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Following Friedl: Forays into African Fieldwork

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Abstract
This article explores Gottfried Lang's influence over my enculturation into the theory, method, and practice of cultural anthropology. It traces this influence through three Tanzanian fieldwork experiences. The paper concludes with a reflection on the importance of cultivating a mindset of serendipity when undertaking any kind of anthropological investigation, and an exhortation that this mindset be recognized as an integral component of anthropology.

Key Words: Gottfried O. Lang, Sukuma, *bafumu*, livestock development, serendipity, Tanzania

This article is both an investigation into and reflection on my experiences becoming an anthropologist, an enculturation into its perspectives and the learning techniques of gathering and analyzing data in field research projects. Substantial portions of this experience were either directly guided by Friedl Lang or bore his lengthy shadow of mentorship. My goal is twofold: to better understand the dynamics of my approach to the anthropological endeavor and to discover (re-discover) the influence Friedl's tutelage had over my professional career. I will focus on my enculturation into anthropology as a graduate student at The Catholic University of America, Friedl being the central figure in guiding me. Then, I will discuss my first fieldwork abroad, in Tanzania, the Sukumaland Livestock Development project, and also take a brief look at its Maasai version. I'll conclude by putting it all together.

THE CATHOLIC UNIVERSITY OF AMERICA

Regina Herzfeld, chair of the Anthropology Department, after learning what dated literature background I had of the field, more embarrassed than I, hustled me into Friedl's introductory course. The course, a highly structural excursion into the basics of cultural anthropology, I learned later, was rather typical of the time. I discovered the department attracted an unusual collection of graduate students. My classmates on the whole were well-seasoned government workers, medical personnel and others, almost all well traveled, sophisticated and articulate. Comments on anthropological issues came easily to them.

Learning anthropology with Friedl moved far beyond the kind of classroom experiences I was used to. Older students in the know deliberately diverted him from his lecture notes, enticing him, I believe willingly, to wander into fascinating anthropological back streets and alleyways. Later, when I was a more seasoned student, Friedl invited me to join one of his interdisciplinary seminars, the foci of which were usually explorations into dauntingly elusive topics, "power and authority," for example. Implicit was the understanding that participants could move into any arena of discussion they chose. Discussions veered madly from the assigned topic. Membership transcended status. I nestled between experts in dead Middle Eastern languages, psychologists, sociologists, historians of the Middle Ages, all bellowing about distinctions, moving the discussion into arenas of knowledge I had little acquaintance with or that by the texts I was reading were sacrosanct, hardly open to scrutiny.

Outside of class Friedl enticed me into his web of mentorship and his family, as he did with many of his students. I think he intuitively associated my naiveté with his own, entering into a new world of discourse when he escaped from the Nazi net to the USA more than 20 years before. In a sense following his path, I ventured unknowingly into participant observation of my future field and its community.

Even headier discourse ended most of Martha's and Friedl's departmental soirees. The keg of beer had to be finished and after the more polite guests had scurried away, Friedl, Michael Kenny, Elliot Liebow, just completing his research of Washington Street Corner men, and diehard members of Elliot's graduate student cohort engaged in an interchange that would make a !Kung community healing ceremony seem like a tea party. Martha served cheese and cigars, while I, departmental Ganymede, poured libations. They had lots to say about their experiences in translating classroom and texts into the real world. While classroom learning provided us with the ethnographic and theoretical bases of the field and most seminars the freedom to challenge the very concepts and theories we were absorbing, Friedl's post party wind-ups in

Martha's kitchen gave us the opportunity to vicariously experience that real down and dirty experience of application.

Washington had a lively anthropological scene, and Friedl made certain his graduate students had full opportunities to participate. Rides to the Smithsonian Institution or other historical settings in his VW bus were *de rigor*. Washington Anthropological Society meetings thrust us into contact with eminent scholars and professionals who seemed perfectly comfortable chatting with uncomfortable tongue-tied graduate students.

The above opportunities presented the informal addendum to the academic priorities of formal lecture and text, the most emphasized academic means for growing an anthropologist. For this inquiry, most salient was Friedl's yearlong "class" in research methods. I put this in quotes because with a few exceptions, his syllabus was again stalled by the student *cognoscenti*, luring him away from his prepared lecture into the arena of applications and adventuresome speculation. These diversions were not escapes from the subject, but rather gave us tyros the opportunity to gain insights into the messy nature of the research process. From participant observation and the possibly deviant nature of one's most accessible informant, to elaborate social surveying and textual content analysis, Friedl's research methods class covered everything that at the time could be covered.

FRIEDL AS A "ROGERIAN"

"That's interesting. Can you tell me a little more about it?" As I recall, this form of dialogue was essentially Friedl's informal means of communication in contradistinction to his lecturing style. His questions were usually accompanied by body language indicating genuine interest in hearing what we had to say. With a slight twist of his head, squint of his eye, and fiddling of his pipe, he was inviting us to expound on an opinion or topic. Later in my graduate career, Friedl characterized his approach as "Rogerian," in homage to Carl Rogers, who was at the forefront of Humanistic Psychology and known for his learner-centered approach to teaching. Friedl may have had contact with Rogers at the University of Chicago in 1945. Roger's seminal work was published in 1951 (Rogers 1951).

Speculation on how Friedl came into contact with Roger's theories of education aside, the manner in which he chose to interact with his graduate students and colleagues involved a stance of open-endedness. His goal was to facilitate learning beginning with an affirmation of the worth of persons, their opinions and make it clear that he was an empathetic listener. Of course, the twofold aim of this approach is one where the teacher learns while gently guiding the student to greater insight, while not necessarily bringing resolution to any issue.

At first, I thought this technique was Socratic, but with Friedl it lacked the kind of not so hidden smugness that seemed to me to lie behind Socrates' methods of getting his interlocutor to see the defects in an argument or opinion. However, another of Friedl's favorite questions, "Have you read...?" could prove a daunting challenge. It was for me, for rarely had I read what he was alluding to. A fellow student, exhausted with this kind of question, finally said, "No! Have you?" To which Friedl responded in surprise that he hadn't. He was just wondering if the reading in question would shed any light on the issue they were discussing.

I note this important characteristic for two reasons. It certainly existed in contradistinction to Friedl's manner of classroom lecturing. Not that he purported to be the expert, but the learning context made the conclusion inevitable. In retrospect I believe Rogers provided Friedl with the theoretical basis for doing what he did best and left him willingly open to being lured from his "text." The second reason I explore in a little more detail at the close of this paper. Suffice it to say that Friedl's pedagogy reinforced the spirit of inquiry, which I have come to learn, is the genius of anthropology. In vernacular, he was constantly encouraging those around him to "think outside the box," although I don't ever remember him using the phase. He was slyly inviting us to engage in healthy adventures in cultural relativism, which meant challenging our conventional concepts and describing what makes us humans do what we do.

During this time some of us had the opportunity to test our wings as researchers in Washington. One of two experiences severely challenged my choice of careers. I wasn't prepared to investigate my own society and I didn't realize that I was already learning to be a participant observer.

BASUKUMA AND *BAFUMU*

Not completely convinced that the exotic can be as much in one's own backyard as a thousand miles away, the prospect of fieldwork overseas grew to be more and more exciting as I moved through an M.A. degree, the university's Ph. D. requirements of two language exams, a first and second minor and into the final stages of my Ph. D. program. Having fallen in love with Anna Marie Czaplicka's *My Siberian Year*, I entertained hopes to do research with the Chukchee or Yukagir (Czaplicka 1920). With the Soviet Union in the midst of the Cold War, Friedl offered me a more accessible opportunity in East Africa. With his encouragement, I obtained a modest fellowship that would support my research for a year. Through the efforts of Regina Herzfeld, Catholic University waived my tuition and academic fees Encumbered with a seemingly immense poundage of research equipment, I set sail for Tanganyika in late summer of 1963..

A few years prior, Friedl and Bishop Blomjous, a Dutch White Father responsible for developing an educational complex at Nyegezi, a few miles outside of Mwanza, the regional capital, had forged an alliance. The Nyegezi Social Research Institute and the Program in Social and Cultural Change in Sukumaland were born (Lang 1962). Sukumaland, the home of the largest tribe in Tanzania, the Basukuma (henceforth called Sukuma), covers approximately 19,000 square miles, extending southwards from Lake Victoria Nyanza like an overloaded hammock. In 1963 its population numbered around 1.5 million. The Sukuma offered interesting potentials for research: originally composed of chiefdoms, yet highly egalitarian, yet possessing a history of traditional cooperative organizations; primarily farmers with a great investment in cattle-keeping, producers of cotton and sisal through enforced cropping by the British, and represented to the world through only a modest scientific literature.

A Catholic University senior graduate student, Charles Noble, and a Dutch researcher, Hans Vande Sande, were the co-directors of the institute, which provided free office space and lodging. Prior to my arrival, a number of European and American students were attracted to the institute. Among these were fellow anthropology students Warren Roth and Margaret Paulus from the University of Cologne. Through his network, Friedl learned of two other Ph. D. candidates who planned on establishing base at the institute, Mary Eaton Read of Stanford and Andrew Maguire of Harvard. Always interested in collaborative endeavors, he invited both to attend a pre-departure colloquium. Aside from long discussions as to what "egalitarian" meant in the Sukuma context, the most important item on the agenda was the fieldwork process. After enduring an entire year of research methodologies, I was surprised to learn that at least one of my new colleagues had never had the benefit of such an academic experience.

I arrived in Sukumaland armed with a number of new items for research as well as a mandate on how to properly carry it out. However, my research methods class notes did not include cameras, watches, cumbersome tape recorders, the prototypes of fieldnote paper and a typewriter, all of which Friedl ineluctably required. I already possessed the latter and was given samples of the penultimate, but was required to buy the rest. Friedl lent me the tape recorder. Save for a few instances, it remained at Nyegezi, being too cumbersome to be packed on the used Honda 16O cc motorcycle I purchased. Accommodations at the institute were reasonably luxurious by Tanzanian standards; a shared house with a private bedroom, water, and an office at the institute.

The Research Project

My original intent was to investigate the Sukuma tradition of *balogi*, loosely translated as witchcraft, sorcery or "poisoning." I asked a question: under conditions of rapid socio-cultural change how would the Sukuma belief in the ability of neighbors to "mystically" harm alter? The topic fit very nicely with the kind of research Friedl and Bishop Blomjous wanted for the Social Research Institute.

I was given free range to err as I wished. I had read enough to know that witchcraft was a subject people in most cultures were loath to talk about and was not the easiest subject for a novice anthropologist to undertake. But I had a strategy, which Friedl considered a reasonable one to test. The scant literature on the Sukuma indicated that those who know most about the subject are those whose training and sensitivity qualify them to reveal *balogi* and assist in redressing their evildoings. These "religious specialists" were called *bafumu*. My strategy was to work intensively with *bafumu* to gather witchcraft lore, case studies and witness divinations. To some extent I was correct, Sukuma religious specialists and curers are experts in revealing *balogi*. In almost all instances they are absolutely necessary, but *bafumu* proved to be extremely reluctant to talk about these evildoers. As one of my informants said, "to know about witches is to be one."

It became quite clear that if I wanted material for a dissertation, I would have to make drastic repairs to my research topic. With Friedl's commiseration, suggestions and eventual approval, *bafumu* became my focus. I was already apprenticed to an engaging *nchembi wa ngoko* (chicken diviner), each day traveling the Sukuma countryside on my motorcycle visiting clients. Each evening I returned to the luxury of the institute, typed out fieldnotes in triplicate, the original for myself, a pink carbon copy for the institute and a third pink copy for Friedl's files in Boulder. Afterward, I would have dinner, share my day with fellow researchers, take a shower (cold) and partake in the occasional party.

However, something was wrong. First, as I reviewed my field notes, Friedl's discussions on the importance of scientific validity and reliability in the collection and analysis of data came to haunt me. How reliable was my information? How could I contribute anything meaningful about *bafumu* on the basis of a year's experience with one or two religious specialists other than to report on how to diagnose a physical, mental or spiritual problem by dissecting a chicken? The answer was obvious, I would have to broaden my investigation. But doing so seemed to run afoul of another reason I was in Tanzania: my rite of passage, the only way I could truly become an anthropologist, or so I thought. Even my original research design didn't take into account my almost unconscious expectation that my first field experience would replicate Laura Bohannan's adventures with the Tiv.

A Word on Fieldnotes and Research Revision

As I began my anthropological enculturation in 1959, I thought a peek at Herskovitz's text, *Cultural Anthropology* might provide insight into discussions of the research process of the time. The following is what I must have absorbed in Friedl's introductory course and was partially reinforced much later in Russell Bernard's *Research Methods in Anthropology* (2006):

> *"Descriptions of actual methods used by anthropologists in the field are rare, though increasing attention is being given to the technical problems of methodology"* (Herskovitz 1955: 369).

But, while describing the ethnological endeavor, Herskovitz dismissed discussion of "points of detail in field method…recording instruments" as being the province of the "specialist" equivalent to "test tubes and microscopes" (p.383), only taking on importance "in terms of the more fundamental considerations that arise out of how the research worker conceives his problem, and his basic approach toward its solution" (p. 383).

Friedl's requirements for his students concerning methods and points of detail differed from those of Herskovitz. A salient example of "points of detail" was Friedl's meticulous rules for fieldnote taking. All names and places in code (GOL for Friedl, CRH for Colby, etc.); recording time of day and atmospheric conditions; noting your physical condition; meticulous entries of observations, actions, conversations, interviews, and reactions, a kind of pre-analysis, but clearly stated as such. All of these notes *cum* recordings had to be done in triplicate. The routine taking of fieldnotes was in no way to be arbitrary, nor their dissemination left to the discretion of the researcher;they were to be public documents. As an example of how procedures have evolved toward Friedl's approach, Bernard's recent text on anthropological research advances a position similar to his (2006: 398).

Code names within the field notes came in quite handy when halfway through our fieldwork, since we were called on the carpet by the Mwanza Regional Commissioner. One of our team members buried his field notes for fear they might be confiscated and possibly be used against his informants. Part of Friedl's insistence on such scrupulous recording of data was related to his vision of the uses to which they would be put—reposing at the institute and available to any scholar who wanted to consult them. Thus, clarity, specificity, and anonymity were crucial.

My apprenticeship centered in and around Mwanza. While waiting for advice from Friedl about the first of my dilemmas and after considering the few months remaining before my funds ran out, I decided to abandon my medicine father. With my assistant, the typewriter, notepaper, two camp beds and personal effects on the back of the Honda, I initiated research into the five administrative districts constituting Sukumaland at the time. My plan was to visit each district tracking down *bafumu*, accepting their hospitality when available and thus, expand my understanding of what I called "the position of *bafumu* in Sukuma society" (Hatfield 1968). In Bernard's view, my efforts were hardly "scientific" (2006: 146). I wasn't even doing "simple sampling." I was armed with an interview schedule I had "tested" in one of the districts and filled with hopes that while gathering survey data on the run I might possibly be able to gain some of the depth of understanding I had achieved in my first apprenticeship, and also have at least a fleeting experience of "being present" to a people, as was Bohannan.

I should note that at the onset of my research I did not fully realize how my persona as inquirer about *bafumu* would dovetail with a common practice in which Sukuma seeking knowledge or curing would apprentice themselves to an *nfumu*. As apprentices they would do exactly as I had done with my first medicine father, *ise buhemba*. They would hang out with their master and in return for their labors the medicine father or mother would provide them with spiritual protection and teach them about medicines and divination. I had become an apprentice without knowing it.

The first stop was to a locally famed practitioner living in Kwimba District. As we bounced across the fallow rows of maize fields, I marveled at the size of Nyumbani's spread and promptly propelled us off the cycle. Thus we made our entrance, limping through the main gate of Nyumbani's homestead. Two discoveries kept me at Nyumbani's residence for the remainder of my stay in Tanzania; I had discovered a prophet and I learned I had a windfall of a fellowship. To be truthful, Nyumbani was not fully a prophet or *nanga*, according to Sukuma tradition, but he was well on his way. How could I not remain to witness and record his evolution? But was I venturing into yet another deviation from my original attempts to study the dynamics of witchcraft? Friedl wrote, informing me that my attempt to salvage my research was acceptable, likely fully aware that the vicissitudes of fieldwork, especially one's first foray into it, probably demanded this kind of openness. Moreover, he had orchestrated a small fellowship for me. A quick calculation revealed that I could remain in Tanzania for at least six more months. I could engage a couple of researchers to continue the survey, be a full-time participant observer*cum* apprentice, thus fulfilling my romantic notions about what a proper rite of passage should be—while witnessing Nyumbani's spiritual evolution. I should note that the survey instrument was a revision of the interview schedule I had tested in Geita District. I expanded it and included detailed instructions to make the research process more accessible to my Sukuma assistants. Bernard has a somewhat different definition for this instrument, labeling the version I learned as "informal interviewing" or "ethnographic interviewing" (2006: 211-212).

Aside from accepting my deep gratitude for his efforts on my behalf, I never learned what Friedl really thought about my multiple changes of a research protocol. The field notes continued to flow into the iInstitute and into his files at The Catholic University. Although there were many bumps and grinds during that last six months, I ended up with satisfactory information about more than 60 *bafumu* from all but one district (Hatfield 1968: 20-27). The result of my research was a Ph.D. dissertation outlining everything one needed to know about how to become an *nfumu* and what happened once a person tried to make *bafumu* a career. As a finale, I ventured into speculations concerning what happens to individuals who gain the kind of privileged knowledge that *bafumu* claim in an egalitarian society. In the process, I learned a lot about witchcraft and prophethood.

LIVESTOCK DEVELOPMENT, SUKUMA STYLE

While my first field experience lasted about one and a half years, the two livestock development projects lasted over eight. My participation in these applied projects began again with Friedl, who initiated a new phase in documenting culture change among the Sukuma (Lang 1971). In 1958 F.A.O. predicted a worldwide animal protein shortage by the early 1970s. In Tanzania, four livestock keeping areas were considered prime for development. The engine for this improvement was the ranching association. Parliament mandated the Livestock Development Project through the Range Act. The three regions of Dodoma, Arusha and Shinyanga were selected.

Although not the team leader, Friedl as "project sociologist" played a seminal role in establishing the Shinyanga initiative. By this time he had accepted a joint position in anthropology and the Institute of Behavioral Science (IBS) at the University of Colorado - Boulder. I had also accepted a position in the anthropology department of the same school. Friedl proposed spending a year initiating the social research/application portion of the project, then a year in Boulder, then back again until the end of the project, an estimated 10 years. I agreed to replace him in the field when he was in the USA. He managed to convince project directors in Dar es Salaam and FAO that it would be appropriate for us to briefly overlap in the field. His FAO Rome contact was Darwin Solomon, an American rural sociologist. Solomon was active in the High Plains Society after he retired from FAO.

Ranching associations were not only a means of organizing local groups, they were also the mechanism for initiating a series of technical "improvements" ranging from water resource management, to seasonal grazing units, to culling and selling of livestock. An important component of the Tanzanian project was to revive a moribund national cattle market coupled with a defunct beef canning industry. The Range Act also mandated a very un-Tanzanian perquisite. Once formally registered, each association would possess its land and its resources in perpetuity.

The role of sociologist on the project was to nurture these fledgling associations. But another task, required for each association registration, was a survey of population and infrastructure. To set the stage so that this work could be accomplished, we inherited the Lang house, possessions and staff, a cat, a bizarre little Fiat van, the Langs' social network and of course, the tape recorder, which was stolen during a meeting in Dodoma. I also inherited Friedl's Tanzanian counterpart, David Masanja, a canny and knowledgeable Sukuma, his Landrover and driver. Later, Reuben ole Kuney, a Maasai who had just completed his university degree in sociology, joined my little team. Friedl had a talent for organizing people and resources that I never equaled. We arrived in Tanzania a few months after he had completed a massive survey focusing on social change in western Shinyanga and was in the process of analyzing the data. Excerpts from an earlier letter to me illuminate the flavor of the survey and the requirements he was seeking in order to analyze the data:

> *Mwadui, Sunday 24 May 1970*
> *"…please ask the programmers (in IBS) to provide an estimate to be sent directly to Rome (FAO). I have about 240 questions… many are scales, some identification. I will need 5 IBM cards per questionnaire."*

He goes on to say that he will need frequency counts, means and variance of all scales as well as standard deviations run on the basis of 4 different administrative divisions, correlations on 20 variables on 17 different categories of people, etc. He closes with:

> *"Finally, I would like to know how much a factor analysis would run for the expected total of 1,200 respondents."*

I quickly realized that this kind of field experience was going to be quite different from my earlier work among the Sukuma despite my own forays into structured questionnaires. The survey was research instrument of choice and its topics were already determined. FAO ultimately did not fork up the money for the kind of data analysis Friedl wanted. I also became part of an international team of expatriates with different agendas and personalities, likes and dislikes, as well as a project manager and staff 500 miles away in the capital. I was no longer independent. Friedl had established his professional credentials and his status as an *mzee* elder, meaning to Tanzanians someone with the wisdom of age and all the respect it bore. I had none of that éclat, although I did have one advantage. I could still resurrect my KiSukuma and both my wife and I had gained proficiency in Swahili, the national language, which was now beginning to infiltrate even the most conservative Sukuma enclaves.

Friedl's survey provided the overall social context. I became involved in local level research, organizational dynamics, water use and rights, as well as the dynamics of livestock ownership and grazing patterns. These forays provided me with the opportunity to learn more about attitudes, community tensions and potential barriers to the kinds of innovations my technical colleagues were seeking.

Successfully sharing these discoveries with my colleagues proved to be a challenge. I soon learned that our technical innovations in range/livestock management practices required considerable tactful assistance to be effectively integrated from the "social expert" on the team. I knew this from my academic studies, especially in Friedl's applied anthropology course, my earlier contact with medical and other experts working with the Sukuma, and my own reading. But I had to establish my expertise with my fellow team members, alas, some of whom preferred to remain innocent of cross-cultural contamination. This pattern continued through the subsequent Maasai Development Project as well, although I also had to deal with some Tanzanian officials and experts who claimed they knew exactly "what these people need."

One example may suffice. Perhaps the greatest challenge to successfully creating a ranching association, was establishing a common herd coupled with yearly "offtake" via sales based on calculations of grazing capability, adhering to the concept of carrying capacity. Not only was the notion of sharing one's animals with everyone else in common antithetical to the Sukuma way of distributing livestock resources, but the idea of culling a herd just because, in their perception, animals might get a little thinner in lean time was anathema. Cattle for the Sukuma are not simply subsistence or economic entities. As any Africanist knows who studied the classic work of E.E. Evans-Pritchard, they have social value. The Sukuma maximize their livestock's wellbeing by "loaning" them out to others. In return the recipients get their products and have a stake in their offspring (Hatfield 1968). I think my breakthrough in team credibility came when a new animal production team member exploded in frustration:

> *"J- C-! They expect these people to do things that no Texan cattleman would agree to."*

Another new area of research arose from the team's internal dynamics. As noted earlier, Friedl trained his students to record everything and everyone, and everyone included his/her observations of and reactions to other researchers, officials, and expatriates; I had dutifully done so while engaged in my Ph.D. research. But these individuals were usually peripheral to what I was doing.

The internal dynamics of our team had extraordinary, mostly negative, impacts on our work. Our initial team leader introduced me to livestock development by lecturing on the evils of ticks and then proceeded to stuff a handful of squiggling engorged ticks into my palm, my livestock project rite of passage. Our team's interactions with our Dar es Salaam project manager were hardly productive, his therapy during his visits upcountry being to harangue us for our lack of productivity. So the inner workings of our team and its various encounters with our project manager became an irresistible subject for my participant observation obsessions. The Sukumaland project was nipped in the bud. I continued on for a half year longer than I was supposed to and watched the project shut down. Friedl did not return, but I did, to the Maasai.

THE MAASAI RANGE PROJECT

The Shinyanga project was the last on which Friedl and I collaborated. A little over a year later I returned to Tanzania to assume the "sociologist" role on the Maasai Range Project in the Arusha Region. Friedl began a project with the Asmat of New Guinea. He was no longer with the Institute of Behavioral Science and the long-range dreams of a Sukumaland project faded. Our mandate was the same as in Shinyanga, but the cultural context and political climate were quite different. The lifetime of the project, funded by USAID, continued for ten years. I was part of the project for the last seven. The American team was much larger than in Sukumaland and included an amazing diversity of technical specialties. Turnover of personnel was astonishing. We had three team leaders and when we were without one for long periods of time I acted as interim. I'm not sure we ever coalesced as a cooperative unit. The phrase, "there is no I in team," comes vividly to mind.

A major shift in national policy occurred shortly before my arrival, inciting an almost unmendable rip in U.S. - Tanzanian relations. The principle of *ujamaa* was now the country's primary development focus and with it, the nationalization of some European farms and other properties. Members of the U.S. Congress condemned Tanzania as a communist state, yet we continued with the then-superseded ranching association mandate.

Later, the country embarked on another major policy, which was the reorganization of populations into permanent settlements, called in Maasailand *operashun mbarnoti*. Doomsday sayers predicted the death of the Maasai project in this maelstrom of conflicting policies. The drawing card of permanent possession of association lands was forgotten, as were ranching associations. My counterparts and I were able to allay project and local government fears by recommending a tweaking of the policy to accommodate Maasai livestock practices. We opined that the Maasai on the whole would be quite willing to resettle and our predictions proved to be correct.

As the sociologist for the project I continued under a mandate similar to Shinyanga, but under much different conditions. I inherited a Friedl-like infrastructure survey that utilized an army of secondary school students. Planning and executing these surveys made the Shinyanga experience seem like kindergarten. I learned some Maasai, but never became fluent. Part of the problem was that I could not hang around a Maasai community for very long periods. A predecessor, taking the romantic participant observation route, had chosen to live with a community and contributed valuable information on Maasai lifeways, but was soundly criticized for his efforts. We had the unachievable requirement that we work in all the areas of Maasailand simultaneously. Thus, my primary responsibility was to conduct infrastructure surveys and when allowed by my technical colleagues, assist in their innovations. Later in my tenure on the project, a visiting American anthropologist, after taking up project time with lengthy consultations, criticized me in his final report for not doing what an anthropologist should be doing.

During this time Friedl and Martha visited Arusha. For a few weeks, the tradition of me following him was reversed. I led Friedl on visits to Maasai colleagues and engaged in a lavish safari through Ngorongoro and the Serengeti. Jaded with work safaris as I was, I was very impressed with being a tourist. We had long talks, not quite replicas of Martha's kitchen with Eliot Liebow and Friedl locked in argument, but I nonetheless found seeing this project through his eyes an unexpected gift.

Ironically at the close of the project, after traveling with the evaluating group for some weeks, the team met with the Arusha regional authorities to review their report. The results were quite negative. Given their instructions to evaluate the project on a series of indicators that had emerged from its very onset more than ten years previously, they were correct. A few of us argued that the project had succeeded in innumerable ways that were ignored in the evaluation. To everyone's surprise, the Tanzanians did not share the lowly opinions of project success that the American evaluators had presented, and instead focused on its successes.

CONCLUSIONS

Noting the immense literature on fieldwork technique and the growth of personal accounts, I am ending this retrospective by adding yet another piece to the already bulging literature. In doing so I am keeping to my goal of reflecting on Friedl Lang's influence, on my own development as a professional, and on re-affirming what I think should be treasured as anthropology's contribution to understanding the human condition.

Friedl as Medicine Father

How could I not have made the connection between my own experience as a student and my first Tanzanian fieldwork? Friedl was as much my *ise buhemba*, medicine father, in D.C. as were my Sukuma *bafumu*, but with one major difference. Our relationship went far beyond a connection via temporary instrumentalities modeling the Sukuma medicine father and the academic mentor ideals. I certainly struck out on my own and often deviated from Friedl's approaches to the anthropological endeavor while maintaining his vision of what this renegade discipline is.

Techniques

In these early experiences in anthropology, I naively concluded that participant observation and structured social surveying were in conflict (cf. Bernard 2006: 384-386). My attempts to traverse Sukumaland in search of *bafumu*, armed with an interview schedule and infrastructure surveys on the two livestock projects, made the kind of participant observation I also thought necessary a challenge. I deliberately chose the type of interview schedule I did because it not only provided me with flexibility, it also gave me a chance to engage in participant observation "on the run." From these early experiences I learned that participant observation, key informant interviewing, and surveying all are essential anthropological techniques, something I should have taken for granted as Friedl's student. Instead, I stubbornly (and I think unconsciously) clung to participant observation, as I narrowly defined it, as superior. It was certainly not easier; in fact, as almost all accounts of this form of fieldwork will attest, it demands an extraordinary investment of self.

Research and Rites of Passage

At the onset of my anthropological career, I did not register the tension that would exist between the requirements of my first fieldwork experience and my understanding of it as a rite of passage. The former involves a host of techniques, some of which I've outlined via my own experience in the three projects described here, resulting in a professionally acceptable product. The latter, though far less measurable, assumes personal transformation, the basis at least being an internalization of what used to be called "cultural relativism." I don't recall any academic discussions in which this aspect of one's first field experience was emphasized. That one's first fieldwork was more than learning how to be a scientist, lay in the interstices of texts, lectures and discussion of theory and that the budding anthropologist who left the ivory tower for the field was not to be the same person who returned.

I don't think my graduate training, including Friedl's tutelage, fully prepared me to understand the dynamics of entering into liminality and how to successfully emerge from it. All I understood was that I had to imbed myself into a community and through the crucible of participation, surface as a new person. However, at the same time I was to bear with me the fruits of a carefully orchestrated research project. Are today's students of anthropology trained to be more savvy than I was about how to balance these often-conflicting goals?

Serendipity

Very drastic changes in our field situations, challenging the direction of research or the successful completion of an assignment confronted my colleagues and I. I jumped from witchcraft to the training of religious specialists to documenting the life history of a prophetic movement in the course of six months. Warren Roth (2011) encountered a similar challenge at the onset of his research. In my case, the first shock arose from discovering the gaping defect in my hypothesis concerning a *bafumu*'s willingness to "communicate the skinny" on witchcraft. Later, two major governmental changes in policy threw monkey wrenches into project activities and eventually affected their evaluations. I was only partially successful in my repairs.

Serendipity, as I am recommending the concept here, is more than the ability to make fortunate discoveries by accident. I suggest it can be a learned mindset, capitalizing on coping successfully with the slings and arrows of capricious field circumstances. The first key lies in possessing a healthy awareness of unforeseen challenges. The second, I argue, resides on a more specific level, in integrating flexibility into one's research designs in order to meet these affronts.

How does one learn such a skill? I believe I began to integrate itsbasics in the outrageously interesting byways that a leading question took us in class and seminar, in those passionate discussions in the aftermath of departmental parties and in Friedl's Rogerian style of student-centered discourse. These occasions modeled the necessity of cultivating an attitude of serendipity, even though the word was never used and we were encouraged to develop ideal models of research with the explicit assumption they would prove workable in the field.

I learned a tremendous amount via coursework, research methods, and reading, but I best learned what field research was like by informally witnessing its incredibly messy nature. This was not just the personal discomforts inherent in that schizophrenic experience of participant observation, but even moreso in attempting to carry out more formally structured research in unpredictable environments (aren't they all?). I hope I have managed to integrate their lessons, which taught that one can triumph over most research challenges.

My suggestion is to recognize the value of "witnessing" thismessiness more formally. Contemporary anthropology has come to welcome "true confessions" of the anthropological experience. The titillation of Bronislaw Malinowski's diary or E.E. Evans-Pritchard's confessions about his life with the Nuer are now taken as a necessary part of honest self-disclosure. Among many recent examples, I could cite Hume and Mulcock's *Anthropologists in the Field* (2004). Such publications provide a more convenient opportunity to witness the nitty-gritty of actual fieldwork, rather than awaiting an invitation to Friedl's kitchen, his seminar, or his Rogerian mentorship.I would like to take this notion a giant step further into pondering the nature of anthropology itself. Cultural anthropology has always been a significant thorn in the side of other purportedly scientific disciplines, claiming preeminence in understanding the human experience. The "thorn" is its nasty habit of challenging, usually western, assumptions about what makes us tick. But, gaining legitimacy in the world of scientific experts brings with it a danger that we will rigidly conform to its rules, which in stereotypic terms requires what IT defines as "rigor and quantification." I like to borrow McKim Marriot's use of "sanscritization" (1955) to describe what happens when a community attempts to raise its status. My point is that the field of anthropology can become so locked in its own patterns of pedagogy, professionalism and requirements for expertise that it loses that open-endedness that defines its character and purpose. Serendipity, the ability to capitalize on unforeseen circumstances, to see the world of change through different lenses, is therefore lost.

I have come to understand that growing an anthropologist involves a lot more than classroom teaching. I learned technique and theory through Friedl's meticulous teaching, but I learned the "art" and "soul" of anthropology in the midst of lively diversions from the text in the classroom and from those ardent, serious, loud, somewhat tipsy, encouraged dialogues I've described. I endured Friedl's gentle Rogerian challenges to my views of how things work and put them all to the test through fieldwork. Perhaps others of us for whom Friedl was a "medicine father" might want to use this reflection as a stimulus to share their own experiences in becoming an anthropologist via this remarkable mentor and human being.

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