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Development of an evaluation instrument for the church sponsored Community Change Training Institute (CCTI) resulted in a pre-post test instrument involving measures of meaning, vocabulary, involvement in community action, and personality. Fisher's technique computed the pre-post scores of 108 persons in CCTI groups in Missouri, Ohio, and Hawaii. Data from Missouri and Ohio reveal several generalizations: a more homogeneous staff-participant group in Ohio than in Missouri; the Missouri Institute planned and participated in by Episcopalians, while Ohio Episcopalians only planned; and Ohio participants and staff sharing planning and execution of local change experiences more equally than in Missouri. The California Psychological Inventory revealed that both CCTI's had similar staffs, but Ohio participants differed from their staff; Missouri participants showed lower scores on sense of well being, responsibility, tolerance and intellectual efficiency. The CCTI was effective in changing self perception of abilities, sharpening specialized analytical vocabulary and increasing aspects of the Semantic Differential Concept. It was concluded that the CCTI approach works with a selected homogeneous group. (Appendixes include the project plan and evaluation instruments. They are available from Rev. John Peatling, Div. of Lay Ed. and Devel. 815 2nd Ave. N.Y., N.Y.) (pt)

THE INDIVIDUAL AS A CHANGE AGENT

John H. Peatling

February 7, 1969

January 9, 1969

OUTLINE FOR PAPER ON "THE INDIVIDUAL AS A CHANGE AGENT" (P&R:DCE:NCC)

I. TEXT OF PAPER --

A. History of Community Change Training Evaluation project

B. Design of Evaluation --

1. Problem

2. Solution

C. Execution (& Extension) of Design for Evaluation Study

D. Analysis of Data --

1. Plans for Analysis

2. Problems encountered

E. Provisional Reflections of CCTE (to date)

1. Problems

2. Limitations

3. Generalizations

II. APPENDICES TO PAPER --

A. The Over All Plan for CCTE --

1. Initial Paper (11-4-66)

2. Subsequent Schematic of Procedure (10-12-68)

B. The Instruments Used in CCTE --

1. California Psychological Inventory (CPI)

2. CCTE Instrument --

Outline for Paper on "The Individual as a Change Agent" (P&R:DCE:NCC)

a. Face sheet: Description of Participant

b. Semantic Differential Scales #1 (Concepts, Goals)

c. Semantic Differential Scales #2 (Concepts, Roles)

d. Semantic Differential Scales #3 (Concepts, Abilities)

e. Vocabulary Test

3. Back-home lists, form for....

4. Staff Ratings on Participants

THE INDIVIDUAL AS A CHANGE AGENT

A Paper for the Professors & Research Section
DCE:NCC
Evanston, Ill.
February 7, 1969

By:

John H. Peatling

A. A HISTORY OF THE COMMUNITY CHANGE TRAINING EVALUATION PROJECT

Rather than the reluctant dragon, a punster might entitle this section, draggin the reluctant! What follows will be in the nature of a personal account: this history is too close to me, too much a part of my very immediate past and present, to be recounted in any but anecdotal terms. Therefore, with this explanation, I beg your indulgence for this non-scientific recounting of an attempt at the use of scientific standards.

The proposal to evaluate a Community Change Training Institute was made to me originally, as I remember it, by the Reverend George Reynolds, then the head of our Training Services area, and the Reverend John Steidl, one of his staff of Trainers. The first encounter in my office consisted in their coming in with the proposal that I evaluate the CCTI program for them, which I countered with the proposal that I consult with them on their problem of evaluation. Anyway, it started with my feeling that I'd have a minimal involvement: as I thought of what I already had to do, doing any

more seemed an absolute impossibility.

My initial request was that they share with me the goals of the program that was to be evaluated: any neophyte knows that this is the place to begin! So I received a memo from John Steidl which read, in part, as follows:

> "Attached is a set of Community Change Training Objectives. They are the product of initial work by those of us who are serving on the staff of the Missouri and New England operation who were in town on September 1, 1966...." (John Steidl to John Peatling, September 16, 1966)

I can only remember dimly my distaste for the sheet of "Objectives" that I found attached. It was not that the whole, the gestalt, was so bad; in fact, that was all right. But the language was an intermediate bastard sort of thing...not quite the cloudy and confusing abstraction of the Trainer-type, nor yet quite the precise, operational, behavioral language of the researcher. It was precise enough to suggest that a kind of precision could be attained, but it was booby-trapped with enough abstractions to make it well-nigh useless for evaluative purposes. (Had I been wiser, stronger, more hardhearted, I'd have bowed out or, at the very least, told my two friends to gather PMRs, check to see if they were predominantly favorable, and then make their decision, since that decision would be primarily philosophical any way one could imagine events taking place!) However, I was neither wise, nor strong, nor hardhearted:

I responded to the proffered list by attempting to raise the question of the behaviorally-related statement with my friend Steidl. This really committed me to the project, or so it seems in retrospect. And, after his own bewilderment, anger and frustration had run their presumed course, he rose to my challenge and came back with a still better list of objectives. But I was not satisfied yet! To this second list I responded with reinforcing approval, and suggested that perhaps an evaluation could now take place. (For the curious in the audience, the second set of objectives, somewhat operationalized, can be found in the Appendix B. of this paper: it lies immediately behind the Staff Rating form, which merely translated the detailed objectives into the form of rateable statements about an individual.)

With the answer to the "What Do You Want to Do?" question out of the way, I admitted that a design might well be made, if desired. Of course, it was either desired or tolerated...I'm not sure which. With some enthusiasm for the design task I set about that task. I considered the several statements, identified several levels of evaluation, and began an internal analysis of the goals themselves. Of one thing I was sure: to evaluate something like a Community Change Training Institute one was going to have to measure change in one form or another, and so some form of the Pre-Post Test design would be central. But what change? and by whom? These questions worried me through the fall of 1966. By October of 1966 I had a draft of an instrument, built around the use of Osgood's Semantic

Differential.

The first draft of the CCTI evaluation instrument had the following parts to it:

1. Face Sheet -- one page --
   + Name, address, telephone number, sex, formal education, denomination, function within the denomination.
2. Instructions for the Semantic Differential -- one and one-quarter pages: modeled on the advice of E. L. Thorndike --
   + "Instructions should be in simple language and should always be accompanied by at least three concrete samples of the task." (E. L. T., The Nature, Purposes, and General Methods of Measurements of Educational Products, 1918.)
3. Twelve Semantic Differential Scales, using Concepts that had come out as a result of the internal analysis of the statements of goals. In these scales, nine pairs of polar adjectives were used with the concepts: with a kind of simple faith in my chosen method, I proposed using the same nine with all Concepts, and proposed accepting the factor identifications reported in Kerlinger's Foundations of Behavioral Research, pp. 567, 571.
4. Six Semantic Differential Scales, using Concepts that resulted from a private hunch, which the Trainers planning the CCTI were willing to (provisionally) grant, and which

my memory tells me was somehow related to a monograph from the College Entrance Examination Board entitled, Career Development: Self-concept Theory. (Super, Starishevsky and Matlin, Jordaan, CEEB, 1963). I believe I had been reading in this monograph about this time, and had been intrigued by Super's comment on Stephenson's 1961 work --

+ "The vocational self concept is thus a function of perception of the perceptions (role expectations) of others." (Super, et. al., p. 10)

5. A Vocabulary Test of 14 words that were deemed by the Trainers planning the CCTI to be essential to the analysis of a social system. This test was in the form of a sentence completion test, in which the sentence consisted of a definition, plus the phrase, "...is called:." Each sentence was followed by a blank of equal length, in which the respondent was to write the word defined.

6. Instructions for the Semantic Differential -- one page -- These were essentially similar to those preceding the previous set.

7. Five Semantic Differential Scales, using as Concepts simple restatements of the five major categories of goals for the CCTI. In this case, each such category had been recast into the form of a statement of ability, via the phrase, "My ability to...."

This first draft of the evaluation instrument was pre-tested on some thirteen officers of the Executive Council of the Episcopal Church. Their responses were tabulated, and analyzed to see if any one of the pairs of presumedly polar adjectives was "not working." (In this case, "not working" would have meant collecting a transformed score of 4, on a 1 to 7 scale, from more than 50% of the sample.) While several individuals found that one or more of the pairs were simply not useful to them, in no case did a pair of adjectives act as a neutrality magnet. Therefore, the nine pairs were left as originally set in parts 3, 4 and 6. However, questioning from the pre-test group suggested that all Semantic Differential Scales follow one another, rather than be broken up by the insertion of the Vocabulary Test into the sequence. The Vocabulary Test, however, proved to be very difficult for the Officers of our staff and, upon further reflection, I felt that the sentence completion format was, in reality, a quite difficult task: it involved not only vocabulary, but recall, matching of personal understandings to printed definitions, and involved (for the scorer) the problem of recognizing synonyms. Therefore, I recast the Vocabulary Test into a straightforward recognition task by giving both the definition and the word defined (imbedded in a set of somewhat similar distractors), and asking the respondent to recognize the pairing alone. (Actual usage suggests that

this choice was wise, since it proved to be possible for some persons in all groups to recognize all pairs correctly in the pre-testing and, in the post-testing, for many to do so.)

With the pre-testing of the Semantic Differential Scales, completed, I turned my attention to the larger picture of over all design. My conviction that a Pre-Post Test Design was dictated by the need to measure "change," really set the basic design. But some further analysis was necessary to settle on the actual measure or measures needed to accomplish the desired (and required) evaluation. To that end, I produced the paper in Appendix A that is dated 11-4-66, in which the population was analyzed in terms of Set theory, the possible statistical treatments considered, and a proposal for the actual measures to use was made. However, at this point, we are on the border of either detailed comment on the Design, or a continuation of the history of this project. Since I shall address myself (later) to the matter of Design, I will continue the history of this project.

By this point, as you may well imagine, my resolve to be minimally involved in this evaluation project was beginning to fade: having put so much of myself into the design of both the project and the instruments I began to look for reasons to be involved in the actual Institutes in which the instrument was being used. I am not sure whether it was happy rationalization, or genuine insight, but I found myself beginning to think (and then to talk)

about the need to be on hand when data was collected, so that I could properly interpret the results. I think that the reasoning was correct, but I'm not sure that the dynamics behind the talk weren't somewhat more complex. Anyway, I began to make this point, and the planners of the first Institute agreed: I was to be a member of the Staff of the Institute, although my research role and function on site was to be explicitly recognized.

Thus, I found myself flying to St. Louis, Mo., meeting local Staff and being driven South into the Ozark foothills to Potasi, Mo., where there was a YMCA Camp that was to be the site of the first CCTI to be evaluated. This was in November of 1966, and the camp was, essentially, a summer affair. While it was claimed that it was "winterized," this urban man was never convinced that "winterization" was the best word for it! However, in the few days of Staff meetings prior to the arrival of the participants, the pleasures and the problems of an inter-disciplinary staff were encountered. On the whole, I came away convinced that such a procedure was valuable: beside the Trainers, there was a person with Social Work background, a Community Organizer (formerly with Alinsky) and this curriculum designer, theorist and researcher.

A genuine hesitancy to use all the carefully constructed goals and the instrumentation to measure achievement of them cropped up. The functional existentialism of the Trainer-clergyman on the CCTI staff had to be met. Fortunately, the Staff members from our

Executive Council stuck with the design, and we were able to persuade the others to go along. But the insertion of a researcher into the mix led to a good deal of banter about fixing the data by deliberately misrepresenting their positions: analysis of the results really suggest that nothing of the sort happened, fortunately.

The California Psychological Inventory was administered the first evening by a psychologist under contract to the Executive Council from the Psychology Department of Washington University, Dr. Ray A. Craddick. The instruments had not been designed as speeded tests, so the testing occurred after the evening meal on Sunday, and the respondents had all the time they required. The CPI was administered first, then the CCTI evaluation instrument.

A rereading of my notes of the week of November 12-19, 1966 reminds me that care was taken to randomly assign participants to cabins, just in case we should want to break out and compare subsample made up of cabin groups. As things have worked out, so far, this was an unnecessary precaution: the analysis simply had not moved that far, and this remains but a possibility.

As is often the case, other demands intrude upon even the best of plans. I had to be in Washington, D.C. on Thursday, November 17, 1966, and in New York City on Friday, November 18, 1966. Plans called for my return to Potasi on Friday afternoon, in time for the late evening administration of the Post-Test using the CCTI

evaluation instrument: but delays in New York and in transit prevented that. So, unfortunately, my two day absence was compounded by having to depend on someone else to administer the Post-Test. Moreover, when I got back to Potasi late Friday evening, November 18, I found things in a state of confusion: it was the end of what, apparently, had been two trying days for both staff and participants, and farewell parties were well under way. The plan to do Staff Ratings of participants had been put out of mind, although a few hardy souls were in the staff cabin struggling with ratings. Physical and emotional exhaustion, or too much partying, prevented anything like the planned-for rating procedure. Out of 38 participants, the best that four of the eight staff could do was 16 ratings: as a result, this process was scratched from the evaluation plan then and there.

The Missouri CCTI was followed by two other CCTIs that were instrumented: one in Ohio, which was put on for a community organization in a Cincinnati "ghetto," and a second, which was put on in Hawaii for urban workers related to the churches there. In both instances, problems of time and money prevented my attending these CCTIs. The CPI was, however, administered by APA members, and the participants did take the CCTE instrument in a general Pre-Post Test design. The data from these three CCTIs fills two file drawers in my office and three large record books, in which scores have been posted. But that, too,

is part of the matter of Analysis, and we will get to it shortly.

The preceding anecdotal approach to the history of these CCTIs and the project to evaluate them can, now, come to its end. The CCTIs produced a very large amount of data with this design, and had we been able to gather Staff Ratings, there would have been even more. However, it might be added that the program of CCTIs has simply gone on: the decision to continue was made, as I might well have known it would be, on very general, philosophical considerations. These considerations were at the highest level of abstraction, however, and so had no direct tie to the empirical data: there seemed to be a set of beliefs held by those who decided to do "one more" (and then "another") which could be summarized thusly -- CCTIs are a good thing; Community Change is a good thing; the Church is around to do good things; therefore, the Church will, of necessity and right, continue to hold CCTIs. I shall leave the critique of this sort of logic to others. I would only affirm that it does not seem to me an unfair summary description of a set of operative beliefs, nor would I hesitate to affirm the power of such a set of beliefs.

B. DESIGN OF THE EVALUATION: PROBLEMS & SOLUTIONS

I have already suggested that an evaluation of anything like a Community Change Training Institute seems, inevitably, to involve one in a set of statements about change. For instance, the very act of holding a CCTI implies that those who participate can, in some way, be changed (trained) vis-a-vis the subject of Community Change. Of course, the title of the Institute implies that the focus of the Training is upon Change in some grouping that is (or can be characterized as) a Community. Thus, the theme of change runs in and out of the whole idea of a CCTI like the threads in a bold plaid. To evaluate a change-involved program like CCTIs suggests that the basic problem is to know, at at least two points in time, where people <u>are</u>: where are they before they begin? and where are they when they complete and (as well, perhaps) where are they some time after they complete?

The classic Pre-Post Test design seems both necessary and, in a way, inevitable. In its simplest form this design implies a set of measures "before" and a set of measures "after" some event, which is termed the "experiment." (See Kerlinger, FBR, p. 295, 308, 314-315) In its somewhat more adequate form it implies the existence of several such groups, and in its best and most rigorous form it implies that assignment to the several groups, including both the "experimental" and the "control" group involves both

matching and randomness. However, what we have in a CCTI evaluation, per se, is a situation that seems to preclude random assignment, and makes matching difficult but not impossible. One is left, then, recognizing that the classic design of Pre-Post Test is necessary, and that equally necessary is a compromise with the demands of theoretical experimental rigor.

If one takes the basic Pre-Post Test design as a clear implication of the nature of the evaluative task set by the desire to create a program to evaluate the CCTIs, one is left with the problem of what will constitute the set of "before" and the set of "after" measures. Rather obviously, if one expects to measure change, the two sets must have comparable (or identical) elements. Only if a measure is not to measure change, but some other variable, should it be a member of only one of the sets.

These considerations led to the kind of over all design which you find diagramed in Appendix A and which is dated 11-4-66. There the measures are classified in four ways:

1. Measures of "meaning"
2. Measures of vocabulary
3. Measures of involvement in community action
4. Measures of personality

Of these four classes of measures, #1 and #2 were proposed for use in both Pre- and Post-Test; number #3 was proposed for Pre-Test use and for a second, follow-up post-testing; while

measure #4 was proposed only for pre-test use, since adult personality structure was presumed to be relatively stable. The assumptions lying behind the use of these are that personality measures will not be affected by five days of training for Community Change; that involvement measures will not change until after a back-home period of "use" of CCTI-related learnings; but that measures of both "meaning" and vocabulary may change as the result of five days of training, since these measures are directly relateable to goals of the CCTI itself.

The problem of deciding upon what measures to use is, of course, a critical problem for any research. This is always true of evaluation studies. So let me turn to that problem and share with you the reasons for making the choices that were made, as best I can.

The idea of measuring change in "meaning" relates directly to the assumption that some dynamic, often called motivation, is related to observable action. The theoretical presumption is that, in truth, there is not really such a thing as unmotivated behavior: the motive may be unknown to either the act-er or to the observer, but the presumption is that action, per se, argues for the existence of some dynamic, power, force, or urge that carries intention into action. Since a CCTI is, by intent, directed toward changed action, it seems clear that a CCTI is also, implicitly, directed toward change in motivation, or whatever one

chooses to call the dynamic-that-leads-to-action. Moreover, it also seems theoretically tenable to hold that "meaning" is a most powerful dynamic, and that one tends to act in ways congruent with the "meaning" one understands. Thus, a consideration of this kind would seem to lead toward a search for measures of "meaning." And any such search is almost bound to encounter the work of Osgood, Suci and Tannenbaum, which they have published in a book called The Measurement of Meaning. (U. Ill. Press, 1957)

The Osgood methodology is called the Semantic Differential, and makes use of the idea of securing the record of a series of free associations to continua anchored by pairs of polar (opposing) adjectives, which record is, in turn, factor analyzed. Repeated use of this methodology led Osgood, Suci and Tannenbaum to report a relatively long list of such adjectives, giving what had appeared as apparent general factor identification in their studies for these pairs. My own interest in the Semantic Differential had been of some standing by the time the problem of this evaluative design presented itself, and my own experience had convinced me that it was a sensitive device for measuring "meaning." Therefore, after the internal analysis of the declared goals of the CCTIs had lead to the identification of a set of concepts that appeared to underlie the peculiar statements of goals, I concluded that the Semantic Differential would be a useful, relatively

easily constructed measurement tool for "meaning."

Now, as Osgood, et. al., are careful to state, the "meaning" that is measured is not at the surface level of the ability to state definitions, but is at a presumedly deeper level of "meaning," which repeatedly had shown itself to factor into three dimensions called: the evaluative, the sense of potency, and the sense of activity. This suggested that, through the use of the Semantic Differential, one would come close to securing a measure of what theory termed motivation, the dynamic resulting from the perception of meaning. If action were to change, the argument would seem to go, then the "meaning" should change. So if the measures of "meaning" should change in statistically significant ways, then the presumption of a potential for change of action should be viable. These considerations led me to the use of the Semantic Differential as a measurement tool in the CCTI evaluation program.

One of the goals of the CCTIs spoke of changing the participant's ability to analyze and, so, to understand another's analysis of a "social system." Careful consideration of this goal convinced me that the ability to analyze was at least related to the ability to understand a specialized vocabulary. In this particular instance, the specialized vocabulary was that of the Trainers and those urban sociologists they chose to use as their authorities in the matter of analyzing a "social system." Therefore, it seemed that it would be useful to check on the change in

the participant's ability to understand a set of terms that were considered key terms in this analytic vocabulary. Work with the Trainers involved in the planning of the first of the CCTIs to be evaluated led to the identification of fourteen such key terms. These key terms were:

1. Boundary maintenance
2. Communication patterns
3. Conflict
4. Controversy
5. Decision-making
6. Goals
7. Linkages
8. Norms
9. Power
10. Roles
11. Sanctions
12. Status
13. Self-interest
14. Values

As was mentioned earlier, a pre-test of a vocabulary test designed as a sentence completion task led so readily into the problem of functional synonyms that the original format was changed. Instead of asking for a sentence completion, the respondent was presented with the definition, the words that match the definition, and a set of distractors. The set of distractors was twelve in number, and included the following:

1. Assumptions
2. Competition
3. Decision
4. Facilitators
5. Evaluation
6. Distributive behavior
7. Facilities
8. Initiating set
9. Institutionalization
10. Legitimation
11. Problem-solving
12. Resistance
13. Socialization

The result was a two-page vocabulary test, with seven definitions and answers, plus six distractors, on each page. On such a test the probability of simply guessing correctly should range between one in thirteen (0.08) and one in seven (0.14), which I deemed an acceptable level.

From a task as complex as sentence completion the vocabulary test was changed to the somewhat simpler task of recognizing the correct pairing of definition and word. Thus, the problem of scorers recognizing functional, albeit individually peculiar, synonyms was avoided.

One result of the initial pre-test of the proposed CCTI evaluation instrument was the identification of the desire on the part of the Trainers planning the CCTIs to know something about the prior involvement of participants in community action. To that end, the Face Sheet data was expanded by four questions, which necessitated an additional page. The four questions added to the Face Sheet data were:

1. Membership in Activist Organizations -- a check list;
2. Participation in such organizations -- a check list;
3. Participation in Direct Community Action -- a check list;
4. Responsibility for Planning and Executing Local Change -- an open-ended response was used here: each participant was asked to write a single paragraph, which was then to be coded for reference to either Planning or Execution responsibilities.

With the addition of this data one would have a clue to the degree of actual involvement of the participants (both as individuals and as a group) in the over all process of Community Change. This new data was conceived to be part of an eventual correlational analysis, because it seemed likely that there would be some predictors in this kind of data. Also, when used in a post-post-test, these questions might suggest whether participants tended to be more involved in either activist groups or in the planning and execution of local change after taking part in a CCTI than they were before.

C. EXECUTION AND EXTENSION OF THE DESIGN FOR AN EVALUATION STUDY

The original plan of evaluation was relatively simple and straightforward, as one can see in the initial design paper. (Appendix A, item dated 11-4-66) The original proposal envisioned using two CCTIs, one in Missouri and one in Massachusetts, plus a control group to be picked from one of the Phase I (Sensitivity Training) Institutes sponsored by the Department of Christian Education of the Episcopal Church.

The two experimental groups would have given a kind of East-West spread to the over all participant population, while the use of a Phase I as a control would have given a kind of placebo for the simple effect of five days together upon individuals and groups. The original time schedule, too, would have concentrated the total data gathering into very short period of three or four months. In the original plan, it would also have been possible to hypothesize that the participants, essentially, came from the same general population of clergy and lay full-time church workers.

This relatively simple design seemed all right to the Trainers planning the Missouri CCTI, and to me, and so the evaluative project was launched. Unfortunately, several events occurred after the conclusion of the Missouri CCTI that made the design more complicated. First, the Massachusetts CCTI had to be cancelled because of insufficient registration: the dynamics behind this, apparently, were complex and, at best, can be said to represent a

kind of "evaluation" of the whole project of CCTIs. Then, the Trainers' values turned out to conflict with the demands of research: they found themselves unable to secure assent from Phase I staffs to involve <u>their</u> Phase I in the research. The reasons for this refusal to allow the planned research use of a Phase I, at this distance, appear to have resulted from both the Trainers' values (e.g., anti-manipulation and pro-spontaneity, anti-testing and pro-observation, anti-lawful regularity and pro-uniqueness) and from a simple unwillingness to meet the suggested standard that Staff take all tests they ask the participants to take, including the CPI. Thus, the control group faded away. There was some talk about asking NTL for permission to use one of their summer laboratory sessions, but this never came to anything.

In addition, the Trainers interested in the CCTI program got involved in a two week-end format of training for a group of employees of a community organization from the black "ghetto" of Cincinnati, Ohio. There was also a request from Hawaii for a CCTI. That these two additions could be accommodated to the over all design shows the design was generalizable. It was possible to place these two new CCTIs into the design, decide to use the three groups as controls against one another, at least until a genuine "control" group could be found, and to move ahead with the data gathering. (As I reflect on the experience, I know that

I undervalued the importance of time, for we moved into a series of extensions which both put research-based decisions into jeopardy and seriously impaired my ability to work upon the data -- as time passed other things kept happening, and new priorities got set, and although the data accumulated I was able to give it only sporadic attention.

In the Ohio CCTI we had serious fall-off in the number of persons who took both the pre- and the post-tests. My recollection is that we lost almost half of the potential number, partly because administration of the tests and the needs of research were represented by Trainer-planners and not by a researcher, and partly because some people came to one of the two weekends but not to both. However, we did secure some 20 sets of complete data from this group of participants, plus full data from the staff.

At the Hawaii CCTI we had good cooperation from the participants, and we secured complete data from forty-two persons. However, we encountered an absolute refusal of the Staff to participate in taking the tests, so no data on this Staff exists. The participants, however, represent both clergy and laity, Islanders and Mainlanders, Caucasians, Orientals and Polynesians.

One result of the necessity to change the locus of the CCTIs was to make me aware of the strength of the initial design... strength in the sense of robustness, imperviousness to change, and generality. The changes, however, did vitiate the initial

assumption that all participants would come from the same general population. What we now had was the introduction of a presumedly radical socio-economic difference as a variable in any cross-Institute comparisons, and the unknown factor of slightly differing treatments under the heading of the experimental treatment (two five-day Institutes and one two weekend Institute). While it is not impossible to take account of these variations, it does complicate what was already a relatively complex analytical problem. This may be one reason why I am, today, reporting on work in progress, rather than on work completed!

To back away from the complicating factors, however, and to attempt some generalizations, will be useful, I think. The experience suggests that care spent in planning evaluative research allows one to adjust to the perhaps inevitable problems of drop-out, refusal to cooperate, and addition. Moreover, the Pre-Post design evidenced a kind of robustness that would seem to commend it to anyone interested in project evaluation. However, it must never be forgotten that the interest of both the planners and the carriers out of any project to be evaluated is absolutely essential: what is to be attempted <u>is</u> complex, time-consuming, and will suffer if it gets less than top priority attention.

D. THE ANALYSIS OF THE DATA: PLANS AND PROBLEMS

Kerlinger begins a chapter on the design of research with the following two sentences:

> + Design is data discipline. The implicit purpose of all research design is to impose controlled restrictions on observations of natural phenomena. (FBR, p. 301)

The plans and problems to be addressed in this section refer to the problems anticipated and encountered in disciplining the data obtained. As has been already indicated, the "control" and the "restriction" of observations were essentially accomplished through the use of the CCTE instrument and the CPI.

The two figures in the initial design paper, dated 11-9-66 and entitled Figure #1 and Figure #2, suggest that the process of disciplining the obtained data are of two types. One type is simply descriptive, and involves a look at the participants through the window of the Face Sheet data. The other type is essentially statistical (inferential) and involves use of the model of Figure #2. In this second type of data discipline the data is compared across time or groups for signs of change or difference.

The basic statistical technique chosen was Fisher's t, which allows one to infer the probability of two sets of scores (represented by their means) coming from (or representing) the same population. Since the scores are matched, in the sense of being pre- and post-test scores for the same individual, a rejection of the

Null Hypothesis here would argue for the existence of some kind of change. The use of t with the Semantic Differential data--either in its three factor form or as a total score-- would allow one to test the Null Hypothesis of no change, Concept by Concept, or in terms of total vocabulary scores.

The same model (Figure #2) can also be used for a correlational analysis that puts either CPI scores or coded Face Sheet data over against scores on the Semantic Differential Scales (Concept by Concept); of course, one could also use pre-post change scores on these same Scales. Such a correlational analysis would lead one to the threshold of factor analysis, which might well be the way to most systematically explore the "effect" of a CCTI upon persons, as represented by the available data.

This has been a relatively quick statement of the plans for the analysis of the data. These plans were given an internal priority order, however, and the search for changes of statistical significance (.05 level) was given the first priority. While Fisher's t is not a particularly difficult statistic to calculate, the sheer amount of data produced has meant that, quite literally, this is all that has been done to date, and that only on portions of the data. The problem of the data analysis, then, has loomed increasingly large as time has gone by, and some reflections on that problem will not, perhaps, be amiss here.

The design for this evaluative project is, admittedly, an

attempt to responsibly answer the question of what kind of a difference CCTIs make. From the beginning, it was recognized that it would be valuable to have predictors (of known probabilities) both as diagnostic tools and as either screening or intra-Institute clustering devices. Therefore, the design called for the gathering of data in the areas that might, theoretically, be expected to be somehow related to the changes occurring because of, or in conjunction with, the CCTI experience.

The result, however, has been the accumulation of a vast amount of data. For example, for each individual in each of the three CCTIs there exists, at the first level of raw data, 191 scores. Each of these scores must be coded (in the present format this has to be done by hand) using a key:

1. The 12 S.D. Scales based on the Goals and the 6 S.D. Scales based on vocational/self concepts each use 9 pairs of adjectives, and each pair must be coded onto a 1 to 7 scale.
2. The 5 S.D. Scales based on statements of Ability to accomplish the stated goals of the CCTI each use 3 pairs of adjectives, each of which must also be coded onto a 1 to 7 scale.
3. The 14 items on the Vocabulary test must be corrected and, then, coded onto a 0 to 1 scale (Incorrect or Correct).

The result is that 162 codings must take place with the first

set of 16 S.D. Scales, and 14 codings must take place with the vocabulary test, while 15 codings must take place with the set of 5 S.D. Scales regarding perceived abilities: all together, 191 codings for each individual. But this is merely the first level of raw scores.

In addition, each S. D. Scale having 9 pairs of adjectives has to have three triads summed (according to Osgood's factor identification) and a factor score entered: thus, in addition, 54 sums must be computed and entered. Simple addition suggests that for each individual 244 codings are necessary for the first and second levels of raw data. In addition, to make data manipulation easier, this second level of raw data was entered (posted) into Record Books. This posting necessitated posting 83 scores per person, in addition to the results of two pages of Face Sheet data, and raw and standard scores for 18 scales of the CPI: a total of some 130 postings, per individual. Since we have in our data records for 94 participants in three CCTIs and records (full or partial) for 14 Staff from two of the three CCTIs, this meant posting 130 scores for 108 persons, or some 14,040 separate postings.

When these 14,040 postings are completed, however, one merely has the second level raw data in a form that makes its manipulation relatively easy. To figure Fisher's t to test for significant change involves one in going into this body of data and repeatedly checking the pre- vs. post-columns. (In fact, since there are two

administrations, the 14,040 represents the posting only of pre-test information. To add post-test data means adding some 8,964 postings. Thus, for a total posting of pre-test and post-test scores we have a task that involves some 23,004 postings. If we figure five working days per week for a 52 week year, and recognize that two years have passed since the first CCTI of this project, we can account for 520 working days. 520 divided into 23,004 gives one just slightly over 44 postings per day as an average. While this is, admittedly, a somewhat meaningless figure, it does witness to the enormity of the task that has been before us just to get the data into a second level raw form where it could be manipulated, disciplined, with some ease. To make this kind of a comparison across all 108 persons involves one in punching into a desk-top computer (Olivetti Programa 101) two scores per person (216 scores) on 24 scales (23 S.D.s + 1 Vocab.), or some 5,184 operations.

The figures above represent the extent of attempting to merely discover change across the period of the CCTIs for the 108 persons for whom we have data. Perhaps it is not too surprising, then, when I report to you that we are still in process. However, if this seems like an overly long time to some of you, may I point out several things:

1. The entire task, to date, has involved primarily one person, me;

2. At the most, it has had the short-term attention of three people (myself + two secretaries);
3. The entire venture has become a somewhat academic matter, since the essential decision vis-a-vis the continuation of the CCTI program was made far in advance of any reasonable delivery date on the research report;
4. Electronic data processing was not designed into the instrument or the analysis, although it is now evident that this is the only practical way to deal with the mass of available data.

When these four items are considered, perhaps, it does not seem too surprising that the analysis of the CCTI data is still incomplete. The fact that it is still being worked on, however slowly, implies that the data is of value: to my mind, this data is of very real importance for a range of research questions well beyond the initial evaluative question of the "usefulness" of the particular program.

E. PROVISIONAL REFLECTIONS ON CCTI (TO DATE)

After the preceding history of the attempt to apply scientific processes to the evaluation of the program of CCTIs, I would like to get to what, for some of us, is the "meat" of such a report, the reflections of the researcher upon the meaning of the data. After all, sheer data has a limited appeal, even to the most dedicated researcher. It is the process of reflecting upon the data -- interpreting, if you will -- that, for most of us, constitutes the vital, challenging aspect of knowledge production via research. I will, then, turn to those reflections. But first, I must say something about the problems and the limitations of this research. Only in that way can I retain both my honesty and integrity as a person, and as a researcher. So I ask you to bear with me.

1. Problems in the CCTI Data & Research (to date) --

As I think over the past two years of sporadic work upon the CCTI data I think that the problems with the project fall into three inter-related categories: data, priorities, and expectations. When I think somewhat further upon these three I realize that each is a complex, rather than a simple, unitary problem. And when I cast these categories into the form of a mattrix, so that the inter-relationships may become evident, I am somehow amazed that anything has been done (on the one hand) and, on the other hand, surprised only at my own naivete in thinking things

could have been different than they have been.

For instance, the problem Data per se is complex because there is both the problem of the sheer amount of data -- enough to pack two filing cabinet drawers with just the raw data -- and the problem of the complexity of the data gathered on each individual. These two problems, obviously, are related, but when one looks at that relationship one sees the problem of data in a way that gives flesh to some larger problems of research in the behavioral sciences. The simple amount of data for the 108 persons in the CCTI sample data is, in itself, a problem: on the one hand, even the two pages of Face Sheet data would give anyone a very considerable job of organization and analysis, and on the other hand, I realize that an N as small as 108 does not allow one to generalize too much beyond this population, or an exactly equivalent population. Thus, the nature of the data gathered in behavioral science research tends to push such research toward small sample projects, even though such small sample research is almost fatally limited by virtue of its small Ns. This, perhaps, is where the inter-relations of amount and complexity comes into focus. For there are, as mentioned earlier, not merely two pages of Face Sheet data but material from Scale after Scale after Scale for each one of those 108 persons. Yet, to do the responsible evaluation that was originally intended, such multiple measures seem to be quite unavoidable. Especially since there was no solid body of research to

rely upon for predictors, no standard measuring devices to turn to, and no machine scored tests to do the job desired by the program planners.

Inevitably, this kind of tailor-made evaluative research partakes of the exploratory, as well as of the experimental modes. Therein lies much of the excitement, the fascination that drew me into a continuing involvement in this project. And, as well, therein lies much of the problem. The need for a large N tends to stand there in judgment, pulling toward an ever larger number, for the sake of hoped for generalizability. The need for multiple measures also exists, for one travels in only partially charted areas of human behavior, and every measure has the potentiality of being the one that will unlock the gate to a pathway toward understanding.

I make no apology for either the amount or the complexity of the data gathered: I am still convinced that it was all necessary, given the state of our knowledge when the project was initiated. But the sheer mass of what we produced has created a problem for the project, and for this researcher, that can perhaps stand as a warning. To do what we know to be necessary is to engage in a process of unknown complexity, and Humility and Caution are better companions than Optimism in planning such a venture. What we want to know, what we feel we need to know, too rarely exists "on the shelf" for a rapid purchase: if we do, in fact, need to know this information, then we must be prepared to await the discovery to

finance the exploration, to bet on the rich return, much as men did in the 16th and 17th centuries. Instant information is, unfortunately, rarely available in the fields of our interest, regardless of what the school of the prophets of the new, post-linear age may tell us.

In much the same way, the problem of priorities is more complex than a simple decision that "this is important!" The priority problem seems, in my experience, to break down into three priorities: priority of time, priority of personnel, and priority of budget. Now, time, people and money can, at a certain high level of abstraction, be regarded as inter-changeable units. I would not deny that insight, at that level of abstraction. But I would report what seems to be empirical: this inter-relation of time, people, and money is not always self-evident in the process of life within a research-sponsoring institution. All too often there exists a disparity between the sense of the "importance" of a piece of research, on the part of those planning or carrying out the research, and those who make the time, people, money decisions for the sponsoring institution. This disparity need not lead one into invidious comparisons of personalities: to a considerable degree, during the time of this project, I have operated on both sides of the disparity -- it has found a lodging within me. But the disparity has, and does exist.

The problem of time priority vis-a-vis this project has been expressed by a rather consistent desire for results right away. Now, as I suggested a moment ago, instant information simply is not available in most behavioral research: electronic data processing and

computer analysis of processed data can speed up the time required between the gathering and the interpretation of data, but even this cannot yet give us near simultaneous answers to our posed questions.

The problem of personnel priority vis-a-vis this project has been, basically, expressed in the consistent inability of the sponsoring institution to devote more than one research-competent person part time and a maximum of two secretaries, for a short time. The personnel problem is, of course, complicated by the fact that the sponsoring institution is involved in many projects, and the one person available has had to cycle his attention through a variety of projects, somewhat like a time-sharing computer: unfortunately, since I am the person who has engaged in this kind of cycling, he is not as efficient as the computer in moving total attention from project to project. If there is a lesson here, perhaps it is that behavioral research requires the full (or major) attention of personnel on a research team for extended periods of time, rather than the momentary attention of personnel to a variety of projects. Or one may recast this reflection and say that there is, indeed, a place for the research administrator, but it is not at the head of a research project; when other projects also claim portions of his time, the research administrator makes a poor project director. In some ways, both industry and the

academic world may have learned this lesson better than those of us who work in church institutions that try to do recognizably necessary behavioral research on the church's identified problems, concerns, queries and areas of simple ignorance.

The problem of budget priority vis-a-vis this project finds its expression, I think, back at the initial decision to seek the requisite information in what seemed like a "cheap" way, through the use of existing staff, operating on the fringes of their available time. Of course, the budget problem also influenced the matter of the number of personnel that could be devoted to the data discipline. And the budget priority, also, undoubtedly affected the institution's unwillingness to devote really massive blocs of time to the project. (I am willing to make this judgment because I have been, and am, a part of that institution.)

The priority problems of time, money and people interact in a variety of ways, not only within the overall problem of priorities, but also upon the problems of data (amount and complexity) and expectations. Given the priority decisions, the ability to process either the amount of data, or to deal with its inherent complexity, could almost have been forecast.

Finally, the problem of <u>expectations</u>. It, too, is not simple; one can always complicate this problem by asking, "Whose expectations are you talking about?" Of course, there were my original expectations that I would be minimally involved: these surely led me to (initially) play a reluctant role in the project. The ef-

fect of that initial reluctance on the whole is something that someone with greater knowledge and objectivity than I must assess. As well, there was the expectation of the Trainer-planners of the CCTIs, and the expectations of those supervisors and decision-makers in the sponsoring institution that allowed the project to be initiated. While it may be unfair to these persons, I think their expectation was that good, useful information could be rapidly achieved by a relatively minor investment of time, money and people. And, since the belief in the appropriateness of the CCTIs already existed, it probably seemed like a "good thing" to have that belief upheld by such a "careful" process of evaluation. I do not want to throw stones, for I, too, was involved in this expectation; but it would be less than honest not to recognize that it lurked in the back alleyways of many of our minds. What I report to you, about this problem, is the not too surprising fact that this expectation has been almost unmet: the information has not been forthcoming with the expected speed, and the results (as they have begun to emerge) tend to suggest that the data may give a considerably more critical look at the CCTIs than any of us expected. Of course, this hunch is the stuff out of which we build the foundations of rationalizations about the "true" (i.e., hidden, non-obvious) meaning of the data, forgetting our earlier confidence in the design, procedure, and goals.

2. <u>Limitations Upon the CCTI Data and Research (to date)</u> --

Several limitations have been alluded to throughout this paper, and now is the time to look at them. This research cannot fly the banner of "perfect" research: what I have is too fallible, too human, too limited for that! However important this research project may be; however important this body of data may be; it does not yet answer all questions about the CCTIs, or about Change in a Community, or about churchmen as Change Agents. It suggests some answers, and its seeming potential beckons the researcher on; it gives some clues to what may be answers, but (now) only clues.

The most obvious limitation of the research is that the initial design is still incomplete: that is, the data has been gathered, but the analysis is still in process. In some ways, this is the most obvious of the limitations. Another limitation is simply the relatively small size of the N: 108 respondents is not a large sample, by any means. Moreover, that sample is essentially a voluntary sample; people have self-selected themselves into this sample, often for reasons that we simply do not know. Theoretically, this non-random, as yet non-matched, self-selected sample puts a severe limit on the degree to which anything we learn can be generalized to the larger population. However, we all need to recall that Ernest Ligon has reminded us, almost endlessly, that voluntary samples are what we in the religious institution are going to have to study, period. There is a sense, a very real sense, in which he

has been quite right. Instead of pouting, somewhat like disappointed children, because we cannot have our dreamed of "perfect sample," we need to work with what we have, with what we can get, and learn to discipline the data that we can come by. Therefore, I admit this limitation, call it to your attention, and want to proceed to talk about an attempt to discipline this kind of limited data.

There are, then, three basic limitations upon this evaluative research and the data it has generated:

1. The limitation of <u>incompletion</u>;
2. The limitation of <u>a small N</u>;
3. The limitation of <u>a self-selected Sample</u>

Each of these limitations restricts the ease with which generalizations may be made with full "scientific" confidence. However, in the life of the Church, there appears to be a time and a place for generalizations that have less than full scientific precision: perhaps now is such a time, perhaps here is such a place.

Anyway I want to turn my attention to some proto-scientific generalizations about the "Individual as a Change Agent" that are based on this research data. (After what may have seemed like a very long prologue, a pre-mass that has gone on and on, a lengthy first course, we are come to the "meat" of this paper. The interpretation of data <u>is</u> the "meat" in any research. And that is what I want to move to now.)

3. Generalizations Based on (or Beginning with) the Research Data

You have been tolerant of my history and parenthetical observations so far, and I want now only to outline how I propose to go about this business of generalizing from the Research Data, and how I intend to deal with the topic of this paper, "The Individual as Change Agent." What I intend to look at as the basis of my generalizations is a comparison of data from the first two CCTIs, the one in Missouri and the one in Ohio. The CCTI in Missouri was made up predominantly of full-time church workers, while the one in Ohio was not; quite the contrary, it was predominantly made up of persons working for a local Community agency in Cincinnati, Ohio. The data has not been analyzed into individual patterns of response and I will, therefore, be reflecting on group data. Still, I think it will be possible to keep the "Individual" in mind, for the two groups are often related to the "Change Agent," either as sponsor or as reference group. Therefore, to look at our data as descriptive of the field in which the Change Agent works may help us to discern, however, tentatively, the outline of the person and work of the Change Agent.

In your paper you have a set of Tables numbered 1 through 11. I propose to comment directly on each, and then to engage in some comments which may be generally classified as "conservative" and as "hypothetical-radical." The result, I hope, will be some light on the task before the individual as a change agent in this society.

3a. Comments on Tables #1a and #1b Regarding Group Ages

In Table #1a there are figures for the average birth date of both participants and staff at the Missouri and Ohio CCTIs. I think it is interesting that the difference between staff and participant average ages at Missouri was only 3 years and 29 days, whereas the average difference at Ohio was 8 years and 3 days. This suggests that there might have been more homogeneity of experience, interest and values in the Missouri CCTI than in the Ohio CCTI and, implicitly, that some observeable difference in response to the CCTI might exist which could be attributable to this age-differential. This divergence in the average ages between participants and staff at these two Institutes may serve, then, as a first, early warning that differences between groups may exist, and that a part of the reason for that difference may rest in this difference in mean birth dates.

In Table #1b we have information on the range of ages of the two groups of participants: it was 34 years in Missouri and 45 years in Ohio. The idea of homogeneity seems to favor the Missouri CCTI; the Ohio group, age-wise, seeming somewhat less homogeneous. The Table also includes data regarding the median birth date--at Missouri it is 1/30/29, while at Ohio it was 9/30/35. This argues that the Ohio CCTI participants were, on the whole, a younger group. If the often used theme of youth vs. age (or middle-age, more exactly) has anything to it, one might expect to find differences

between these two groups that may be credited to this youth-middle age continuum.

Tables #1a and #1b, then, give us a hunch about the two CCTIs: one was more homogeneous than the other, and one was somewhat more youthful than the other. As we move on, let us remember these first suggestions of difference, and see if the differences we note between these two groups can reasonably be credited to either factor.

3b. Comments on Tables #2 Regarding Extent of Formal Education

When one turns to Table #2 one has a display of the frequency with which participants and staff at both Missouri and Ohio identified the extent of their own formal education. The CCTE instrument originally was made out for a population of full-time church workers, and it seemed reasonable (for that group) to have the lowest category be "High School graduate, no further training." At Missouri this expectation was borne out, but at the Ohio CCTI it was necessary to add a yet prior level to cover the Elementary School graduate and/or the High School attender who did not graduate.

The double underlining identifies the mode, or most frequently chosen category, for all four groupings. At Missouri the mode for both staff and participants is the category that includes Seminary training, the initial graduate degree or the professional school degree. However, at the Ohio CCTI the mode for

participants dropped all the way down to the new, added category of the High School non-graduate, while the corresponding drop in the staff only took it to the category of College graduates with post-graduate training without a graduate degree. The differences between participant and staff in the two CCTIs is, on this factor, marked and clearly evident.

If similarity of experience has anything to do with homogeneity of groupings, it would seem that the participants and staff in the Missouri CCTI were a homogeneous group, while they were not in the Ohio CCTI, if we use the extent of formal education as a guide to this judgment. Perhaps differences, if they exist, will reflect this difference in formal education.

3c. Comments on Table #3 Regarding Religious Affiliation

A glance at Table #3, which records responses to a Face Sheet question regarding the "Religious Affiliation" of the respondent, will show that the Missouri CCTI was, essentially, an operation of, by and for Episcopalians. The Ohio CCTI, however, was by Episcopalians, but it was for non-Episcopalians. Once again, Missouri seems to have been a more homogeneous group than was Ohio. While "Religious Affiliation" is likely to seem particularly weak as a factor whereby one "explains" differences between groups, its presence in a growing cluster of factors tending to make the Missouri CCTI seem markedly more homogeneous than the Ohio CCTI is, at the least, interesting. Perhaps future work

with the data will fray-out the meaning and influence of this factor. However, for the time being, Table #3 stands as a sign of clear difference in the religious affiliation of the participants in these two CCTIs.

3d. Comments on Table #4 Regarding Status & Function Within One's Religious Affiliation

Table #4 actually contains two kinds of data, which is indicated by the horizontal double line in the table itself. The first kind simply queries whether a person is baptized, confirmed (a matter of Anglican concern) or ordained to some "ministry." The second kind probes somewhat further into the respondent's relation to his religion by asking where the person "serves" in a parish, or in non-parochial work within the church, or in a "secular" community agency. While the simple frequency tabulation reported in Table #4 does not in any way exhaust the analytical possibilities, the double underlining identifying the modes should prove interesting. Table #4 identifies that at the Missouri CCTI the mode for participants and staff was the ordained ministry category, a contribution to homogeneity, although the probe points to the fact that most participants served in parishes, while most staff were in non-parochial work (in this instance, a denominational headquarters). At the Ohio CCTI the modes point toward a clergy-lay division between staff and participants, while the probe points toward even further heterogeneity for the participants' mode was

the community agency, while the staff was split evenly between parish and non-parish work within the church. The argument for the greater homogeneity of the Missouri CCTI, here, is slightly inverted into an argument that Ohio was marked by more heterogeniety than was Missouri.

Does this factor of status and function "explain" the differences we may observe between the Missouri and Ohio groups? The data discipline used to date does not allow an answer to that question. However, the data does not suggest that status and function are uninvolved in difference: therefore, this factor, too, must be added to the growing picture of two rather divergent groupings.

3e. Comments on Table #5 Regarding Membership in Activist Organizations

In Table #5 we move to the first of the questions added to the Face Sheet data as a result of the instrument pre-test. In this Table we have a record of the frequency with which respondents identified their membership in so-called "activist organizations." At this moment in history, after the riots of 1967 and 1968, this may not seem like a listing of genuinely "activist" groups; however, in 1966 and early 1967, considering the original population intended, it probably was a fair and representative listing, and it did prove to be useful. Once again, the Missouri staff and participant mode agrees, while the Ohio participants and staff split. Of course, the extent to which this factor of similarity of membership in ac-

tivist organizations influences, explains or "causes" any observeable differences is outside the limits of the present data analysis. But one can surely observe the differences between staff and participant on this factor and wonder: such wonder may, eventually, prove to be a clue that leads into explorations of the effect of similarity of experience upon both staff and participant.

3f. Comments on Table #6 on Participation in Activist Organizations

It was felt that to query merely about "membership" might not be either adequate or accurate, and so this question was designed to probe the extent of that "membership" which was identified. The Table shows that at the Missouri CCTI the mode for the participants was the category involving membership in a local chapter, which was originally intended to be an intermediary level of involvement between merely paying dues and being an officer of a local chapter. The staff distribution is bi-modal, with one mode coinciding with the participant mode, while one is the category of non-membership. At Ohio, while the participant mode was in the category of dues paying, an even greater group were involved in the two categories of membership in the local chapter and being an officer of such a local chapter. The Ohio staff mode, however, was in the non-membership category.

While the difference may not be as clear-cut as in some of the previous tables, the general "dimension" of homogeneity would seem to distinguish the Missouri from the Ohio CCTIs. In this case, it

would seem, the heterogeneity is a staff-participant difference, rather than a group vs. group. Perhaps the matter of involvement will turn out to have been a key, but who knows? At this point, we can only keep the divergence in mind, be aware that it contributes somewhat to the growing picture of two divergent groups, and (in the future) see if our data discipline will give us a clue to the effect of this particular factor.

3g. Comments on Table #7 Regarding Participation in Direct Community Action by Participants and Staff

Table #7 records the frequency with which participants and staff identified their own involvement in "Direct Community Action." The modes for Missouri, although different, are both within the over all category of what we could call "active" involvement. In the case of the Ohio CCTI, however, the difference is quite different. The two modes of the Ohio participants fall within the over all category of "active" involvement, while the staff mode falls into a category that can only be termed non-involvement. This, perhaps, is a very clear sign of participant-staff divergence, suggesting that the Missouri group had considerable greater homogeneity than did the Ohio group. This divergence of experience may have been a factor of some importance at the Ohio CCTI; however, only further data analysis will give us an idea of whether or not it actually was.

3h. Comments on Tables #7(a) and #7(b) Regarding the Participation in Direct Community Action via Analysis of Rank Order Correlations

In Table #7(a) the frequency counts recorded in Table #7 have been transformed into a series of ranks, using one of two standard procedures for dealing with tied ranks. This was done to allow the calculation of Spearman's Correlation Coefficient for Ranks commonly known as "rho."

In Table #7(b) the correlation mattrix for the possible combinations of the four groupings is reproduced. The figures in the cells are values of "rho," which, as Nunnaly points out, is simply a short-cut version of the more well-known Product-Moment Correlation Coefficient. (Nunnaly, J., Psychometric Methods, p. 122.) These cells, thus, tell us something about the similarity of experience of the participants and staffs of the two CCTIs in Missouri and Ohio. And what they tell us, I think, suggests that the theme of homogeneity of experience, which I have referred to repeatedly, is perhaps a reality.

The rather remarkable thing about Table #7(b) is that the "rho" for participants and staff of the Missouri CCTI is 0.6042, a tolerably "high" value, while the "rho" for participants in the two CCTIs is 0.6375, a slightly "higher" value, which could argue that the experience of the two groups of participants in direct community action was relatively similar. Now when we compare staff-participant groups across CCTIs we find that "rho" drops to 0.5625 for the Missouri staff vs. the Ohio participants' comparison, and to 0.5417 for the Missouri participants vs. the Ohio staff comparison. The

"rho" for the Ohio staff-participant comparison, however, drops even further, to a tolerably "low" figure of 0.4792, and the two staffs' general difference of experience is witnessed to by the relatively "low" figure for "rho" of 0.4000.

This comparison of groupings within these two CCTIs suggests to me that the factor of staff-participant homogeneity, whatever its final shape may turn out to have been, or its ultimate effect be determined to have been, was quite real. Moreover, this matrix suggests that there is a way, using standard measurement techniques, to record and assess the degree of such a difference. In some ways, this, in itself, seems like achievement in the field of religious research.

3i. Comments on Table #8 Regarding Participant and Staff Responsibility for Planning and Executing Local Change

The final question added to the Face Sheet Data as a result of the instrument pre-test was an open-end question inviting the respondent to write a single paragraph describing his responsibility for the planning and the execution of "Local Change." As Table #8 indicates, approximately 1/3 of the Missouri staff and participants did not write such a paragraph. Something like the same proportion of the Ohio staff did not write a paragraph, and a somewhat smaller proportion of the Ohio participants failed to write a paragraph. We are dealing, then, with about two-thirds of the staff and participants in the following comments.

The Table's mode for the Missouri CCTI finds both participant and staff indicating responsibility for planning responsibilities. The data suggests that the Missouri staff and participant experience of local change was that of planners. However, the Ohio modes indicate that the participant experience was almost evenly divided between the planning and the executing of local change with a slight edge going to the execution of "Local Change." The Ohio staff, interestingly, was equally divided. If we figure a Chi Square (X^2) on the participants' experience, however, we would find the result to be non-significant (0.780). Therefore, we must be very careful about making too much of what, at first look, seems to be divergence in experience between participants in the two CCTIs.

Content analysis of the paragraphs themselves into more discrete categories might show some difference in experience, but that must await the time when it will be possible to devote energy and time itself to a tedious task. For the moment, we must be content with noticing the slight divergence in experience, and note only that it seems to go well with the pattern of differences noticed so far.

Table #8 is the last of the Face Sheet data, all of which describes the staff and participants in the two CCTI groups in Missouri and Ohio. Beginning with Table #9 and continuing through Table #11, we will look at some differences and similarities

discovered through the California Psychological Inventory (henceforth referred to as the CPI), a set of Semantic Differential Scales (henceforth referred to as the S.D. Scales) and a Vocabulary Test. With this data we will probe the comparison of the two CCTIs to a group of "normals" (CPI), the degree and direction of changes that may be related to the effect of the CCTIs on (a) fundamental concepts and, (b) the specialized vocabulary of urban sociological analysis.

3j. Comments on Table #9 Regarding Group Means on the CPI Scales

A study of Table #9 suggests that on the CPI Scales the two staffs were remarkably similar. One might almost take these scores as a description of the kind of person characterized earlier as a Trainer-Planner. (In no case is the difference in staff group means as great as 10 standard score units.) However, a study of either the left-hand or the right-hand columns suggests that both the participants in the two CCTIs and the staff and the participants in Ohio differed along a number of the CPI scales. For instance, the staff and the participants in the Ohio CCTI differed most along those scales that mark the difference between the two participant groups. This data strongly suggests that the staff at both CCTIs and the participants at the Missouri CCTI come from a similar population. This similar population is marked by "high" scores on the CPI scales of Do (Dominance), Sp (Social Presence), Sa (Self-acceptance), Ai (Achievement via Independence), Py

(Psychological Mindedness) and Fx (Flexibility). A similar study of the Ohio CCTI participants suggests that it represents a population that is dissimilar from that population which provided both staff and participants at Missouri and Staff at Ohio. Admittedly, the Ohio participants exhibit a good deal of CPI "normality," but they also show relatively "low" scores on the CPI scales of Wb (Sense of Well-being), Re (Responsibility), So (Socialization), Sc (Self-control), To (Tolerance), and Ie (Intellectual efficiency). If one compares the differences between staff and participants (extreme right-hand column) for the Ohio CCTI it is interesting to note that nine of the eighteen CPI scales exhibited rather marked divergence between group means. (In this case "marked divergence" means a difference of 14 or more standard score units.) These nine scales are those marked with an asterisk in the right-hand column of Table #9, excepting the scales Sp (Social Presence) and Fx (Flexibility), where the order of difference is 11 and 10 standard score units respectively.

It would seem that the theme of homogeneity vs. heterogeneity of staff and participants finds, in this data, very real support. The Missouri participants and both staffs do seem to come from a similar population, one that in certain respects is supra-normal. The participants in the Ohio CCTI, however, seem to come from a distinct population that is, in some respects, relatively sub-normal.

3k. Comments on Table #10 Regarding Group Means on S.D. Scales

Table #10 records measures of change across the period of the CCTIs, measures that were built to tap change that was related to the stated goals of this program. While the CPI scales tell us something about the population involved in these CCTIs, the S.D. scales tell us something about what happened to this population as a result of the CCTI experience. In this sense, the S.D. scales represent the stuff out of which one constructs an evaluation of these two Institutes.

Because of a peculiarity in the general form of the formula used to calculate the "t" ratio, a minus sign preceding a value for "t" represents an increase in the post-test mean over the pre-test mean. In Table #10 values of "t" are arranged according to the three factors that Osgood's research discovered to be operating across a variety of studies. Each individual raw score on each factor represents, as the table indicates, a summed score on three pairs of polar adjectives.

A glance through Table #10 indicates that out of 144 cells only some 20 cells have asterisks (*) to indicate statistical significance at or below the .05 level of confidence. This over all measure of change suggests that, if both CCTIs are taken together, the data argues more for a lack of significant change than for its presence. (For instance, through use of the binomial theorem, one would expect by chance alone that half of the cells would show sig-

nificant change: i.e., 72 out of the 144 cells. The binomial theorem would also lead us to expect that the standard deviation of such a chance distribution would be 6.000. The difference between the chance mean of 72 and the observed number of significant cells, 20, is 52, or some 8.6 standard deviations from the mean expected by chance alone. Thus, there seems every reason to characterize the data as supporting a null hypothesis of an insignificant amount of change, when both CCTIs are considered together.)

The evaluative question of the "effect" of the CCTIs based on this look at the data, should lead to a judgment of no <u>evident change</u>. However, lumping the Missouri and Ohio CCTIs together may well be a case of losing a sign of change through putting together two admittedly dissimilar groups. Let us, therefore, look at each CCTI by itself.

When we look at the two CCTIs separately, the table gives us 72 cells per CCTI, with a chance expected mean of 36, and a standard deviation by chance alone of 4.246. The results for the Missouri CCTI are that of the 72 cells only some 18 show significant change. This is 18 from the chance mean of 36, or some 4.23 standard deviations from the mean. Thus, the argument of lack of change, or stability, seems to still stand. The Ohio CCTI cells show only two cells where the sign of significant change is marked with an asterisk. The difference with the chance mean of 36 is 34, or 8 standard deviations. In this instance, there seems little reason

to doubt the "stability" of the group, given this pre-post data.

While "stability" seems to be dictated by the data, a look at the S.D. concepts that do show a significant change will still be useful. While the amount of over all change is less than any of us expected, the places where change did take place may offer useful clues.

First, let us look at the Missouri data, since the greatest number of changes took place at that CCTI. The Missouri CCTI participants had a "t" value on the Potency Scale for the S.D. concept ENGAGEMENT of -2.0976, which was significant at the .05 level. The "t" values on the Evaluative and Activity scales did not reach statistical significance, but they do indicate change in the same direction. One could say that the Missouri CCTI experience was associated with an increase in the sense of the potency of the concept ENGAGEMENT. Similarly, the "t" value for the Evaluative scale of the S.D. concept POWER was -2.6954, again significant at the .05 level, and there was movement in the same direction on both other S.D. scales. Thus, there was an increase in the participants' evaluation of POWER. The S.D. concept of PUNISHMENT showed a significant increase on the S.D. scale for Potency ("t" = -2.1709), and the other two scales also showed movement in the same direction. Thus, the participants' sense of the potency of the concept of PUNISHMENT increased. The S.D. concept of SELF-INTEREST showed significant change on both the

Evaluative and the Activity scales, with a same direction (but non-significant) movement on the Potency scale. Thus, the participants' evaluation and sense of the activity of the concept of SELF-INTEREST increased significantly. The S.D. concept of COLLABORATION showed a consistent movement in the expected direction, with a very highly significant shift on the potency scale ("t" = -3.4489 p = .001).

The S.D. concept of CHURCH, interestingly enough, showed a consistent movement on all three S.D. scales, in the unexpected direction. The decrease on the Potency and Activity scales was significant and highly significant, respectively. Thus, the participants' sense of the potency and activity of the CHURCH decreased significantly across the five days of the Missouri CCTI. This finding of significant decrease in the concept CHURCH in the population of full-time Church workers stands in sharp contrast to the direction of movement (albeit that movement was non-significant) in the participants in the Ohio CCTI. (An interpretation of this empirical finding could be a fascinating project in itself.)

The final five S.D. concepts on Table #10 represent a restatement of the declared goals of the CCTI in terms of individual abilities; as such, they give a reading of the participants' perception of their goal-directed abilities which, according to the CCTI planners, was the whole point of the program. For the Missouri participants the direction of change was, in all instances,

in the expected direction, and the amount of that change was significant at or below the .05 level for four of the five S.D. concepts: STRATEGY PLANNING, COALITIONS, EVALUATE and FEELINGS. Only the concept DIAGNOSE, which dealt with the ability to diagnose social systems, did not show significant change. Thus, the participants at the Missouri CCTI appear to have left with a sense of an improvement in their abilities. However, it is important to realize that what we have in these S.D. concepts differs from the preceding Concepts in that they are, obviously, a reading of self-perceived "abilities." The data may, therefore, suffer from a kind of "softness." Whatever the reason for these changes, the participants' evaluation, sense of potency and sense of activity of four of these five abilities did, in fact, increase.

Next, when we turn to the Ohio CCTI participants' S.D. scales, we find an almost complete lack of evidence for significant change. The only two S.D. concepts that exhibited significant change were the concepts of SELF-INTEREST and COALITIONS (an "ability" concept). The S.D. concept SELF-INTEREST shows a movement in the expected direction on all three S.D. scales, but a significant change only on the Evaluative scale at the .05 level ("t" = -2.2759). Thus, one can say that the participants at the Ohio CCTI showed significant change in their evaluation of the concept of SELF-INTEREST, and movement in the expected direction of a non-significant sort in their sense of potency and sense of activity regarding this concept.

(It is interesting to note that the Ohio participants showed significant change on their Evaluative scale, while the Missouri participants showed an even more striking change on their Activity scale for SELF-INTEREST.)

The second concept on which the Ohio participants showed significant change of S.D. scale scores was the "ability" concept of COALITIONS, which dealt with the ability to form and use coalitions for social change. A fascinating thing about the "t" values for the Ohio participants on this scale is that all are in the same direction, that which points toward a decrease in their evaluation, sense of potency and sense of activity of the concept of their own ability to form COALITIONS. One might, of course, argue that the Ohio participants came to the CCTI overly optimistic, but that argument might more readily have been made of the Missouri participants. It looks as if the Ohio CCTI, somehow, left these persons less certain of their "ability" vis-a-vis this concept than when they arrived: this is in direct opposition to the intended goals of the CCTI, and may well be a springboard for some searching questioning of the usefulness of the procedures used in the Ohio CCTI. (Such a searching questioning must, of course, be undertaken by those who were on the staff in Ohio, since this researcher was not there and has no detailed idea of what did, in fact, take place. However, I would hazard a guess that the procedures which worked in November, 1966, with church workers in Missouri simply did not work when used

with the different participant population at the Ohio CCTI. This failure might, in fact, cause one to wonder about the applicability of a format developed for the kind of supra-normal population at Missouri with the sometimes sub-normal population at the Ohio CCTI.)

Before leaving Table #10 we may look briefly at the results of using the "Sign Test" (Credited in A. L. Edwards' Statistical Methods for the Behavioral Sciences, p. 288, to Dixon and Mood, "The Statistical Sign Test," in the Journal of the American Statistical Association, 41, 557-566) to see whether several configurations of S.D. scales, taking only the direction of movement indicated by the plus or minus sign attached to the value of "t" as sign of the direction of movement, would indicate a significant movement in either CCTI. Although a column by column check of Table #10 indicates that the movement in the Activity scale column for the Missouri CCTI alone significantly departs from chance, when we look at either CCTI as a whole, or at both CCTIs together, the pattern of movement in the expected direction is significantly different from chance. The "z" for the over all patterns are as follows: Missouri, "z" = 2.12; Ohio, "z" = 2.35; Total, "z" = 3.08. In all three instances "z" is a large enough value to make the chance probability of occurrence probable at or below the .03 level. Thus, while the amount of significant change is small enough to give a strong (and true) impression of "stability," when we look only at

the direction of movement, whether that movement be statistically significant or not, we find that movement to have been cumulatively significant.

This cumulatively significant movement is, I suspect, what the Trainers on the Missouri and Ohio staffs felt "intuitively," when they said that both CCTIs were "good." It has been a long road to find empirical evidence of this intuitive judgment, but I think this is the supporting evidence for their feeling, and about the only such evidence. One may, of course, still question whether the CCTI did, in fact, perform up to the pre-Institute expectations of its planners.

31. Comments on Table #11 Regarding the Vocabulary Test

In Table #11 the results of the Vocabulary Test constructed especially for the CCTIs is recorded. Two things can be said of this table: (a) the increases between pre-test and post-test scores for only the Missouri CCTI gives a "t" value that is statistically significant; and (b) the comparison of the absolute group mean scores, both pre- and post- indicate that, while the Missouri CCTI participants left with the vocabulary well in hand, except for the word CONTROVERSY, the Ohio CCTI participants most certainly did not. This is a case where statistical tests of significance are not necessary to see the common sense reality that a specialized vocabulary was taught in the Missouri CCTI (admittedly, to people who already appear to have been relatively familiar with it, so that the process

was one of sharpening definitions and making vocabulary usage more precise), while in the Ohio CCTI only four words were correctly identified by more than 50% of the participants in the post-test -- i.e., POWER (70%), LINKAGES (65%), COMMUNICATION PATTERNS (55%), and SELF-INTEREST (50%).

It would seem that the CCTIs were effective in sharpening an analytical vocabulary for the Missouri population of somewhat supra-normals, but was not effective in doing so for the Ohio population of somewhat sub-normals. The difference between the participants on the Ie scale of the CPI of 16.933 standard score units might be a clue to the reason for this markedly different result from one CCTI to the other.

3m. Some Semi-Random Observations on the Research and the Data and on the Individual as a Change Agent

We have come a long way to reach this point, and I hurry on toward the conclusion, which will come shortly. However, before concluding, I want to make some observations about the matter of training "Change Agents."

It seems that the CCTI format is a relatively effective way to do some things, such as:

1. Change self-perceptions of one's abilities
2. Sharpen a specialized, analytical vocabulary
3. Denigrate the concept of CHURCH

4. Increase the sense of Potency in the concept **ENGAGEMENT**

5. Increase Value placed on the concept **POWER**

6. Increase the sense of Potency in the concept **PUNISHMENT**

7. Increase both the Value placed on, and the sense of Activity of the concept **SELF-INTEREST**

8. Increase the sense of Potency of the concept **COLLABORATION**

But this is true only for members of a population of full-time church workers, who also provided the staffs, somewhat supra-normal.

It seems that the CCTI format is nowhere as effective with a population that resembles the Ohio participants, predominantly normal but selectively somewhat sub-normal.

Now, the Change Agent, if he belongs to the Missouri-staff population has a problem: the people he may well want most to help help themselves are likely to be like the Ohio participants, who came from a Cincinnati "ghetto" neighborhood. The problem of the Change Agent, both as an individual and as a member of a population like that in the Missouri CCTI, is often called "relating." There is ample evidence in this data, even though its disciplining is still incomplete, to suggest that the CCTI approach "works" only with a selected, specialized population. Moreover, the number of statistically significant shifts on S.D. scales, even there, is far, far less than expected. (I do not regard this report as, in

any way, serving to reinforce the optimistic expectation that "a good thing" would be supported by "good research.")

The problem of giving orientation and training for change agentry does not seem to have an answer in the five-day CCTI format. At best, it has the beginnings of an answer: it would seem that much careful work needs doing, before the somewhat romantic dreams of effectiveness find fruition in observeable, measureable change.

I wonder, do the relatively "low" CPI group mean scores for the Ohio participants suggest that along such factors as: Sense of Well-Being, Responsibility, Self-control, Tolerance, and Intellectual efficiency, there is rewarding research? Does this pattern characterize the "ghetto" resident? (I do not know for the data is limited to such a small number, but the question should stand for further research.) Does the factor of Intellectual efficiency really covary with low vocabulary scores, and with a pattern of minimal change (albeit in the "expected" direction)? Is the factor that CPI calls Responsibility a predictor for "successful" Change Agents? (Is a high "Responsibility" score a help or a hindrance to the Change Agent?)

Christian religious educators have talked a good deal about beginning where people are. I wonder, can we see in this data some support for this old pedagogical saw: when there was a kind of homogeneity of staff and participants, changes took place more fre-

quently than when there was not such homogeneity. Is the descriptive category of homogeneity, however, one we can be content with? Must we not find ways to get out of the like-to-like "success" pattern, ways that allow likes-and-unlikes to work together with a similar pattern of success?

I conclude with these questions. Answers are only hinted at in the data. There may be even better hints when the data disciplining is carried further. And, surely, others may pick up and carry these hints to the point where they can be accepted or rejected.

We seem to know how to do something (minimally) with people <u>like</u> ourselves; we do not, yet, seem to know how to transfer that ability to people <u>unlike</u> ourselves. In an increasingly pluralistic Society, where the various "unlikes" are conscious of their distinctivenesses, this question of the effect of group heterogeneity is a major, practical research and development question. We can run programs, true. We can intuit success, for sure. But to attain the goals we set for ourselves, that is still an elusive goal.

The individual who would be a Change Agent, it would appear, will do better with "his own people, his own kind," than with those significantly unlike him. This rather pessimistic conclusion seems to be dictated by the research reported today, given the extent of the data discipline applied to it.

TABLE #1a: Average Age (Given as Birth Dates) of Participants & Staff

MISSOURI CCTI — Total N = 40		OHIO CCTI — Total N = 26	
Participants (N = 32)	Staff (N = 8)	Participants (N = 20)	Staff (N = 6)
Average Age = 07-15-29	Average Age = 06-17-26	Average Age = 06-17-32	Average Age = 06-14-24
Difference = - 00-29-03	Difference = + 00-29-03	Difference = - 00-03-08	Difference = + 00-03-08

TABLE #1b: Range of Ages and Median Birth Date of Participants

MISSOURI CCTI — Participant N = 32	OHIO CCTI — Participant N = 20
RANGE OF AGES (Birth Dates): 34 years	RANGE OF AGES (Birth Dates): 45 years
MEDIAN BIRTH DATE: 01-30-29	MEDIAN BIRTH DATE: 09-30-35

TABLE #2: Extent of Formal Education of Participants & Staff

	MISSOURI CCTI (N = 32)		OHIO CCTI (N = 20)	
	Participants	Staff	Participants	Staff
00 = Elem. Sch. or High School w/o Graduation:	0	0	8	0
01 = High Sch. graduate, no further training	1	0	3	0
02 = Attended College, Business or Technical School.	1	0	4	0
03 = College (4 yr.) Grad., w. Bachelor's degree.	4	0	1	0
04 = College Grad. + post-grad. training w/o degree.	8	2	3	3
05 = Post-Grad. training + degree at Masters level (MA,BD,LLB)	17	5	1	2
06 = Post-Grad. training + degree at Masters+ level (STM,LLD)	0	1	0	0
07 = Post-Grad. training + degree at Doctoral level (STD,EdD,PhD)	0	0	0	1
NA = No Answer	1	0	0	0

NOTE: Blue double underlining (=====) = Mode.

TABLE #3: Religious Affiliation of Participants & Staff

	MISSOURI CCTI (N = 32)		OHIO CCTI (N = 20)	
	Participants	Staff	Participants	Staff
01 = Episc.	25	7	4	6
02 = U.C.C..	1	0	1	0
03 = Presby.	0	1	2	0
04 = Methodist	1	0	3	0
05 = Other	5	0	10	0
NA = No Answer	0	0	0	0

NOTE: a) Mo. CCTI "Other" = 1 each, Lutheran Church, Mo. Synod; Jewish; Roman Catholic; Lutheran Church in America; "Protestant"

b) Ohio CCTI "Other" = 3 Catholic; 2 Baptist; 1 each, Lutheran, Pentecostal, Islamic, Jewish, None.

NOTE: Mode indicated by double underling (═══).

TABLE #4: Status & Function Within Religious Affiliation of Participants & Staff

	MISSOURI CCTI (N = 32)		OHIO CCTI (N = 20)	
	Participants	Staff	Participants	Staff
O1 = Baptized Lay Person	1	0	5	0
O2 = Confirmed Lay Person	4	2	6	1
O3 = Ordained Minister	24	6	2	5
O4 = Serving in a Parish	21	2	2	3
O5 = Serving in non-parish work within the Church.	8	6	2	3
O6 = Serving in Community Agency	2	0	8	0
NA = No Answer	2	0	4	0

NOTE: Modes indicated by double underlining (▬▬)

TABLE #5: Membership in Activist Organizations of Participants & Staff

	MISSOURI CCTI — Total N = 40		OHIO CCTI — Total N = 26	
	Participants (N = 32)	Staff (N = 8)	Participants (N = 20)	Staff (N = 6)
01 = SNCC, CORE, NAACP, ESCRU.	23	4	8	2
02 = Local Community Organization	3	1	5	0
03 = Local Poverty Brd (OEO auspices)	5	1	8	0
04 = Local Poverty Program	7	1	4	0
05 = Other (Specified)	6	3	3	1
06 = None: I do not belong to any activist organization.	4	3	2	3
NA = No Answer	0	0	0	0

NOTE: Mode indicated by double underlining (═══)

TABLE #6: Participation in Activist Organizations of Participants & Staff

	MISSOURI CCTI — Total N = 40		OHIO CCTI — Total N = 26	
	Participants (N = 32)	Staff (N = 8)	Participants (N = 20)	Staff (N = 6)
01 = I do not belong to any activist organization	5	<u>3</u>	2	<u>3</u>
02 = Pays dues, no more	10	1	<u>6</u>	2
03 = Member of local chapter	<u>15</u>	<u>3</u>	5	1
04 = Officer of a local chapter	1	1	5	0
05 = Other (Specified)	4	0	3	0
NA = No Answer	1	0	1	0

NOTE: Mode indicated by double underlining (<u> </u>)

TABLE #7: Participation in Direct Community Action (Participant & Staff)

	MISSOURI CCTI — Total N = 40		OHIO CCTI — Total N = 26	
	Participants (N = 32)	Staff (N = 8)	Participants (N = 20)	Staff (N = 6)
01 = Rent Strike	0	1	2	0
02 = Voter Registration Drive	8	2	**8**	0
03 = Picket	8	**6**	5	1
04 = No Participation	10	1	6	**5**
05 = Sit-in	2	0	3	0
06 = Economic Boycott	2	2	3	0
07 = Protest March	**13**	5	**8**	0
08 = Other (Specified)	10	2	2	0
NA = No Answer	2	0	0	0

NOTE: Mode indicated by double underlining ( )

TABLE #7(a): Rank Order of Participation in Direct Community Action By Types of Action (Participants & Staff)

	MISSOURI CCTI: N_t = 40		OHIO CCTI: N_t = 26	
	Participants (N = 32)	Staff (N = 8)	Participants (N = 20)	Staff (N = 6)
Rent Strike	8.0	6.5	7.5	6.0
Voter Reg.	4.5	4.0	1.5	6.0
Picketing	4.5	1.0	4.0	2.0
Nothing	2.5	6.5	3.0	1.0
Sit-in	7.0	8.5	5.5	6.0
Boycott	7.0	4.0	5.5	6.0
Protest March	1.0	2.0	1.5	6.0
Other	2.5	4.0	7.5	6.0
No Answer:	7.0	8.5	9.0	6.0

TABLE #7(b): Correlation Mattrix (Entries = Spearman's Rho)

		MISSOURI CCTI:		OHIO CCTI:	
		Particip.:	Staff:	Particip.:	Staff:
MISSOURI CCTI:	Particip.:	1.0000	0.6042	0.6375	0.5417
	Staff:	0.6042	1.0000	0.5625	0.4000
OHIO CCTI:	Particip.:	0.6375	0.5625	1.0000	0.4792
	Staff:	0.5417	0.4000	0.4792	1.0000

NOTE: For all of the Rhos in the above mattrix the Standard Error of Rho is equal to 0.3535.

TABLE #8: RESPONSIBILITY FOR PLANNING & EXECUTING LOCAL CHANGE

	MISSOURI CCTI — Total N = 40		OHIO CCTI — Total N = 40	
	Participants (N = 32)	Staff (N = 8)	Participants (N = 20)	Staff (N = 6)
Paragraph Written:	20	5	16	4
Planning Responsibility Indicated	15	5	12	3
Executing Responsibility Indicated	10	2	13	3
No Paragraph Written	12	3	4	2

NOTE: Mode indicated by double underlining (▬▬)

TABLE #9: GROUP MEANS ON CALIFORNIA PSYCHOLOGICAL INVENTORY
(All Scores Expressed as Standard Scores)($\bar{X}$ = 50)

Scale:	CPI Participant Means in Standard Scores Mean = 50.000 S.D. = 10.000		CPI Staff Means in Standard Scores Mean = 50.000 S.D. = 10.000		Direction & Difference of Means Between the Staff & Participants S.D. = 10.000		CPI Scale:
	Missouri (N = 32)	Ohio (N = 20)	Missouri (N = 8)	Ohio (N = 6)	Missouri	Ohio	
Do *	64.100	51.900	65.250	66.000	-01.150	-14.100 *	Dominance
Cs *	58.733	47.650	60.625	61.833	-01.892	-14.183 *	Cap. Status
Sy	55.233	47.750	55.750	56.500	-00.517	-08.750	Sociability
Sp *	61.233	49.200	66.500	60.333	-05.267	-11.133 *	Soc. Presenc
Sa	64.266	55.250	65.625	61.333	-01.359	-06.083	Self-Accept.
Wb *	53.400	40.150	48.125	54.833	+05.275	-14.683 *	Well-being
Re *	54.466	40.400	50.500	57.333	+03.966	-16.933 *	Responsibilt
So	47.233	39.750	41.500	48.500	+05.733	-08.750	Socializatio
Sc	46.733	40.700	41.625	49.666	+05.108	-08.966	Self-Control
To *	56.200	40.650	51.625	56.666	+04.575	-16.016 *	Tolerance
Gi	45.466	42.350	39.875	48.666	+05.591	-06.316	Good Impress
Cm	50.400	52.150	50.250	55.666	+00.150	-03.516	Communality
Ac *	54.333	41.750	55.625	61.000	-01.292	-19.250 *	Ac Conform.
Ai *	63.066	45.650	64.000	65.833	-00.934	-20.183 *	Ac Independ.
Ie *	56.333	39.400	58.250	55.166	+01.917	-15.766 *	Intell.Effic
Py *	63.166	49.200	65.250	68.333	+02.084	-19.133 *	Psych.Minded
Fx *	66.900	48.600	67.125	58.666	+00.225	-10.066 *	Flexibility
Fe	51.266	48.400	51.625	52.666	-00.359	-04.266	Femininity

NOTE: An asterisk (*) in the left hand column indicates a CPI Scale on which the participants' means differ by 10.000 Standard Score units or more. These may be factors (or "dimensions") along which useful research into the differences between ghetto residents &/or workers and others might be pursued. (JHP-1-18-69)

NOTE: An asterisk (*) in the extreme right hand column indicates a CPI Scale on which Participants & Staff (Ohio) differed by 10,000 or more Standard Score units. (JHP-1-18-69)

NOTE: A minus (-) in extreme right column means that Staff Mean is higher than Participant Mean by that amount of Standard Score units.

TABLE #10: SCORES ON FISHER'S "t" (PRE- vs. POST-) ON TWENTY FOUR CONCEPTS MEASURED VIA SEMANTIC DIFFERENTIAL SCALES: ONLY PARTICIPANT RESPONSES USED IN FIGURING "t".

mantic fferential ncept:	Semantic Differential EVALUATIVE Scale: Score based on Sum of 3 adjective pairs.		Semantic Differential POTENCY Scale: Score based on Sum of 3 adjective pairs.		Semantic Differential ACTIVITY Scale: Score based on Sum of 3 adjective pairs.		Semantic Differential Concept:
	Missouri (N = 32)	Ohio (N = 20)	Missouri (N = 32)	Ohio (N = 20)	Missouri (N = 32)	Ohio (N = 20)	
GAGEMENT	-0.7975	-0.4651	-2.0976*	0.0000	-1.1293	-0.1439	ENGAGEMENT
ANGE	-0.8904	-0.3896	+0.6460	+0.2352	-1.2589	+0.4795	CHANGE
WER	-2.6954*	-0.8976	-0.5804	-0.4316	-1.8176	-0.9433	POWER
WARD	-0.5227	-0.8848	-1.5588	+0.6048	-1.3618	-0.6355	REWARD
NISHMENT	-0.0816	+0.9178	-2.1709*	-0.0562	-1.2060	-0.9938	PUNISHMENT
LF-INTEREST	-1.9378	-2.2759*	-1.3759	-0.9399	-2.9047**	-1.8707	SELF-INTERES
GOTIATION	-0.1930	-0.9857	+0.4222	-1.2595	-0.3444	-1.2127	NEGOTIATION
LLABORATION	-1.5790	-0.7051	-3.4489***	-1.0823	-1.2334	-0.2588	COLLABORATIO
NFLICT	+0.4775	-0.2542	+0.1491	-0.6543	-0.8645	-0.5846	CONFLICT
HIEVEMENT	-0.6687	-0.8876	+0.2471	-0.4322	-0.4108	-0.3228	ACHIEVEMENT
CURACY	+0.0952	+0.6274	+0.2864	+0.8757	+0.5291	+0.3334	ACCURACY
ELINGS	+1.6603	+0.8145	+0.9730	-0.8027	-1.4876	-1.8157	FEELINGS
NGE AGENT	-1.2188	-0.4459	-1.4492	-0.7939	-0.3043	0.0000	CHANGE AGENT
URCH	+1.8481	-0.3236	+2.0267*	-0.5727	+2.8479**	-0.1571	CHURCH
	+0.1052	-0.2809	+1.1443	-1.4751	+0.3273	-1.5704	ME
IEST	+1.6460	-0.1558	+1.3916	-0.7024	+1.1911	+0.3855	PRIEST
STOR	+0.9426	-0.1735	+1.2829	-0.0428	+1.4765	-0.0647	PASTOR
OPHET	+1.1293	+0.1209	+0.7414	+0.0390	+1.2256	+0.4449	PROPHET
ACHER	+0.2569	0.0000	-0.4006	-0.7512	+1.0437	-0.7303	TEACHER
AGNOSE	-1.7677	+0.2238	-1.3944	+0.2749	-1.5067	+0.6402	DIAGNOSE
RATEGY PLAN	-2.0940*	+1.5197	-1.7927	+1.1059	-2.1240*	+1.8437	STRATEGY PLAN
ALITIONS	-2.9277**	+1.1988	-2.8503**	+1.2488	-3.0436**	+2.0601*	COALITIONS
ALUATE	-3.3189**	+1.5955	-3.0599**	-0.3924	-2.9404**	-0.1157	EVALUATE
ELINGS	-4.0968***	+0.7852	-4.6402***	0.0000	-2.8099**	-0.5699	FEELINGS

NOTE: A negative value of "t" represents an increase in the Post-test Mean over the Pre-test Mean (the "expected" direction).

NOTE: * = .05 level; ** = .01 level; *** = .001 level (of significance).

TABLE #11: VOCABULARY TEST FROM CCTE (% OF SAMPLE N MAKING CORRECT ANSWERS ON PRE-TEST AND POST-TEST)

Index	VOCABULARY "WORD" (Definition Given)	Pre-Tests		Post-Tests		Direct and Difference		Rank Order (Dir./Diff.)		Index
		Mo.	Ohio	Mo.	Ohio	Mo.	Ohio	Mo.	Ohio	
01	GOALS	84.37	45.0	84.37	30.0	00.00	-15.0	13	13.5	01
02	STATUS	56.25	35.0	84.37	35.0	+28.12	00.0	6	10.5	02
03	ROLES	75.00	35.0	81.25	30.0	+ 6.25	- 5.0	11	12.0	03
04	SANCTIONS	75.00	15.0	90.62	20.0	+15.62	+ 5.0	9	7.5	04
05	LINKAGES	62.50	30.0	93.75	65.0	+31.25	+35.0	3.5	2.0	05
06	VALUES	43.75	40.0	68.75	25.0	+25.00	-15.0	7	13.5	06
07	DECISION-MAKING	56.25	35.0	75.00	35.0	+18.75	00.00	8	10.5	07
08	COMMUNICATION PATTERNS	50.00	35.0	81.25	55.0	+31.25	+20.00	3.5	4.0	08
09	SELF-INTEREST	46.87	05.0	84.37	50.0	+37.50	+45.0	2	1.0	09
10	CONFLICT	65.62	25.0	68.75	30.0	+03.13	+05.0	12	7.5	10
11	NORMS	53.12	35.0	81.25	40.0	+28.13	+05.0	5	7.5	11
12	BOUNDRY MAINTENANCE	18.75	15.0	87.50	25.0	+68.75	+10.0	1	5.0	12
13	CONTROVERSY	59.37	20.0	46.87	25.0	-12.50	+05.0	14	7.5	13
14	POWER	78.12	45.0	87.50	70.0	+09.38	+25.00	10	3.0	14
	Rank Order Correlation Coefficient (Spearman's), Rho = 0.5396. Standard Error of Rho = 0.2773.									

TABLE #12, PARTICIPANT GROUP MEANS ON CPI FROM CCTIs IN MISSOURI, OHIO AND HAWAII (1966-1967).

	MISSOURI CCTI:	OHIO CCTI:	HAWAII CCTI:	
	Participants -- (N = 32)	Participants -- (N = 20)	Participants -- (N = 42)	
Do(Dominance)	64.100	51.900	60.76	01
Cs(Cap.Status)	58.733	47.650 *	57.80	02
Sy(Sociability)	55.233	47.750	52.11	03
Sp(Social Presence)	61.233	49.200	56.00	04
Sa(Self-Acceptance)	64.266	55.250	59.90	05
Wb(Well-being)	53.400	40.150	2.47	06
Re(Responsibility)	54.466	40.400 *	51.42	07
So(Socialization)	47.233	39.750	44.83	08
Sc(Self-Control)	46.733	40.700	47.45	09
To(Tolerance)	56.200	40.650 *	55.09	10
Gi(Good Impression)	45.466	42.350	45.61	11
Cm(Communality)	50.400	52.150	49.09	12
Ac(Achievement Conform)	54.333	41.750 *	54.16	13
Ai(Achieve. Independ.)	63.066	45.650 *	61.78	14
Ie(Intellectual Effic.)	56.333	39.400 *	53.42	15
Py(Psychological Minded)	63.166	49.200 *	61.95	16
Fx(Flexibility)	66.900	48.600 *	64.02	17
Fe(Femininity)	51.266	48.400	52.83	18

NOTE: An Asterisk (*) indicates a CPI Scale on which a difference of 10 or more Standard Score units exists between the Ohio group and either or both the Missouri or the Hawaii groups.(JHP-1-20-69)

Appendices are available from:

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