for once to produce anthropological texts in which the subjects are not dehumanized into mere puppets. Engage in political action to further the interests of the people studied, involve them in such action, and prevent your academic work being used to reinforce or legitimize such material exploitation as they are subject to. Realize that although your report is cast in the mold of current anthropology, which is just one ephemeral subsystem of one historic society, it is also a contribution to a more lasting undertaking: the pursuit of human knowledge, which may hopefully transcend our own society and its embarrassing incentives. And as a last resort, write a paper like Bleek's; although this does not solve the problems, it helps at least to state them.¹

NOTE

¹ I consciously adopt an idealist position here. I am aware of the alternatives. Anthropology could be seen as an intellectual tentacle of imperialism, the anthropologist as an agent of cultural and even political domination (cf. Copans 1975; Asad 1975). Also, the dilemmas of fieldwork such as discussed by Bleek could easily be rephrased in the Marxian contradiction between use value and exchange value, where the anthropologist (often operating in domestic or precapitalist niches of the capitalist world system) tries to manipulate such claims as provided by a noncapitalist idiom of social relationships (kinship, friendship), in order to secure data that he then profitably transforms into commodities for the capitalist academic market. My present argument would then amount to bourgeois false consciousness. But while such perspectives would add system and precision to Bleek's ideas, they do not do justice to the fieldwork experience. Where is the materialist or radical analysis of fieldwork as a model that compels the anthropologist to do *both*: lovingly embrace the idiom of the host society, and sell it out?

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A Brief Note on Dwindling Research Opportunities for Anthropologists

by ROBERT T. TROTTER, II

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I have recently become alarmed by the growing sentiment of despair over apparently diminishing anthropological research opportunities that is evident in certain anthropological circles. At regional and national meetings there is a growing chorus of pessimists discussing increased costs, problems of distance, governmental restrictions (both ours and *theirs*), the likelihood of being considered a CIA agent, and a plethora of other difficulties associated with anthropological field research. Simultaneously, students agonize over the difficulty of finding a group or a research problem that other anthropologists have missed. It seems that anthropologists, like Coca-Cola and Volkswagens, are now relatively common in even the remotest parts of the globe.

These reports and complaints caused me considerable concern for several reasons. First, I am a natural pessimist, a condition I discovered while watching old Peace Corps commercials. I always saw the glass of milk in the commercial as being half empty and probably sour after so much use. Second, if these reports are accurate, then the research opportunities that are left might not last long enough for

me (and, of course, my esteemed colleagues) to make full professor. This possibility produced a state of acute anxiety, which I felt could only be eliminated through a direct objective in-depth analysis of the problem. I was spurred on in this endeavor by the realization that many famous anthropologists have made their reputations writing about anthropology rather than about other cultures. I came to the additional realization that by taking up this problem, I could stretch out my current areas of research a bit further down the promotions track.

THE DISCOVERY OF RESEARCHABLE PROBLEMS. My research problem became to find a relatively inexpensive, close, unfunded, and multicultural (but all English-speaking) research setting that did not deal with any questions vital to our national interest or to the security of our nation. After casting about futilely for a while, I chanced on an invitation to a holistic health conference in San Francisco and decided to explore it as a potential scene for anthropological inquiry. This choice had several obvious advantages (not the least of which was the term holistic in the title), and I was encouraged by the activities encountered there, even after a short time in the research setting. I photographed a Chinese lion dance, recorded field notes on acupressure therapy, and did a stint of participant observation in art therapy. In the latter session everyone agreed that my creative use of chartreuse, khaki, and greenish-browns embellished by winged pith helmets eloquently expressed my concerns over the need for new avenues of anthropological research.

Then came my incredible good fortune in attending one of the keynote speeches, a thing I do not normally do at conferences. The conference participants were addressed by a Native American medicine woman who briefly expounded on a subject she called "The Theory of Resistentialism" or "why peanut butter and jelly sandwiches always fall to the floor jelly side down." She explained that the theory was first generated by a cross-cultural encounter that was part of her experience in college, and she presented a considerable amount of anecdotal information to support the theory. Most of the anecdotes revolved around the behavior of her car, an ancient VW that hates the city of San Francisco and runs smoothly everywhere but there. During the break following her presentation I recorded comments from several other scholars who had been in the audience. Most thought the anecdotes had been allegorical, since ancient Volkswagens were considered to have little to do with either peanut butter and jelly sandwiches or the laws of gravity. One theologian discussed the rampant animism of the anecdotes, and two clinical psychologists nearly came to blows in an argument over the probable symbolic roots of Volkswagens and sandwiches. Fortunately, I was able to continue my anthropological approach to the subject by asking to accompany the informant to lunch, after the other speakers had made their presentations. This type of

key-informant interview has always been central to anthropological research, and it proved useful in this case. After several of us succeeded in starting the informant's car by pushing it a couple of blocks, we drove to another part of the city where we discussed the theory of resistentialism over an organically fertilized lunch. My informant clarified the issue by stating that the theory of resistentialism explains, in layman's terms, the ability of certain inanimate objects to resist or contravene human will power; thus, cars do not start in cities they do not like, and peanut butter and jelly sandwiches follow their notoriously noxious bent.

EXPANSION OF RESEARCH DESIGNS THROUGH TEAM APPROACHES. I returned home filled with hope, after brief sojourns into Mexican-American folk medicine and nutrition therapy. I felt that I needed only to find a comparative cultural base for studying the theory of resistentialism, to demonstrate that the pessimists had been premature in their prophecies of doom for anthropological research. At home, I exposed several key informants (a psychologist, an anthropologist, two sociologists, and the janitor) to the theory with interesting results. The theory is obviously widely recognized, although not often labeled.² One informant referred me to the pioneering work done by Klipstein (1976) on a similar phenomena in the physical sciences called Murphy's Law, which has, upon occasion, been incorrectly transferred into the realm of human behavior much like the earlier misapplications of Darwin's work to social theory. Several other informants related anecdotes describing the contrary behavior of automobiles. All of the married female informants (with children) plus several of the unmarried male informants had had recent experiences with peanut butter and jelly sandwiches that tended to validate the theory often enough to consider it worthy of study in cross-cultural environments.

EXTRANEOUS IMPEDIMENTS TO UNIVERSITY BASED RESEARCH. I decided to take a daring, frequently recommended but often ignored, step, and not only provide ethnographic evidence for the theory's existence, but also to do a bit of hypothesis testing (in the hopes that a mathematic exploration of the problem would provide me with at least one article beyond the original description, even if the results turned out negative). Given the current state of affairs at my university, both in terms of promotion and of litigation, I conscientiously presented my research design and proposed research topic to the University Human Subjects Experimentation Review Panel (UHSERP). After lengthy explanation and strenuous negotiation, it was finally brought home to the panel that it would be extremely difficult to obtain signed informed-consent forms from the peanut butter and jelly sandwiches. One member of the panel did insist that the principal investigator and any research assistant sign release forms absolving the university

of any physical, social, or psychological damage that might accrue as the result of the experiment. This individual was a former cancer researcher and was concerned over the possible problem of extended exposure to heavy concentrations of peanut butter and jelly, not to mention enriched white bread.³ This particular encounter is typical of the experiences of other researchers and exemplifies the difficulties that anthropologists and other social scientists have in justifying their research techniques and research problems to human subject review panels composed primarily of physical scientists.

THE PROBLEMS OF EXPERIMENTAL DESIGN. The experimental design was originally conceived as a double blind, but this became impracticable when no one was found who would put out their eyes in the name of science.⁴ Finally, it was decided to use a substance, deodorized chalking compound, that had the same consistency and specific gravity as peanut butter and jelly as a control. The chalking compound was spread on the bread and dropped the same number of times as the peanut butter and jelly sandwiches to allow a statistical comparison of the control drops to the peanut butter and jelly drops. Our null hypothesis was that peanut butter and jelly sandwiches dropped to the floor jelly side down at the same frequency as those with the chalking compound. We were assuming a 50% jelly-side-down drop rate (JSDR). Our alternative hypothesis was that the jelly side would be down significantly more often ($p < .05$) than not. We felt confident in testing the peanut butter and jelly against the chalking compound, since none of the anecdotal information about the theory of resistentialism referred to chalking compound. However, use of the chalking compound could present a complicating factor in the experiment, since we had no idea of the exact relationship between human beings and chalking compound sandwiches. This relationship, or the lack thereof, presents an excellent opportunity for further research.

The original pretest was scheduled for the principal investigator's kitchen; however, this turned out to be an untenable situation (although it proved useful to the original object, that of searching out new research settings for anthropologists). The principal investigator became involved in a lengthy exploration of role relationships of spouses in American culture, revolving around the question of responsibility for the removal of experimental artifacts from various surfaces, primarily the floor, at the conclusion of a particular research session. This somewhat serendipitous spin-off from the original study may form the basis for future research by the author on this subject.

Before conducting the experiment the author experienced a certain amount of difficulty in finding research assistants to drop the two varieties of sandwiches. It was decided not to use a mechanical dropping device, since the theory of resistentialism only predicts the behavior of peanut butter and jelly sandwiches in relation to human droppers, not to

mechanical devices. Out of all the students who complained of not having research problems, only one volunteered to work on the project. Unfortunately, this student had to be let go halfway through the project when it was discovered that he was eating nearly one fourth of the experimental objects in order to stretch his financial aid a bit further. This student was replaced by my two children, which allowed us to test the theory in relation to both males and females. Also, the destruction of the experimental projects was reduced since the children only took random bites from the sandwiches (both varieties).

PROBLEMS OF ANALYSIS. The experiment was eventually conducted in a laboratory at the university. Although clean-up was easier, this does pose some problems in generalizing the results to include the home environment. Further testing will be necessary to generalize the theory to various rooms in the home.

Unfortunately, there have been some serious problems in analyzing the results of the experiment. Each time the data is run through the available SPSS (Statistical Package for the Social Sciences) program on the university computer, the computer indicates a fatal error, even though the data have been cleaned and assembled by experts. Strangely enough, the same program works perfectly for any and all other experimental data. When we tried tabulating the data by manual means (first by calculators and then with pencils), we had an extraordinary run of battery malfunctions, electrical failures, and broken pencil points. I am relatively certain that these problems are due to coincidence and will be overcome in the near future. However, I have requested that no one transport the available data by car, especially by means of ancient Volkswagens.

One other note of caution should be interjected into an otherwise positive report. I have recently discovered that the concept of holistic health is felt to be a potential threat to national security, as well as being thought to be vital to our national interest. It is even possible that both FBI and CIA agents participated in the conference, posing as scholars. I had my own suspicions of the two clinical psychologists mentioned earlier, even at that time. However, I am reasonably certain that none of the anthropologists in evidence were CIA agents, due to an empirical test I have developed to distinguish anthropologists from other professionals. This test is based on a complex anthropological behavior pattern that can be termed the "principle of gormandancy." The test of that principle, in its simplest form, indicates that if you talk for 15 minutes to an individual claiming to be an anthropologist without that person having mentioned food or drink (or where to find it) at least three times during the conversation, that individual is not an anthropologist. A thorough discussion of the behavioral complex associated with the principle of gormandancy is outside the scope of this paper, however, and at the present

time it is sufficient to know that we can now scientifically distinguish between *them* and *us*.

CONCLUSIONS. Even if the quantitative testing of the theory of resistentialism takes longer than originally expected, this exploration of the availability of anthropological research opportunities must be considered an unqualified success. Beginning with a simple research setting the author was able to generate an extensive research program with ever-widening ramifications and serendipitous spin-offs. Although the central subject was humorous, this clearly indicates that relevant anthropological research can be conducted in any available setting.

This condition leads to the conclusion that there has not been a diminution in research opportunities for anthropologists. In the past, anthropology has been eminently successful in exploring areas considered marginal by the other social sciences and equally successful in causing those areas to become central to the theory and practices of those same social sciences. Anthropology has always had far greater impact than its numbers alone would predict. Part of this is due to the marginal position anthropology has taken in the social sciences, since innovations nearly always come from the marginal areas of a system. And part of it is due to the focus, in anthropology, on mundane and on marginal topics. Therefore, if there has been a dwindling of research opportunities in anthropology it is only because of increased conservatism within the discipline and a decreased willingness to look at marginal areas of research. If such a trend should continue for a significant length of time, then the research opportunities for anthropologists will dwindle to the point that we will be indistinguishable from the other social sciences, but until that time our opportunities will only be limited by our own imaginations.

NOTES

¹ Some anthropologists may be upset by what is obviously a new trend among informants to provide not only information, but also to organize and label their own theories and paradigms. However, I feel this tendency will ultimately benefit anthropology. Not only does it allow us to collect data more rapidly and report it more cogently, but it also eliminates charges of increased jargonism when we demonstrate that the labels came from the emic, not the etic perspective.

² One of my colleagues, Dr. James Aldridge, an experimental psychologist, informed me that some work had been done in psychology on a potentially related phenomenon, "The Stability Drive of the Blueberry Pancake." It seems that if a blueberry pancake is dropped down an elevator shaft it will move with increasing speed toward the bottom of the shaft, which can be considered a temporary state of stability. The closer the pancake gets to the state of stability, the faster it moves, thus empirically demonstrating a stability drive in the pancake. Naturally, once it reaches the stable state, the pancake stops moving, indicating the satisfaction of the stability drive. The correlation between this phenomenon and the theory of resistentialism is not within the

scope of this paper, but may form the basis for further joint experimentation by myself and Dr. Aldridge. Anyone wishing to take part in research on this or related subject is encouraged to contact the author.

³ Her arguments about enriched white bread were cogent enough to cause us to change to stone-ground whole wheat. This makes the results of the experiment less generalizable but safer to the participants. We will be doing a separate analysis of stone to ascertain its impact on the theory of resistentialism, as an intervening variable, at a later date.

⁴ Again, I am indebted to Dr. James Aldridge for his concise definition of a double-blind experiment, "someone who puts out both of their eyes before dropping the peanut butter and jelly sandwiches, rather than just one eye." The latter is a single-blind experiment.

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Comment on James N. Kerri's "Applied Anthropology, Urbanization, and Development in Africa: Dream or Reality?"

by ILSA SCHUSTER

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In his article, Kerri (1977) "dreams" that development strategies can be devised on the basis of anthropological research findings on motivation for rural-urban migration and the myriad urban social problems that result from mass migration to towns. He suggests that "developmental strategies will not only attempt to maximize the wise and efficient utilization of resources but also their equitable distribution." He further "dreams" that one strategy might be to prevent the flow of migrants to town by rural development projects and that anthropologists should be involved in the planning, implementation, and evaluation of rural development programs, including the expansion of agriculture, decentralizing educational facilities, and African governmental support of rural and urban voluntary and common interest associations.

One can voice no objection to such a laudable dream: in fact one would hail attempts to put the dream into practice. Contemporary reality, however, often dictates otherwise. Kerri himself notes the banning of ethnic associations in Nigeria as his sole "note of caution." I should like to add a few others based on my teaching and fieldwork in Zambia.¹

Political considerations of African governments sometimes outweigh "wise and efficient utilization of