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ABSTRACT

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Toward the Development of  
An Ethnographic Model For  
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## ABSTRACT

### Toward the Development of An Ethnographic Model for Program Evaluation

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A problem, endemic to program evaluation, centering around the inability of evaluations to produce meaningful information, is outlined. A general strategy to be utilized in responding to the problem, the ethnographic approach, is presented.

The discussion of an ethnographic evaluation model focuses upon: the role of the participant observer, establishing oneself as a participant observer evaluation, and the handling of sensitive information. The exploratory activities out of which came the initial issues to be investigated are described, along with some observations about the process of delimiting the investigation, and of initiating "personal" relationships. Discussion is interspersed throughout the paper, focusing upon the methods used to collect data, and the line of investigation pursued during the study. Finally, the advantages of the ethnographic approach to evaluation as compared to the experimental approach are discussed.

## TOWARD THE DEVELOPMENT OF AN ETHNOGRAPHIC MODEL FOR PROGRAM EVALUATION

Program evaluation has been enmeshed in a fundamental controversy throughout its relatively short existence as a discipline. The conflict has been the result of the seemingly irreconcilable differences created by its heritage and its present focus of inquiry. The predominant ancestors from which evaluation developed are the experimental research tradition and the psychological measurement movement, both of which have served as the foundation for educational research throughout the twentieth century.

The most popular design for evaluation studies today (for both practitioners and sponsors of evaluation) is a direct descendant of Ronald Fisher's work on the design and conduct of experiments in the late nineteenth and early twentieth century. Fisher pioneered efforts in the design of experiments and the statistical treatment of data for the purpose of drawing inferences as to the causal relationship between variables. The concept of randomization, the techniques of analysis of variance and analysis of covariance, and the use of the null hypothesis were all utilized by Fisher in his watershed experiments in the fields of biology and agriculture. Through the work of Fisher, the practice of observing phenomena in order to determine cause-effect relationships assumed a precise, rigorous, and methodical form. Practitioners in the field of evaluation have, over the years, concentrated their efforts upon refining the Fisher model and using it to assess the impact of educational innovations and other programs designed to ameliorate social problems.

Coupled with this type of design for evaluation studies was the particular technique of collecting data on participants in a program via instruments designed to measure the specific trait(s) or characteristic(s)

that programs were attempting to change. The development of these types of data collection instruments was largely stimulated by the works of James Cattell, Edward Thorndike, and Lewis Terman, during the first part of the twentieth century (Thorndike, 1951, p.5). The emphasis in the educational measurement movement was upon the use of paper and pencil tests, administered to an individual or groups of individuals in order to produce information about such things as intellectual ability, personality, and performance. The flavor of program evaluation methodology which grew out of this educational research tradition reflects very strongly the experimental design and testing influence.

Problems began to arise, however, when researchers were called upon in the 1960's to determine the effectiveness of educational and social reform programs that were being funded primarily by the federal government. The initial attempts to evaluate these programs involved using what were at the time the best methods available; the traditional research design and measurement strategies. This type of research, however, is heavily dependent upon a setting that can be controlled by the researcher; it demands a very precise manipulation of environmental variables; and the program, both internally and in relation to its larger context, must approach a homeostatic state. The California Gold Rush probably more closely reflected these conditions than did the social intervention programs of the 1960's, and understandably so. What was being tried in these programs was unique. The society was diverting huge sums of money into the development of programs designed to cure its many ailments. In reflecting back over a decade since these first attempts, it is easy to conclude that there was a naive optimism prevalent which engendered the belief that not only would these programs have some impact, but they would stabilize after a short period of time and thus enable this impact to be

measured. Unfortunately, this was not the case and as the 60's wore on, the optimism faded.

The fact that evaluation studies carried out in the traditional manner failed to discover any significant program impact stimulated a variety of explanations. The ones most often heard were: the idea was no good right from the start because our lot in life is determined either by how hard we try to help ourselves, or by our ability; the programs were not given enough time and money to work their magic; the programs were poorly managed and finally, they actually did accomplish something, but the evaluators were collecting the wrong kind of data and could not ascertain the true impact of the programs. Irrespective of which of the above explanations has the most validity, the last one had the greatest impact on the practice of evaluation.

Edmond Gordon typified the prevalent attitude when writing in 1970 on the evaluation of compensatory education programs:

"Quite apart from the problems related to the conditions under which programs were initiated and conducted are the problems of evaluative research in general. Here, one often finds a low level of expertise and inadequately developed methods. The best educational research scientists often choose to work with basic problems in areas such as child development, learning, linguistics, rather than with evaluative research. Evaluative and field research have only recently gained in respect and demand among educators and the public. Consequently, high demand has been suddenly created in a field with insufficient expertise. Although many good research scientists were drawn into evaluation, they could not readily transfer their research competence to the new situation. Indeed, given their experience in controlled laboratory settings, the problems of evaluative and field research may have been more difficult for them than for some

less experienced investigators" (1970, p.2).

Robert Rippey, in a book entitled Studies in Transactional Evaluation, is likewise very skeptical about the value of evaluations:

"Most evaluations to date have been useless. Formative evaluations usually come too late often after a program has lost, or retained, its funding. Summative evaluations, despite their intent, have been inconclusive. At the moment, there seems to be no evidence that evaluation, although the law of the land, contributes anything to educational practice other than headaches for the innovators, and depressing articles for journals devoted to evaluation" (1973,p.9).

Egon Guba suggests that the widespread dissatisfaction with evaluation is largely due to methodological inadequacies:

"...many agree that evaluation has not been as fruitful as had been hoped or expected. Useful evaluation information is not often produced; and even when it is, decision-makers and policy formulators sometimes see fit to disregard it. While there are undoubtedly many reasons for this state of affairs, a major reason seems to be the lack of methodology uniquely suited to evaluation's needs" (1978, p.1).

This paper is offered in the belief that program evaluation is presently in a predicament analogous to that of the child who was taught to swim by being thrown in the creek and instructed to "sink or swim." Likewise, we as evaluators are forced to develop our techniques while we simultaneously struggle to keep our heads above water. The dilemma is further compounded by the fact that evaluators are expected to not only make some judgement as to a program's worth, but also to provide information to program managers, and to conduct research. As practitioners in a developing discipline, we are not afforded the luxury of simply practicing our trade. We must distill information from every field work experience

that will contribute to our efforts to create conceptually sound models and viable methods for conducting evaluation studies; the intent of this paper is to make such a contribution.

### Overview of the Model

The study from which the model grew was an evaluation of the Alternate Community Service Program (ACSP), a volunteer work program for juvenile offenders who have committed minor violations of the law. ACSP operates in Charlottesville, Virginia and is envisioned as a model program, responding to a need within the juvenile justice system for alternate forms of disposition that can be made available to officers of the court to use when such time-honored forms of disposition as dismissal, probation, and incarceration are deemed inappropriate.

The model is summarized in Figure 1. The basic approach is "utilization-focused," for it is "...aimed at increasing the likelihood that evaluation input will be substantial, meaningful, and relevant" (Patton, 1978, p.34). It responds to Patton's definition of "utilization" as being "...not something that suddenly and concretely occurs at some one distinct moment in time. Rather, utilization is a diffuse and gradual process of reducing decision-maker uncertainty within an existing social context" (Ibid, p.34). The investigation proceeds from "known to unknown" in an "expansive/intensive" manner. Any inquiry into a specific issue follows a pattern of: collecting information to describe the issue, reflecting on the information, and offering an interpretation about the issue. From the methodological point of view the investigation unfolds through a pattern of intensive interaction between the program and the evaluator. In my study, this interaction was primarily between myself and two key informants. Related to the pattern of personal interaction is the element of continuous feedback of information, at the



end of a cycle of depiction-reflection-interpretation, relative to a given issue. This feedback is typically informal in that it is not issued as a written report, but is shared in conversation. This informal reporting was balanced in the study by the preparation and delivery of two formal documents to the program director. These were the "program description," and the "final report."

Insert Figure 1 About Here

The result of the process of questioning and requestioning people is the accumulation of data that, when complemented by observational data and written records, can be used to construct a descriptive picture of the program that moves beyond mere depiction to include reflection and interpretation. Throughout the course of an investigation, the three - depiction, reflection, and interpretation - must be intertwined. Interpretation begins when one first arrives on the scene; the attempt to accurately depict the setting continues until final departure. The progression is from tentative small scale descriptions which subsume depiction, reflection, and interpretation, to refined descriptions that are more comprehensive and less tentative. Clifford Geertz, in The Interpretation of Cultures, describes the process as follows: "Previously discovered facts are mobilized, previously developed concepts used, previously formulated hypotheses tried out; but the movement is not from already proven theorems to newly proven ones, it is from an awkward fumbling for the most elementary understanding to a supported claim that one has achieved that and surpassed it" (1978, p.25).

In essence, the approach is characterized by a utilitarian perspective and an interactive relationship between evaluator and program. The

investigation proceeds on the basis of a thorough understanding of the program's intent and design, to explore critical issues through an expansive/intensive inquiry and to offer meaningful interpretations to the program staff, sponsors, and clientele.

The discussion below focuses primarily upon the process component of the model, particularly the methods for establishing a role and gaining an understanding of a program, and for expanding and intensifying the inquiry. Because ethnographic research generally uncovers "sensitive" information, the problem of how to handle this information will be examined. Finally, the advantages of ethnographic over experimental research, for program evaluation, are discussed.

#### Establish Role/Gain Thorough Understanding

The role of the participant observer can be likened to the role of a parent attempting to guide a child through life. The development of a child involves an almost continuous redefinition of "reality" on the basis of new experiences and knowledge gained each succeeding day. The child's behavior can be understood as an attempt to respond to, or interact with, reality as he or she understands it at that moment. This interaction causes the child's perception of reality to be altered or refined. A new and tentative understanding of reality is achieved that serves as an intellectual oasis until the child is refreshed and ready to resume his explorations. Ever present, the parent attempts to preserve potent symbols of the younger days while providing insight and direction as the child grows older. At no time does the parent's "reality" become one and the same with the "child's," yet the parent's perspective allows him to "observe" the child as he incessantly processes information derived from new experiences, reshapes "reality," and refines his understanding of it. While "observing," and attempting to preserve elements

of the child's experience, the parent exerts a guiding influence; he cannot live the child's life for him, and in that sense he is "objectified"; he can profoundly influence the child's development, and in that sense he is "involved."

The participant observer as an evaluator can achieve a similar relationship with a social action program. To illustrate, in an alternative school for dropouts which I helped to evaluate, the original proposal for the project emphasized the importance of getting parents of students actively involved in the school. While students were being recruited for the program, it became obvious to the staff that involving parents was not a realistic objective because students were adamantly opposed to it. Many had moved out of their homes because they were being beaten, humiliated, or abused in other ways. Thus, the decision was made to eliminate plans for parental involvement. In a related problem, the same project intended to have teachers provide counseling services to the students. However, a number of students came to teachers for help with problems (mostly family-related) that were so severe in nature, teachers did not feel qualified to respond.

In both of these examples, the project staff was forced to respond to a "reality" that was different from what they had anticipated. The form of the project was altered by the staff as they attempted to respond to this changing "reality." As part of their "involvement" in this program, the evaluation team assisted the staff in coping with a "reality" that changed as a result of new experiences and information, by helping them to develop new strategies for dealing with students' personal problems. From the perspective of "objectivity" or "non-involvement" the evaluators documented the existence of these problems, the extent to which they influenced the program, and the changes that were made by the staff in

responding to them. We had established ourselves as being "insiders" to the degree that we were asked to take part in the staff deliberations relative to these problems, but we retained the "outsider" perspective such that we were able to view the problems as they arose, the staff's response to them, and record this as part of the documentation of the program's development. The participant observer/evaluator is able to collect some data and be of some value because he is perceived as being "like" an *insider*; but he is able to collect other data and reflect on all data differently because he is an *outsider*.

### The Role of Insider/Outsider

Pelto develops the *insider/outsider* role further from the point of view of an anthropologist in the field:

"Some people have written as if the ethnographer gains access to local private information to the degree that he becomes identified as a local - an *insider*. This is, of course, very important - up to a point. Beyond that point, the fieldworker is privy to significant information because he is an *outsider* - someone who is different from every other member of the community. He is different because his core prestige ultimately rests on membership in another society; he is neutral in the local competitive scene...he provides the possibility for social interaction in which the rules are suspended to a certain extent. Friendship and social affiliation can be obtained from him with relatively little social risk, because he is not competing for prestige in the social arena" (Pelto, 1980, p.220).

The nature of the personal relationships that one must form in establishing oneself in the *insider/outsider* role is not always clearly understood by the prospective participant observer. It is naive to assume that the personal relationships the researcher needs to develop with the "locals" will assume their proper form naturally. The researcher should be

fully aware of the type of relationship that needs to be created. On the surface, the researcher must become "friendly" with the people, displaying an interest in what they are doing and listening to them when they talk. However, in order to extract information from people, instead of simply acting as a passive recipient of information, the researcher must attempt to create a relationship that moves beyond "friendly."

#### Relationship Based on Intimate Content

Raymond Gold, in an article entitled "Roles in Sociological Field Observations," describes the type of relationship the participant observer attempts to create, and he bases his explanation on George Simmel's distinction between "intimate form" and "intimate content." He cites the following passage from The Sociology of George Simmel, edited by Kurt Wolff:

"...intimacy is not based on the *content* of the relationship...certain external situations or moods may move us to make very personal statements and confessions, usually reserved for our closest friends only, to relatively strange people. But in such cases we nevertheless feel that this 'intimate' *content* does not yet make the relation an intimate one...That 'intimate' content, although we have perhaps never revealed it before, and thus limit it entirely to this particular relationship, does nevertheless not become the basis of its form, and thus leaves it outside the sphere of intimacy" (Wolff, 1964, p.127).

Simmel further develops his concept of "intimacy" as follows:

"In probably each relation, there is a mixture of ingredients that its participants contribute to it alone and to no other, and of other ingredients that are not characteristic of it exclusively but in the same or similar fashion are shared by its members with other persons as well. The peculiar color of intimacy exists if the ingredients of the first

type, or more briefly, if the "internal" side of the relation is felt to be essential; if its whole affective structure is based on what each of the two participants gives or shows only to the other person and to nobody else" (Ibid, p.126).

To be effective, the participant observer must carefully orchestrate the development of relationships so that they facilitate the exchange of "intimate content," but do not actually achieve a state of "intimate form." The fully articulated role of the participant observer goes far beyond being just a friendly fellow that people enjoy talking to. A great deal of purposive and subtle molding and massaging of personal relationships must take place if the research is to be successfully carried out. Additionally, for the researchers attempting to do an evaluation as an ethnographic study, a unique problem arises relative to the expectations of the staff and sponsors of the program which adds a new dimension to the problem of establishing a role.

#### Providing a Service While Defining the Role

An anthropologist in the field establishes a relationship with the people being studied that allows them to be observed on a daily basis, and to be questioned about what it is they are doing and why they are doing it. The relationship that allows for meaningful inquiries to be made is one in which the anthropologist's presence is at least tolerated, if not openly appreciated. The anthropologist must devote whatever time is necessary to gain the right to be present and the ultimate research objectives are dependent upon the creation of this acceptable role.

The evaluator encounters a similar problem in trying to become a participant observer in a program; he or she must acquire the right to be present as daily patterns of activity unfold. However, this problem takes on an added dimension because the clients of the evaluation expect

the evaluator to perform some type of service, and in most cases they are paying for the service. The anthropologist does not have as the focal point of his study the provision of a service to a community. Thus, the anthropologist can afford to bide his or her time in order to ensure that the proper role is created; whereas the evaluator is not afforded this luxury. An evaluator is supposed to be doing something that is of value to either the program staff, the sponsors, or both. As a result, the evaluator must demonstrate early in the association with a program that a service can be provided to those responsible for the program

The anthropologist has in his or her bag of methodological tricks certain pursuits that can be engaged in while a role is defined and developing relationships are nurtured; the archetype is the delineation of kinship system of the people being studied. Although the study of kinship systems has long been a focal point of research in anthropology for a plethora of reasons, the ones discussed below are the most relevant to the problem of how to conduct research while simultaneously defining a role for oneself in the research setting. The first advantage to investigating kin relations is that it is something most everyone, in any culture, is willing to talk about, even to a semi-stranger. A second aspect of kinship study is that it is a convenient starting point for learning the local language. Finally, as the anthropologist begins to illuminate and understand the web of kin relations and their linguistic referents, an insight is gained into aspects of life in that particular culture, such as economic relationships; division of labor; the exercise of power, authority, and influence; the particularities and peculiarities of behavior associated with affinal and consanguineal relations, and so on. The point is that pursuing questions about kinship enables the anthropologist to use the research activity to establish a foothold in the

community. From this base, he or she can proceed to branch out into other areas of inquiry that were previously stated in the research proposal, or newly discovered in the research setting.

The participant observer/evaluator, likewise, needs a methodological "trick" that will allow data to be gathered while he or she is becoming established in the program. Also, in response to a concern mentioned previously, the program staff needs to be convinced, within a short period of time, that the evaluator is capable of helping them and is a worthwhile person with whom information should be shared. Additionally, the newly arrived evaluator has a "language" or jargon that must be learned in order to be able to converse freely and meaningfully with the program staff. To respond to these concerns the evaluator, like the anthropologist in the field, needs to find a subject that people in the program would be willing to discuss as they become familiar with, and begin to understand, the role being assumed, and his or her value to them in that role.

In the evaluation study that forms the basis of this paper, I responded to the problems of establishing a role and demonstrating my value by using a technique for developing a "program design" that is part of the late Malcolm Provus' Discrepancy Evaluation Model (DEM). I had used this particular technique countless times as a staff member of a project funded by the Bureau of Education for the Handicapped, designed to teach special educators operating teacher training programs how to do their own program evaluations. A program design in the DEM consists of two parts. First, the program in question is defined as consisting of a number of "functional" components. Each of these functional components is explained in terms of: 1) its needed inputs, 2) the interaction that is expected to take place among the inputs; simply put, the "process", and 3) the "outputs" that are to be produced as a result of the process.



In the project mentioned above, a staff member would sit down with participants and assist them, or elicit from them a program design. The idea was to proceed from the general to the specific. One began by trying to get an overview of the program; from this, several major components were defined and each was described in terms of its requisite inputs, its anticipated process of operation, and its expected outcomes. Once each component was described this way at a general level, the next step was to take the major components and further define each in terms of its functional sub-components. These sub-components were then described in terms of their inputs, processes, and outputs. If done properly, the end result was a clear and fairly concise statement regarding how a program was designed to operate and what its objectives were.

The process of eliciting a program design involved getting people to talk about their programs, and my experience suggested that this topic rivaled kinship as an object of enthusiastic conversation. Furthermore, lengthy discussion about a program enables the evaluator to begin familiarizing himself with that particular jargon. During the period spent with the staff formulating the program description, the evaluator develops a comprehensive picture of the program and discovers potentially fruitful avenues to pursue during the study. Finally, and most importantly in the initial role development stage, the program design process culminates in a tangible product, a program design document, that I have found to be useful to program staff: 1) as an aid to program monitoring, 2) as a vehicle for disseminating "model" programs, and 3) in the preparation of proposals.

By engaging the staff in the development of a program design I immediately demonstrated my value and, therefore, was able to legitimate my presence. In a sense, I used the development of a program design as

a way of buying the time needed to establish myself in the program.

### Expanding and Intensifying the Investigation

Once the program design was completed, the investigation vacillated between what could be called "expansive" and "intensive." The prototype of this inquiry is the archaeological excavation.

Once a site has been selected and a grid has been precisely laid out over it, digging can begin. Individual squares are opened up and excavated. Work progresses along two dimensions: individual squares are excavated in depth and new squares are opened up. Discoveries in individual squares may suggest a direction in which new squares should be opened. For example, the discovery in a square of what appears to be a stone foundation might suggest opening the adjacent squares to attempt to uncover more of the foundation. The entire site is opened up on the basis of this rule. The archaeologist encounters may deadends, but as discarded hypotheses become no more than intellectual backfill, new hypotheses are suggested almost as quickly as each succeeding inch of dirt can be removed. The past is reconstructed as the original squares are dug deeper and as additional squares open up greater areas of the site. Expansion and intensification complement one another in gradually revealing evidence of the past life, enabling inferences to be made about it.

In gathering data to evaluate a program one proceeds similarly. The experience gained in developing the program design suggested a direction for an initial investigation. The strategy, as the data collection process unfolds, was to expand the inquiry to gain a fuller understanding of the nature and purpose of the program and the social and political context within which it exists; and to focus upon and probe intensely specific aspects

of the program. Like the archaeological excavation, intensifying the investigation in one area should lead to a broadening of the investigatory horizon; and expanding the scope of the inquiry should create new areas that merit more intense scrutiny. The result would, hopefully, be an interpretation or evaluation that would be meaningful to the clients because it would address itself in a knowledgeable manner to the illumination of problems with the program vis-a-vis the larger context and to problems that may indirectly relate to context, but are more directly the result of internal policies and practices.

### Questioning Strategies

To produce the type of study outlined above, I relied primarily on data gathered through my own observations, questions, and through analysis of the program's written records. Data previously collected, experience gained through my continuous involvement in the program, the incipient interpretations of data, and simple intuition guided my questioning.

Specific questions fell into the categories of "reportorial" and "posing," suggested by Anselm Strauss, et al., in Psychiatric Ideologies and Institutions, (1964). The types of questions are defined below and each is followed by an example from the actual study:

"Reportorial" questions focus upon "...the who, what, where, how, and why of events." Examples of "reportorial" questions that I asked are: "Who is eligible to participate in the program?", "What problem is the program responding to?", "Where did the idea for the program originate?", "How are prospective participants selected?" "Posing" questions are of four types.

*"The challenge or devil's advocate question.* The fieldworker deliberately confronts the respondent with the arguments of opponents. The

idea is to elicit rhetorical assertion and thus to round out the respondent's position by forcing him to respond to challenge." In reviewing the file on participants, I discovered that a number of people were admitted to the program who did not meet the selection criteria. I asked the director, "Why were the criteria enforced in some cases and relaxed in others?" He responded that the "criteria" were meant to serve as "guidelines" for those officers of the court sending potential participants to the program. In some individual cases, the guidelines were not followed because of the unique circumstances of that particular case.

*"The hypothetical question.* The fieldworker poses a number of possible occurrences - for example, What if (someone) did this or that? What would happen (if)...?" In the project, participants perform volunteer community work in lieu of probation or a fine for committing an offense. I asked a staff member, "What would happen if a participant failed to complete the volunteer work?" The answer I received was that the case would be returned to the court and re-opened.

*"Posing the ideal.* There are two variations on this technique. First, the respondent can be asked to describe the ideal situation...Second, the fieldworker...can assert an ideal to see what response is elicited." I posed the following question to a staff member, "Ideally, would every offender whose case satisfied all of the program's selection criteria be referred to the program for an interview and possible acceptance?" The response was affirmative.

*"Offering interpretation or testing propositions on respondents.* It is sometimes very useful to tell respondents about the propositions that one is beginning to pull together...If they disagree, they will usually volunteer information to counter a proposition, which may lead the fieldworker into further unanticipated search. If they agree, the tendency is

to qualify the proposition: it does not quite meet the case. Again, the fieldworker comes away with additional valuable information" (Strauss, 1969, pp.71-72). I discovered, at one point, that a staff member believed that the program's selection process discriminated against blacks, yet in a private meeting with me, the project director (who was not aware of the contrasting belief of the staff member) indicated that he did not believe this to be the case. In a meeting with the two of them, I questioned the extent to which the program might discriminate, and a discussion ensued between the two which forced the director to "respond to the challenge" raised by the contrasting point of view. (He subsequently decided to look more closely at the program's selection process.)

#### Handling Sensitive Information

A natural consequence of conducting ethnographic research is that the researcher becomes a party to information concerning a wide range of topics. A great deal of this information is characterized by "intimate content," as defined previously. The fundamental dilemma for the researcher is how to handle this information once it has been obtained.

As there is no lockstep method for collecting data in ethnographic research, there is, likewise, no pre-fabricated scheme to direct the researcher once he has obtained detailed and sensitive information. Yet, the very fact this problem is not easily resolved makes a discussion of it all the more critical. However before analyzing the problem, I will further define it via an illustration from Understanding an African Kingdom: Bunyoro, by John Beattie. He states:

"Obviously, no responsible anthropologist will betray to the authorities the fact, say, that a neighbor has been distilling illicit liquor, or has successfully evaded a tax obligation. But, it is very much a matter of degree. When I learned, for example, that a respected neighbor, employed in the local hospital, was stealing syringes and giving injections with an unsterilized needle

to local people for a fee, I felt justified in suggesting to the medical authorities that increased vigilance might be desirable (and in attempting to persuade the amateur physician of the importance of asepsis). But I did not feel justified in reporting the matter to the police. The anthropologist who learns a good deal about his neighbors in confidence must respect that confidence, except for overwhelming reasons, though it is of course conceivable that there might be occasion when he should not. No hard and fast rules can be laid down; these are matters of conscience rather than of science" (Beattie, 1965, p.55).

Beattie is absolutely correct in observing that no "hard and fast rules" can be prescribed, and as his own experience reveals, the problem assumes a complexity that goes beyond the question of what information should be revealed, to include problems related to both how it should be revealed; a direct report?, a suggestive hint?, and to whom should it be revealed? The problem is obviously multi-dimensional and many things should be considered if the decisions associated with the handling of potentially sensitive information are to be more than perfunctory.

My own analysis of this problem has led me to identify three factors which I think are important to consider in dealing with a particular bit of information, and in trying to decide what to do with it. I have labeled the three factors: utility, style, and context. They are defined and discussed as follows.

#### Utility

The concept of utility spans a continuum from "definitely useful" to "definitely trivial." Obviously, one does not report everything one knows about a program, and for potentially volatile information one has to reflect upon the implications for making certain knowledge public. Unfortunately, much of what the researcher discovers cannot be labeled definitely useful or definitely trivial; thus, the decision must consider the information as it relates to the program as a whole, and in particular,

its utility relative to the "description" of the program to be derived from the research. Edward Shils preserves the complexity of the problem, yet responds to it profoundly and precisely with the following statement:

"...particular truths must make concessions to privacy, as long as the concessions do not make inroads into the general truths discovered by the inquiry" (1959, p.154).

Although this statement does not make the decision less difficult, it does concisely and accurately define the context within which the decision should be made. Once a decision has been reached that certain information is useful and should be revealed, the second and third factors come into play.

### Style

The style factor can be defined in terms of a continuum that ranges from "direct revelation" to "subtle implication." A simple illustration is Beattie's strategy, in the previous excerpt, of advising the medical authorities to be more careful with their drugs and paraphernalia. He decided the information was useful, but he did not think it ethical to directly reveal what he knew. While studying ACSP I managed to reveal to the project director and to the coordinator that they held contrasting beliefs about a crucial aspect of the program by allowing them to "discover" one another's viewpoint in a meeting, without me having to tell either one that they disagreed. The point is that sometimes one considers information to be useful, yet simply stating it or reporting it straightforwardly may minimize the chances of it actually being used and/or may hurt some person or persons unnecessarily.

### Context

The third factor, context, is also related to how information is

revealed. For context, I have defined a continuum of "informal conversation" to "formal presentation." Like the previous two factors, this one is fairly easy to understand, but difficult to apply. It relates to the fact that the same piece of information can produce radically different results, depending upon whether it appears in a final report, a memo, a meeting, or a private conversation. An example of the context in which information is revealed can be drawn from an experience I had as a member of a four-person team evaluating an alternative school. Approximately half-way through the first (and only) year of this program's existence, the director of the project, who was also the principal of the school, was transferred. His replacement was forced to divide his time between the school and another project he was directing. In addition, his philosophy as to how the school should be run differed drastically from his predecessor; and his style of management was more autocratic, whereas the former director's style was democratic. Despite his sincere intentions, the new director created tremendous controversy and a number of the faculty members developed a personal dislike for him. The evaluation team became party to much volatile information and we were aware that many problems were being created by the staff's attitude toward the new director and his practices. At one point, the leader of our team was asked by some staff members to share their feelings and concerns with the director. The team leader met with the director and revealed to him, very frankly, much of what the staff felt and the problems it was creating. Unfortunately, the program was discontinued for reasons that went beyond the problems between the staff and the director. In our final report, however, we did not discuss the specific problems, conflicts, and accusations that revolved around the appointment of a new director, though we knew they were substantial; instead, we discussed, in general, the consequences that resulted from the change in directors. We felt that the



director needed to be apprised of the situation in detail, but in a final report written after the program's demise, we felt it was sufficient to report only our summary conclusions as they pertained to the change in directors.

I have attempted, in this discussion, to attempt to clarify the problem of the ethical treatment of information by presenting certain concepts that I think are crucial to this issue. I do not think that the decisions concerning what to reveal and how to reveal it are necessarily made any easier by considering the factors mentioned above, but I do know from my own experience that framing the problems in terms of these factors makes for more enlightened and conscientious decisions.

#### Final Statement on Handling Sensitive Information

One final and overriding consideration which has been implicit in my discussion needs to be explicitly stated for the problems discussed above to be adequately treated. The primary consideration under which my previous discussion is subsumed is contained in the following statement by Howard Becker which, although specifically referring to the published report, is generalizable to any uses of information by social scientists: "I assume that the scientist is not engaged in willful and malicious defamation of character, that his published report has some reasonable scientific purpose, and therefore do not consider those cases in which a scientist might attempt, out of malice, ideological or personal, to destroy the reputation of persons or institutions. I further assume that the scientist is subject to no external constraint, other than that imposed by his relationship to those he has studied, which would hinder him from reporting his results fully and freely" (1969, pp.264-265).

### Advantages of the Ethnographic Approach

The ethnographic approach to evaluation holds greater promise for the discipline than does the experimental approach which characterizes much of the evaluation work being done. The reasons for this conclusion have to do primarily with: (1) the focus the experimental model brings to bear upon the research setting, (2) the concept of the "program" created by the experimental model, and (3) the lack of reciprocity inherent to experimental research. The specific nature of each of these observations is discussed below.

The focus in the experimental model is on particular variables and the statistical relationships between them. The meaning of events in this model is taken to be self-evident. The particular variables under investigation are assumed to be discrete, self-contained, essentially static in form; each has singular meaning. In this model, understanding means attempting to discover causal relationships between entities whose meanings and essential form do not change during the course of the observation. This kind of "explanation" can be likened to the assumption as to "the nature of comprehension" discussed in an article entitled "Pre-requisites for Understanding: Implications for the Design of Instructional Strategies and Materials," by Buford Wilson. He states: "Typically understanding what another person says seems to be a smooth effortless, automatic process. People speak and we just know what they mean. Partly as a result of this ease of understanding we are led to false conclusions about the way that words work in allowing us to understand other people and written messages. It seems natural to assume that we learn words and their meanings, and that when we later hear the words their meanings are evoked. Understanding, then, is just being aware of the meanings of words. Words function in language, the story goes, as symbols or representations

of meanings. The word is heard or seen, its meaning is evoked, and understanding occurs" (1978, p.17).

However, he continues:

"Words do not function as referents of objects or meanings, rather words act as cues on the basis of which we construct meaningful interpretations of messages or interactions. For example, the words "How are you?" can mean a wide variety of things depending upon how they are said and the context in which they occur...the meaning of the phrase is inferred on the basis of the listener's knowledge about what is happening between the speaker and himself and how this might relate to other interactions, or special knowledge he has about himself, the speaker, or their relationship. The words do not evoke a given set of meanings. If you play the game of changing contexts, changing relations between people, and changing the demands of the situation, you can create a seemingly endless set of meanings for the phrase 'how are you'" (Ibid, p.17).

Wilson's point is pertinent to evaluation studies. Specific actions, events, "treatments," and "variables" are the "words" in a given program, and attempts to understand them as discrete and denotative are misguided. Webster's Dictionary is not Melville's Moby Dick, and though the primary elements of both are the same, it is absurd to think that the immensely evocative imagery of the latter could be discovered in the former. Likewise, for research focused upon educational and other social action programs to be worthwhile, the facade that is self-evident in the research setting must be penetrated if the true nature of the phenomenon is to be revealed. The perspective assumed by the experimental paradigm is that there no meaning beyond denotation. The ethnographic approach, as presented in this paper, is designed to go beyond the denotative, to penetrate the facade in search of meaning.

Evaluators adhering to the experimental model of research must focus upon discrete events or phenomena and assume each has meaning only in a strictly defined, self-contained, denotative sense. The problem with conceiving of a program (or any research setting) in these terms is that the concept of the program created by the researcher fails to capture the essence of the program. This point can best be understood in light of George Kelly's explanation of the process of human thought and its implications for psychological research. In his book, A Theory of Personality: The Psychology of Personal Constructs, Kelly argues that in attempting to understand the subtleties of thought, one must realize that meaning for an individual is created through constructs that are dichotomous in nature. Traditionally, mental constructs were viewed as being uni-dimensional, and Kelly argues that this results in an inadequate diagnosis because the client's entire construct is not revealed. He states: "Consider a person's use of the construct of *respect vs. contempt*. Under conventional logic one would consider these as two separate concepts. If we wished to understand the person's use of the term "respect," we might seek to find out how broadly he applied the term, how he "generalized the concept." We would want to know what acts he considered to be characterized by "respect" and what acts he did not consider "respectful." Thus, we might be able to discover by the method of varying concomitants just what abstraction among the acts he had been able to make. But when we approach the thinking of a person, say a clinic client, in this way, we miss a great deal. *We miss it because we are tacitly assuming that everything which he does not construe as "respect" is irrelevant.* Yet his use of the construct may be particularly meaningful because of what he excludes rather than because of what he includes" (1963, pp.70-71).

Kelly goes on to explain that in order to understand what "respect" means to a given individual, one must go beyond a strict interpretation and explore what the person "sees as relevantly opposed to respect" (Ibid, p.71). In summary, Kelly states:

"The psychologist who employs the approach of the psychology of personal constructs is led always to look for the contrasting elements of his client's constructs as well as the similar elements. Until he has some notion of the contrast, he does not presume to understand the similarity" (Ibid, p.71).

The experimental model suffers because it is preoccupied with the "similar elements," and it treats "contrasting elements" as irrelevant. The meaning of a construct for an individual is derived from the interplay between the contrasting elements. Meaning for the program is created by the interplay between similar and contrasting elements, within the realm of both the de facto and "extended" programs. The concept of a program, as a strictly defined set of discrete events, can accurately reflect neither the interaction that takes place between the many elements of a program, nor can it accommodate the meaning created by the interplay of elements. The ethnographic evaluator, like the psychologist using the "psychology of personal constructs" applies an expanded definition of what constitutes the relevant elements in the investigation. For example, I studied the arguments for and against programs like ACSP that increase the discretionary options of the court, and briefly presented both sides as part of my evaluation; I do not think the typical experimental study would have explored this particular issue.

For the discipline of evaluation, the preceding discussion has further implications. The evaluator, like the anthropologist, cannot practice his trade without the support of the populace in the research . . .

setting. It is immaterial whether one is studying an alternative school or an aboriginal tribe; if one does not gain the right to be present then very little, if any, data can be collected. Any research conducted in the "field" is dependent upon the cooperation of the people involved. In the case of the traditional research paradigm, "cooperation" frequently involves a willingness to submit to control and manipulation. Ironically, the experimental model spawns evaluation studies whose success is dependent upon the total compliance of the people in the research setting. But, it is seldom that the research which requires complete cooperation proves to be meaningful to anyone other than the researcher and his or her colleagues. The basic problem is that evaluation studies conducted within the experimental paradigm do not, by their nature, enable a reciprocal relationship to be established between the researcher and the people involved in the study.

Marshall Sahlins, in an article entitled "On the Sociology of Primitive Exchange" (1969), discusses the role that reciprocal exchange plays in the "primitive" society. Different types of exchange symbolize different kinds of relationships between people and groups. He specifies three points on a continuum of reciprocal exchange: (1) "*Generalized reciprocity* refers to transactions that are putatively altruistic, transactions on the line of assistance given, and if possible and necessary, assistance returned"; (2) "*Balanced reciprocity* refers to direct exchange. In precise balance, the reciprocation is the customary equivalent of the thing received and is without delay"; (3) "*negative reciprocity* is the attempt to get something for nothing with impunity..." (pp.147-148).

The relationship between the evaluator and the subjects of the evaluation has been characterized by "negative reciprocity." The evaluator is manipulative, demanding, and disruptive through attempts to maintain

the consistency of treatment and conditions. Yet when the study is completed and the evaluator pulls out, a report is written which is usually of little value to the people who must remain with the program. This is because it is generally untimely (discussing events of the past which are like "water over the dam"), and incomprehensible ("reject the null", "significant at the .05 level", etc.). The evaluator indeed gets "something for nothing," and the people who cooperated are left to scratch their heads and wonder as to the meaning of it all.

For evaluation to be meaningful, the relationship between parties to the study must be characterized by a more "balanced reciprocity;" not in the strict sense of immediate reciprocation, but in the sense of an interactive relationship that is symbolized by a continuous exchange of relevant information. The ethnographic approach strives to create a relationship based on "balanced reciprocity." Furthermore, as information is exchanged it is done so in terms of a symbol system that all parties can understand.

In summary, the theme underlying the criticisms pertaining to: focus, concept of program, and lack of reciprocity is that the experimental approach to evaluation is primarily concerned with paying homage to a scientific tradition, the result of which is often a disruptive ritualized process that contributes little to either the advancement of general knowledge or to knowledge about a specific program; whereas evaluation done from the ethnographic perspective attempts to follow a course suggested by the unique circumstances and needs of the particular program being studied, using this lead to produce an interpretive analysis of the program.

### Conclusion

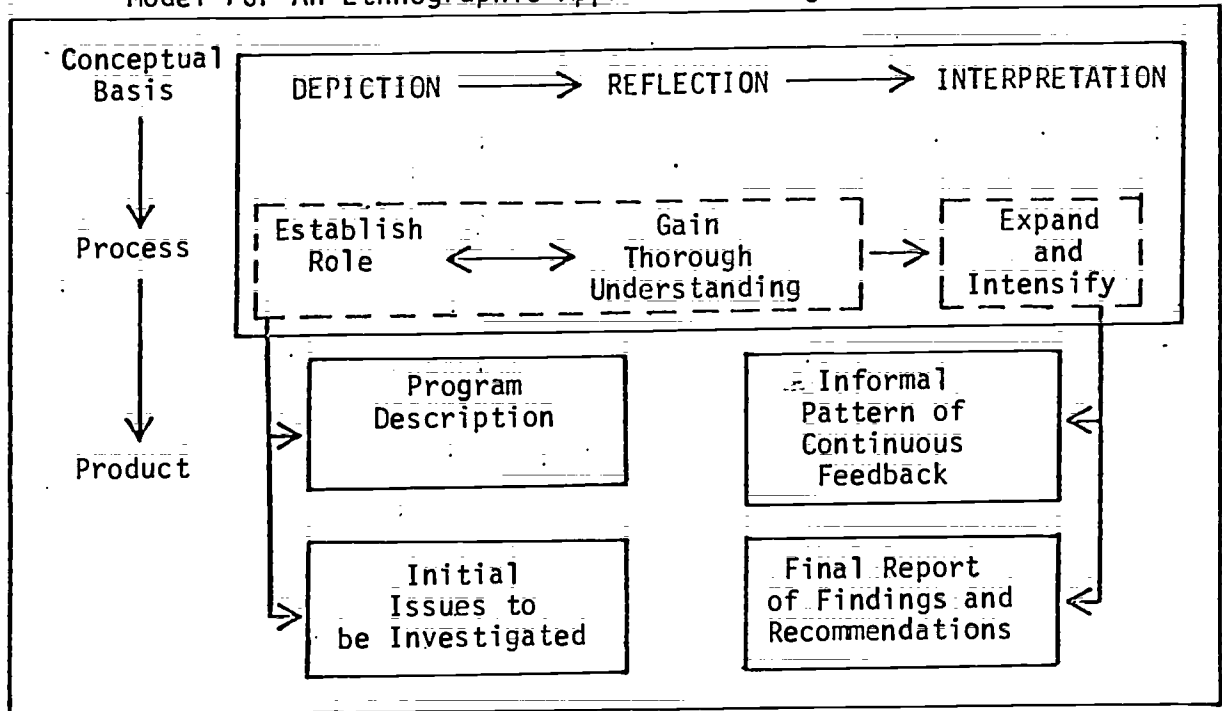
It has been the intent of this paper to present, in incipient form,

a model for conducting program evaluations from an ethnographic perspective. It has been argued that this model has advantages over the experimental model that makes it more relevant to the needs of both the practitioners and clients of evaluation. Finally, it is the belief of this author that the ethnographic approach, as it is refined on the basis of practical application, offers the greatest potential - conceptually and methodologically - for investigating educational and social action programs, improving them, and ascertaining their impact.



FIGURE 1

Model For An Ethnographic Approach To Program Evaluation



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