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ABSTRACT

Activities of the Teacher Corps' "Corps Member Training Institute" (CMTI) were studied to illustrate methods of inquiry which can be adapted to the study of teacher training events to capture some of the reality of these events. For the CMTI, a Study Team was mandated to demonstrate the impact of the Institute (identify, document, judge, and communicate features). The Study Team interpreted this mandate to mean demonstrate the salient features in an evolving, interactive, teacher training event so that the features could be adapted for use in other teacher training situations. By identifying procedures that captured the patterns of interactions, the reality of the event being examined was reflected. The team rejected standard methodologies as not useful because they fail to provide adequate information about the dynamics of the event and because too little useful information is uncovered that could be used in replicating features of the event. They found that a useful collaborative technique for identifying the important characteristics of teacher training events is a combination of a multilevel data gathering approach, a time-series design, field study procedures involving observation, interviews, and photography. (MM)

PROBLEMS IN ANALYZING DYNAMIC EVENTS IN TEACHER EDUCATION

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PROBLEMS IN ANALYZING DYNAMIC EVENTS IN TEACHER EDUCATION

The purpose of this paper is to reflect on the activities of the CMTI Study Team from a methodological perspective. It is our intent to draw your attention to three points:

1) That any methodology used by an investigator must first reflect the reality of the event being examined.

2) That the typical methodologies often used to study teacher training events are too limited in that they fail to capture the reality of such events.

3) That there are methods of inquiry which can be adapted to the study of teacher training events which if used allow one to examine and if clever capture some of the reality of such events.

The Reality of Dynamic Teacher Training Events

When collaborating on the study of any phenomena the methodologist must first ask What are the questions that need to be answered?" Once those questions are determined, the essential characteristics of the phenomena must be identified so that a plan for data collection, analysis and documentation can be established.

For the Teacher Corps' "Corps Member Training Institute" (CMTI), the policy mandate to the Study Team was to demonstrate the impact of the Institute. The key term, demonstrate, implies that the Study Team was to identify, to document, to judge, and to communicate some features of the event that were successful or unsuccessful

The language and meaning of this mandate leaves no doubt that the need for demonstration is more than most standard meanings given to program evaluation in at least two important aspects. First, the responsibilities for demonstration include more than the documentation that certain objectives were attained and others were not. Such documentation does not communicate enough about the training procedures to other staffs who may want to try these procedures in their own settings. Second, demonstration is not merely the collection of all program residue. The responsibility for communicating specific training procedures and their successes and failures under certain program conditions to individuals not involved with the program requires more than a collection of all the instructional materials used in the program. Yet so often we see investigators when asked to show a training program to an outsider respond by collecting their materials. They rely upon an effective display to show that the program should be reproduced at another setting. That kind of information is both too much and too little. It is too much of the content for an outsider to place into a meaningful context and it is too little of the rationale, intentions and specific instructional procedures used to make this content meaningful.

The purpose of demonstration is to communicate to those not originally involved with a particular program the salient features of the program. The overriding question is: What does an outsider have to know to try some of the specific features of the training strategy in another setting? Thus, the special challenge of demonstration can be stated as "describing the successes and failures of specific features of a training program for possible replication at other sites." The underlying meaning of demonstration is to be found in the implications of the phrase "for possible replication at other sites." In fact, it is the meaning of the word "replication" that must be understood.

In teacher training we never mean replication in a strictly scientific sense. No training event can be replicated, if by "replicated" we mean that the event can be made to reoccur precisely as it once occurred. The 1975 CMTI, for example, could not be replicated even if one chose the same instructors, the same interns, and returned for another month at Richmond. Nor does the word "replication" mean adoption. Adoption is to make an outside program one's own by selection or assent. It implies little or no change of the outside program. Rather the word "replication" implies adaptation. Adaptation suggests significant change of the original. Adaptation of an instructional process or a program is not only to alter some salient features of that program but to alter those features of that program to meet one's own needs. The complexity of most educational environments necessarily implies that the meaning of program replication is the identification of features which can be adapted for use in other sites. For example, one of the initial features of CMTI identified by the Study Team was the "esprit de corps" that was created and maintained during the training session. This apparently important feature of CMTI could undoubtedly be replicated (adapted) in other training settings.

In summary, when adaptation is expected from program demonstration, the responsibility of the demonstrator goes beyond documenting effectiveness and displaying program materials. The responsibility extends to identifying, to describing and to judging the value of features which others may adapt for their purposes. The identification of salient features embedded in instructional program which may (or may not) be adaptable is not a simple task. In short, the difficulties of program replication place creative responsibilities upon those undertaking program demonstration.

Characteristics of Dynamic Events. The salient features to be demonstrated for CMTI are those which characterize dynamic events. The adjective "dynamic" has been deliberately chosen to describe teacher training events because they have the following characteristics:

1) They involve humans interacting with each other.

2) They occur over time.

3) They are concerned with some substantive content.

4) The content is brought out over time through an interactive process.

5) Organizing 1, 2 and 3 into an event is deliberate in order to have effects or outcomes on the participants.

Furthermore, the interactive process is not static. The variety of participant responses often changes the content and direction of the event, some outcomes are often embedded within the process itself, each event is occurring within a larger and complex learning milieu, and the apparent content of the event shifts and changes when viewed from different personal and time perspectives. Presumably, to understand what happens in a training event, one must have a description of the event itself which illuminates these characteristics. In particular, any event must be considered as a mixture and interaction of physical, social and moral-psychological environments. The physical space (time, place and objects) in which an event occurs is suggestive, facilitating and constraining, with respect to what occurs. What people do and say to and with others--both "direct" observable behavior (naturally occurring "free" actions and responses)--are part of every event. And finally, the intentions of all the participants shape and give meaning for each individual to each event. Salient features of demonstrated events are not definable in a conventional sense. Features of dynamic events are planned and have a definite structure but the embedded patterns of interactions cannot be entirely anticipated.

The methodologist's task is to identify procedures which capture the patterns of interactions in order to describe the course of events and predict effects. He must use theoretical constructs of social interactions, or operational categories based on an empirical examination of the event to identify patterns. And again, effects are not just terminal outcomes; effects include how ideas develop and change over time.

In summary, our first point was that the methodology used must reflect the reality of the event being examined. For the CMTI Study Team this was interpreted to mean that we were to demonstrate the salient features of an evolving, interactive, teacher training event so that they could be adapted by others for use in their teacher training situations.

The Inadequacy of Standard Evaluation Methods

Standard methodologies are not useful for demonstrating teacher training events because they are too limited. They fail to provide adequate information for policy decisions, they provide insufficient data about the dynamics of the event for an understanding of the event, and they provide too little useful information which one could use in replicating features of an event.

Although the information presented here is not new, we felt obligated to point out these inadequacies for two reasons: first, in the past, evaluators of Teacher Corps projects have primarily used such methods; and second, it has been our experience that many teacher educators are unaware of the limitations of standard evaluation methodologies.

Fox (1976) in analyzing and summarizing prior evaluations of Teacher Corps projects has characterized those as employing either a "one shot case study" preexperimental design or a "one group pretest, posttest" preexperimental design (Campbell and Stanley, 1963). These are two of a set of

conventional designs borrowed from an agricultural experimental paradigm. In general, this methodology is based on three key assumptions: (1) treatment effects are additive, (2) treatment effects are constant, and (3) there is not interference between different experimental units. Before explaining these assumptions and discussing their consequences, the three terms--treatments, effects, and experimental units--must be understood.

Treatment. The objective of most agricultural experiments is to compare the yields of a number of plant varieties, fertilizers, or soil characteristics. The term "treatment," for example, might refer to the fertilizers being compared. For training events "treatment" translates to a specific instructional plan. Recently this has been accomplished by developing instructional modules based on specified behavioral objectives with explicit instructional routines. In extreme cases these instructional routines are controlled by programmed texts or scripts to be followed.

In reality most training events cannot be so explicitly planned. The difference between what is planned and what actually occurs is considerable. This difference is both natural and beneficial. Training events are not mechanistic routines to be blindly followed. Real events grow, change, and develop as the human beings involved in the event interact. In fact, it is the actual patterns of interactions, rather than the intended treatments, that are the important features of training events for policy makers, other researchers and replicators.

Effects. The term "effects" refers to yield or end product of a treatment. In education this translates to assessment of performance at the termination of the "treatment." This is now most often accomplished by developing criterion-referenced tests based on the behavioral objectives of the treatment. Such terminal assessment captures only intended performance outcomes not underlying outcomes or unintended outcomes and completely fails to capture what happens before, during or after the event. Furthermore, too often intended performance outcomes are identified only in terms of new concepts to be learned. Such an emphasis on predetermined symbols and their manipulation is inadequate for all but the most trivial of human interactions with new information. It misses the personal meanings of the new information and the integration of these meaning with past (and future) experiences. These "other" outcomes are of essential importance for replication.

Experimental Unit. The term "experimental unit" refers to the soil plots or "the smallest division of the experimental material such that any two units may receive different treatments in the actual experiment" (Cox, 1958, p. 2). In educational training events the "experimental unit" thus should be the population taking part in the event. However, even though most training events are carried out with groups, individual students are often used as the experimental units. The consequences of using this incorrect experimental unit are disastrous with respect to the three assumptions underlying conventional methodology.

Treatment Effects Are Additive. The mathematics of determining experimental effects is based on Equation 1 (Cox, 1958, p. 14).

$$1) \quad y = u + t$$

Equation 1 says that the total quantitative effect of yield (y) after a treatment can be broken down into two subquantities: u--a quantity depending only on the particular experimental unit, and t--a quantity depending only on the treatment used.

There are three immediate consequences of this assumption. The first is the quantifiability of all three terms--y, u and t. While it is true that one mark of a mature science is the possession of sophisticated measurement instruments and techniques, we must admit that at present in education we are not able to quantify with any validity or accuracy many terms in an educational setting. For example, one anticipated treatment effect of CMTI was that the participants would share in a meaningful multi-cultural experience. The photographic essay, "Together," (Tabachnick and Lemes, 1975) was prepared as a means of documenting (but not quantifying) the actuality of that experience.

The second consequence of the additivity assumption allows the possibility for adding or subtracting these treatment effects in an algebraic manner in order to remove the quantity depending upon the experimental unit. If a "control" group is used that does not receive the treatment, then Equation 2 shows the total "effect" measured when no treatment is administered.

$$2)\quad y_c = u$$

Subtraction of Equation 2 from Equation 1 gives an estimate of the effect due only to treatment (see Equation 3).

$$\begin{aligned} y_t &= u + t \\ -\ y_c &= u \\ \hline \end{aligned}$$

$$3)\quad y_t - y_c = t$$

Equation 3 says that one need only have the measurements of the total quantitative effects after treatment (y_t) and for control group (y_c) to estimate the effect due only to treatment (t).

The third consequence of this additive assumption is the possibility of estimating differences between two treatment effects. If measurements are taken after two different treatments, they can be subtracted. The following set of equations shows this algebraic process.

$$\begin{aligned} y_1 &= u + t_1 \qquad \text{(Measurement after Treatment 1)} \\ y_2 &= u + t_2 \qquad \text{(Measurement after Treatment 2)} \\ \hline \end{aligned}$$

$$4)\quad y_1 - y_2 = t_1 - t_2$$

Equation 4 says that the difference between the final measurements after Treatments 1 and 2 ($y_1 - y_2$) can be considered to also be the difference between the two effects due only to Treatments 1 and 2 ($t_1 - t_2$).

Note, however, that Equation 3 and Equation 4 are correct only if the effects depending on the unit (the u's) are equal for different units. These conditions are assumed to be true when there is no systematic bias which differentiates the experimental units. Control of bias is usually accomplished by random assignments to an appropriate experimental design. In fact, true control groups are not often feasible in teacher training situations. Even where alternate treatment groups are possible (Equation 4), they are only helpful in estimation of the program's effect if the treatment effect of the other treatment were known--presumably from another experiment which employed a true control group.

Many investigators use the pretest in the "pretest-posttest" design as a measure of u.

$$y_t = u_t + t$$

$$- y_p = u_p$$

$$5) \quad y_t - y_p = t$$

The resulting equation (5) indicates that the treatment effect (t) is equal to the difference in pretest and posttest scores (gain). Obviously, Equation 5 is valid only if the unit term is the same before and after intervention. However, as Campbell and Stanley (1963, p. 8) so forcefully point out, there are several sources of potential invalidity to this assumption such as history, maturation, instrumentation, test treatment interaction, etc. Investigators simply cannot blindly use gain scores as estimates of treatment effects.

Note that since control groups have been in use, the designs used to evaluate Teacher Corps projects neither actually can be used to estimate program effects.

Also, because of this additivity assumption an evaluator is primarily concerned about the measurement of specific attributes only at the end of treatment. The focus of this concern is on validity, The inferences of the results depend upon the extent to which the measurements are valid. The researcher must defend (or is criticized upon) the various ways in which validity can be established (construct, content and criterion-related validity). Once validity can be established, results are verified.

Unfortunately, while validity of terminal assessment is important, it does not address questions about the identification of the treatment as it involves the patterns of interactions between treatment and subject or the extent to which the effects of one treatment are actually "added upon" the effects of another treatment. In education, not only are these later concerns most crucial to treatment replication but they should also be more open to critical inspection.

Constancy of Treatment Effects. In an attempt to increase the generalizability of the findings most evaluators replicate the training procedure either by using the basic training program in two or more settings, or by assuming each individual subject in a single training setting (every subject is a replicate of every other subject). In either case, one must assume that treatment effects are constant. This assumption says that treatment effect does not change when the treatment is given to two different units. Algebraically, this assumption allows Equation 6 to represent the measured effect after a specific treatment on one unit (u_1), and Equation 7 to represent the measured effect of the same treatment upon a different unit.

6) $y_1 = u_1 + t$

7) $y_2 = u_2 + t$

Since treatment effect is assumed to be the same for these two different units, the subtraction of Equation 7 from Equation 6 states a very important consequence of this assumption (see Equation 8).

8) $y_1 - y_2 = u_1 - u_2$

Equation 8 states that if two different experiemental units (u_1 and u_2) receive the same treatment, then the differences in effect (y_1 and y_2) are only due to differences in units. With this asusmption, when one compares the end of treatment measures, one is also comparing the differences between the experimental units. Note, following this argument, if u_1 and u_2 were identical units, the difference in treatment effects should be zero. However, if aspects of training procedures are adapted (not adopted) for [illegible] with different populations, one would actually anticipate different treatmen effects. That being the case, it would not be possible to arrive at Equati n 7. Thus, the conclusion that one is comparing differences between units when one compares measures at the end of the same treatment is false.

This problem is especially acute when one uses individual students as the experimental unit. As students are exposed to new material we expect them to assimilate those new ideas into their own personal meanings or ideational scaffolding. We expect the same training event to have different effects on different students. Some will assimilate and use lots of new information in one way, others may generate quite different kinds of new information and relationships. Psychologists now are generally agreed that unless the influence of individual difference variables is considered, predicted outcomes of training events will be masked by within-treatment variation. Persons indeed do differ in how they respond to the same information or the same instructional procedures. Thus, the assumption that treatment effects are constant is simply false in many teacher training circumstances.

Lack of Interference of Experimental Units. This assumption says that when more than one experimental unit is used, there is no interference or interaction between the units. This assumption is particularly important if statistical analysis is to be made of the observations since the statistical

are based on an assumption of independence of observations (or unit measurements). If the experimental units really were independent classes, then one might argue lack of interference between the classes. On the other hand, in teacher training events, if students are the units, this assumption is clearly false. Such interference between units is the essential interaction between human beings one expects in dynamic events. In fact, investigators have typically assumed that the treatment effect for a class is simply an aggregate of individual effects. The argument is usually that "we teach pupils, not classes." This simply is not the reality of dynamic events. By assuming no interaction the researchers closes his eyes to the essence of the event.

In summary, the basic assumptions underlying the conventional methods of inquiry are not met by the reality of dynamic training events. One can only conclude that conventional methods of inquiry are not appropriate for the demonstration of dynamic training events.

Process Evaluation: An Alternate Methodology

Identifying the salient features of dynamic events for demonstration will depend upon the extent to which an investigator can capture and communicate those features. Fortunately, there are methods of inquiry which can be adapted to study such events. For example, the problem of evaluating a program within a complex learning milieu, has been addressed by Parlett and Hamilton (1972) who suggest the concept of "illuminative evaluation": an intensive examination of the evaluated program through a variety of strategies in order to help illuminate significant program features. Another example is the work being undertaken by Glass (1975) who is adapting the techniques of time-series analysis to the problems inherent in the examination of treatment effects over time. A third example is the "process development" model for curriculum development and evaluation suggested by Romberg and Hernandez-Nieto (1976) that responds to special evaluation problems inherent in the nonlinear characteristics of instructional processes as well as human development.

What we have done is to combine features from a variety of sources into a dynamic, evolving method of inquriy we are calling "Process Evaluation." "Evaluation" because we are indeed attempting "to judge or determine the worth or quality of" (Webster's New World Dictionary, 1970, p. 484). The referent of evaluation is a "product." For CMTI the process of instruction used in teacher training events, "worth or quality" has been defined in terms of demonstrable features of training events. The term "process" has been used to emphasize the evolutionary processes involved in training events. Such events have two main features which have helped us in developing this alternate methodology--they are time embedded, and they evolve (have a becomingness). Time is important both as a basis for describing training events (events start, progress and end), and as a variable that characterizes events. Becomingness is important since outcomes are not just yield. One must look for intended and unintended outcomes. Thus, multivariate-multilevel information is critical. In addition one must try to capture how these many outcomes grow and evolve over time (including before, during and after the event). Finally, since such events involve humans we must know how they interact--spend time. From these considerations the following notions of design, intervention and observations were used.

Design

The design used in the CMTI Impact Study is "time-series." Campbell and Stanley (1963) drew educators' attention to this design and labeled it "the interrupted time-series quasi-experiment." They also discussed the statistical analysis difficulties associated with that particular design. Since 1963 the utility of the "time-series" design has been endorsed in several textbooks and articles but rarely actually applied to educational research. The reasons for lack of use fall into two categories. First, a misunderstanding, somewhat perpetuated by Campbell and Stanley, that the intervention was necessarily of short duration (such as the effect of passing a law). It can and is most often used in economics to monitor what goes on before, during and after a prolonged intervention. And second, the more important reason for lack of use has been the difficulties with statistical analyses. However, at this time considerable work has been done on time-series analyses. The best summary is by Glass, et.al. (1975) in their book Design and Analysis of Time-Series Experiments. In that work they describe eight different time-series designs. These eight designs are summarized in Table 1.

Insert Table 1 Here

The varieties of intervention effects which could be examined are listed in Table 2.

Insert Table 2 Here

The intention of a time-series design is to monitor ongoing events. The result of this monitoring is to plot a series of points such as the series of points indicated in Figure 1.

Insert Figure 1 Here

This series of points (joined for illustration) shows rankings on the five perspectives on teacher tasks during CMTI. See Figure 2 for a second example from CMTI.

Insert Figure 2 Here

Then one examines the graphs to determine either change in level or change in direction by using graphical or spectural analysis techniques. These are not new techniques. They have been commonly used in engineering and economics for several years. However, to develop data from human beings raises several problems. First, the usual required number of points is too large (spectural analysis is usually done with no less than 200 data points). The second problem is statistical dependence. Human beings being retested have a tendency to remember how they answered the same questions rather than responding independently. And third, very often in education we are interested in multivariate effects rather than univariate effects. To date no multivariate models have been developed for testing effects of time-series studies. Nevertheless, in CMTI we used a combination of a "single group multiple I design" (for the summer training program) and a "stratified multiple group single I design" for their project experience in the field (see Figure 3).

Insert Figure 3 Here

Interventions

These were three interventions: first, the two-week training session on organizational characteristics; second, the two-week training session on instructional techniques; and third, the on-site training in Teacher Corps Projects. These three interventions were not under the control of the CMTI Study Team. In addition, while there was an overall instructional plan, the training events evolved over time in ways which were not completely anticipated by the instructors.

Observations

The observations used in this design were of two types. First, objective assessment via the use of surveys Ideally for an impact study one would develop a multivariate-multilevel objective assessment procedure.

Multivariate, in that several variables would be studied simultaneously and of necessity treated as independent (because of the limitations of the statistical analysis procedures associated with time-series designs). Since it is clear that the set of variables would not be independent the reporting of results must be carefully judged in terms of the overall set of variables. The basic battery that was developed and administered by the CMTI Study Team was multivariate but only at one level. The CMTI survey battery included five instruments: teacher tasks, intern tasks, school success, school failure, and expectations. On each instrument five dependent variables were judged; namely, teaching as personal development, teaching as technique, teaching as curriculum development, teaching as community involvement, and teaching as organizational behavior.

In each of the four surveys the interns were asked first, to rank order each set of five items and second, to determine the value of each of the items to themselves. For example, Figure 4 shows one set of five items from the tasks of an intern survey. Each item in this set is an exemplar of one of the five perspectives. The tasks of an intern survey contains five sets of items similar to the set given below. Each survey was then administered several times.

Insert Figure 4 Here

Multilevel would mean that the data should be gathered at more than one level. An initial level would include assessment related to the objectives of instruction (usually the concepts and skills one would expect to master during instruction). At subsequent levels underlying effects would be probed.

The CMTI Study Team did not collect objective assessment data. The instructors of both initial intervention did collect that information. In future designs, it would be advantageous if systematic data were gathered at this intended outcome level. The instruments developed by the CMTI staff were intended to measure a second level of socialization to teaching.

To reduce the statistical dependence of observations and to reduce the amount of testing time involved in this study, matrix sampling was followed to gather the data. A small number of students at each observation time were administered a portion of the total battery. Estimates of population characteristics were derived from those subpopulations.

The second part of our data collection scheme was to gather subjective assessment data in order to give meaning to the objective data. The objective data alone simply yields numbers (points on a graph) but fails to give a clear understanding of the salient features of the program. Thus, observations, interviews, and photographs were used to give meaning to the data. The procedures used were adapted from the field study techniques of anthropology-sociology. Communicating the results from this study involves knowing the interventions, how they evolved, and summarizing the objective and subjective data. From this rich data base it is anticipated that consistent patterns can be reasonably identified and communicated to others.

Summary

There are methods of inquiry which can be adapted to the study of teacher training events which if used would capture some of the reality of such events. However, the methods are not easy to use. We are not claiming we have found any panacea to the problems of assessing and evaluating teacher training programs. What we are claiming, however, is the need to expand one's conceptualization of evaluation for such events to the identification of salient features which can be demonstrated. We do believe that the combination of a multivariate, multilevel data gathering approach, a time-series design and field study procedures involving observation, interview, and photography, etc., are useful as a collaborative technique for identifying the important characteristics of teaching training events.

A

Single-Group—Multiple-I

O O O I_1 O O O I_2 O O O

B

Multiple-Group—Single-I

O O O I_1 O O O
O O O I_1 O O O
O O O I_1 O O O

C

Multiple-Group—Multiple-I

O O O I_1 O O O
O O O I_2 O O O
O O O I_3 O O O

D

"Reversal" Design

O O O I_1 O O O I_2 O O O
O O O I_2 O O O I_1 O O O

E

"Operant" Design

O O O I_1 O I_1 O I_1 O O O O I_1 O I_1 O I_1 O

F

"Interaction" Design

O O O I_1 O O O I_2 O O O I_1, I_2 O O O

G

Sequential Multiple-Group—Multiple-I

O O O I_1 O O O
O O O I_2 O O O

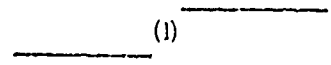
H

"Stratified" Multiple-Group—Single-I

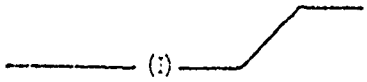
Type A Units: O O O I_1 O O O
Type B Units: O O O I_1 O O O
Type C Units: O O O I_1 O O O

Table 1. Variations on the basic time-series experimental design. (Glass, et.al., 1975, p. 20)

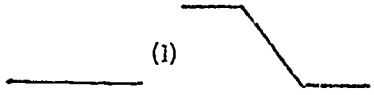
A. Abrupt change in level.



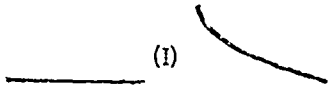
B. Delayed change in level.



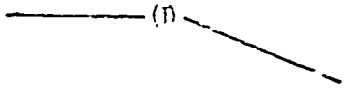
C. Temporary change in level.



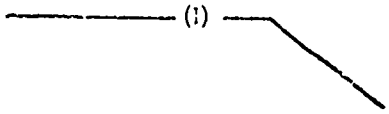
D. Decaying change in level.



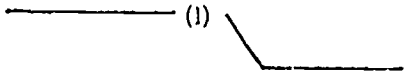
E. Abrupt change in direction.



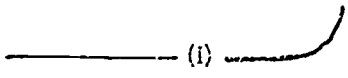
F. Delayed change in direction.



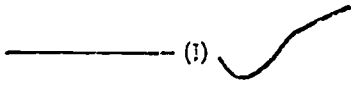
G. Temporary change in direction.



H. Accelerated change in direction.



I. "Evolutionary operations" effect.



J. Change in variability.

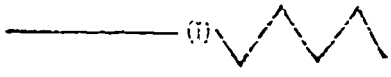


Table 2. Varieties of intervention effects in the time-series experiment. (Glass, et.al., 1975, p. 44)

Fig. 1. Summary of Mean Rank of Five Perspectives on Teacher Tasks

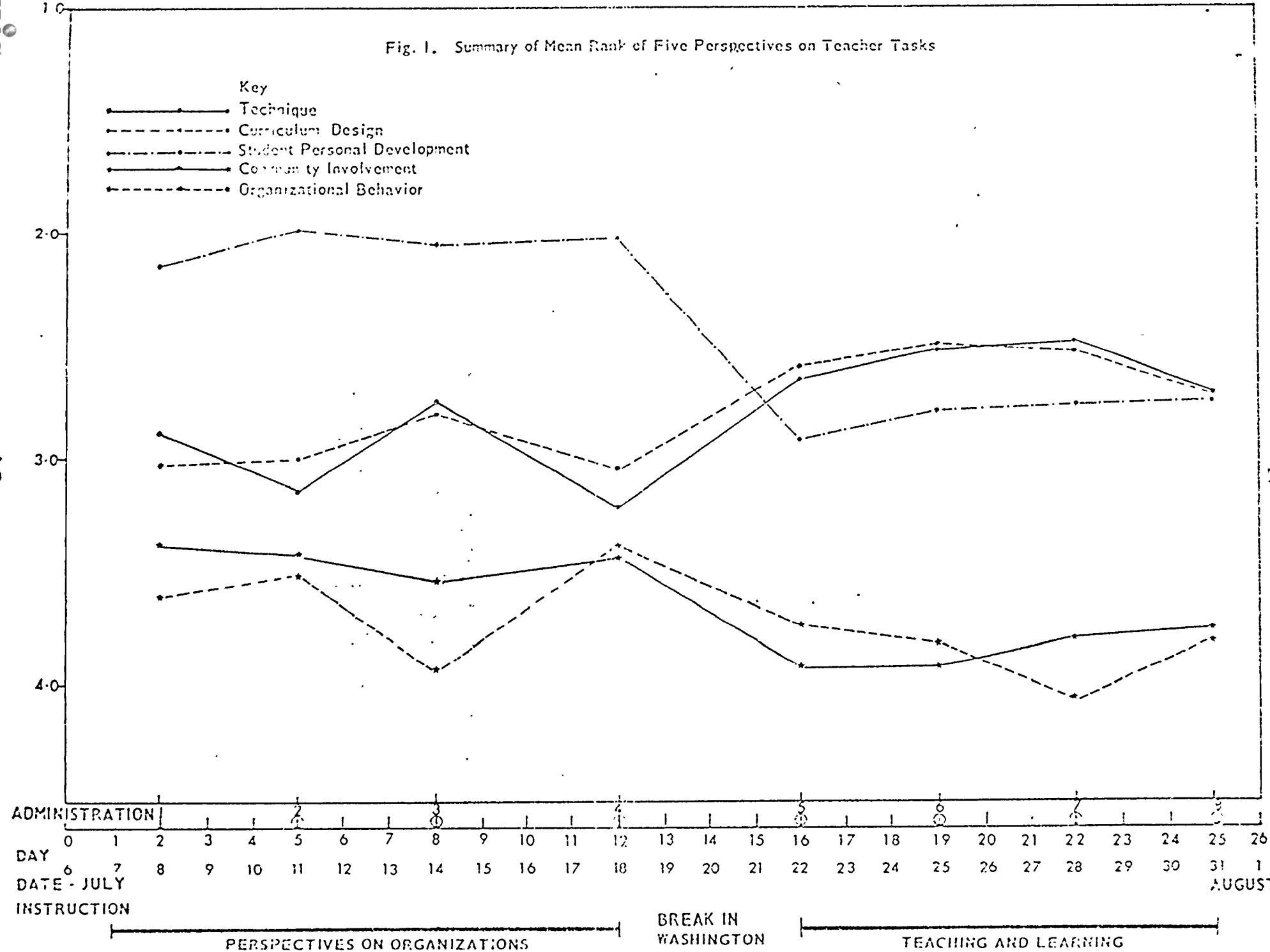
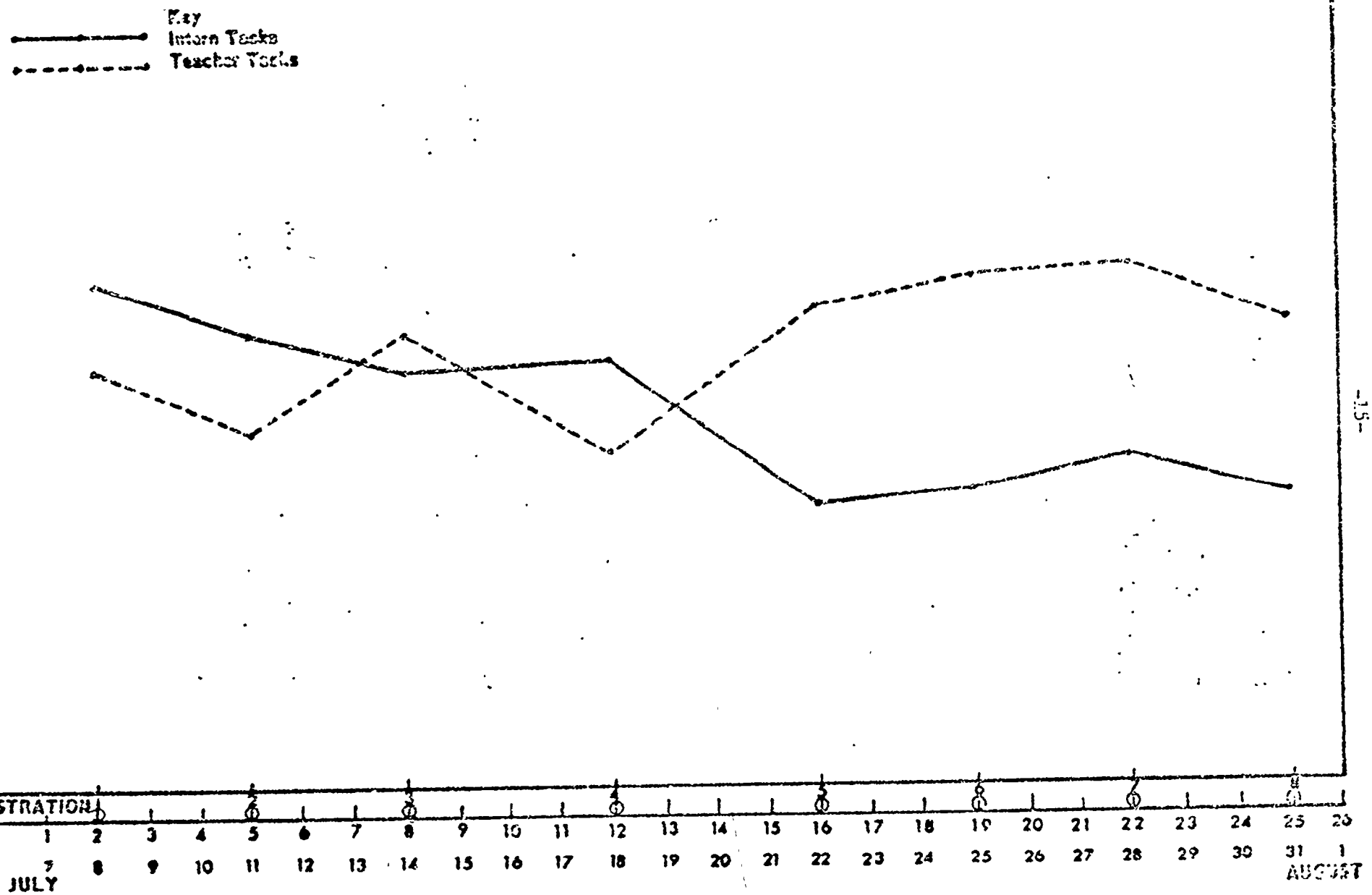


Fig. 8. Summary of Mean Rank of Teaching on Techniques



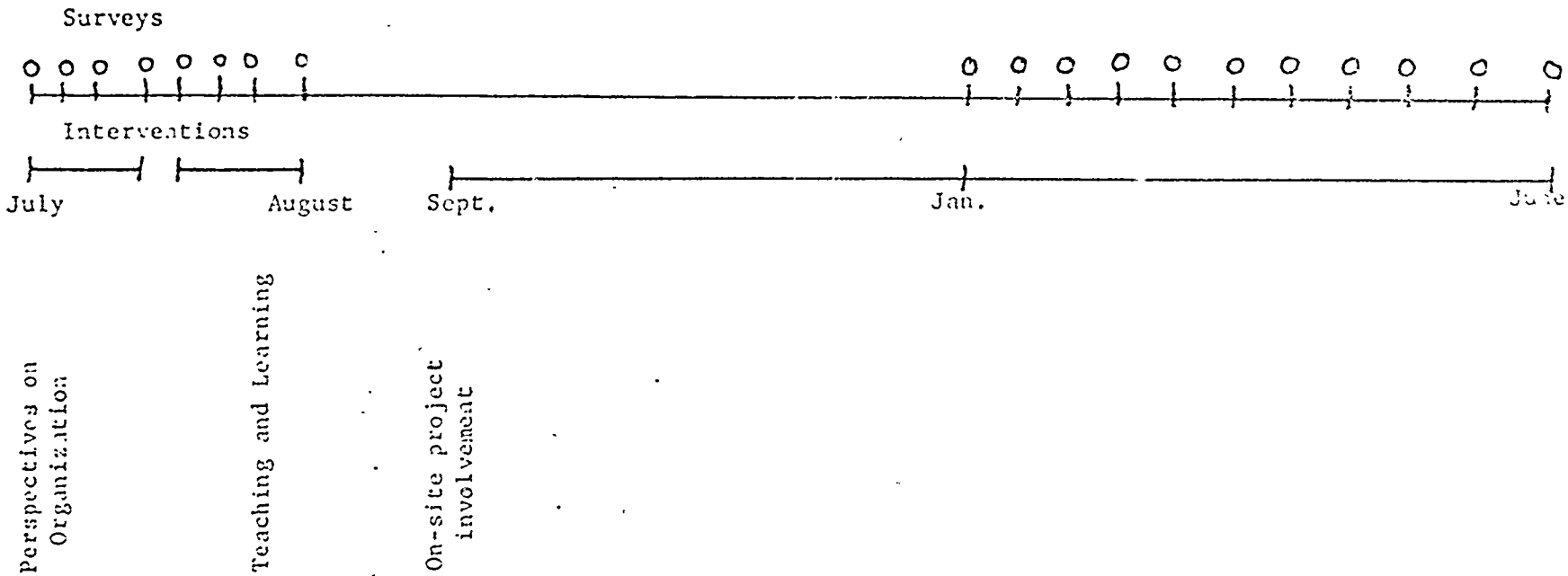


Figure 3. Sequence of Interventions and Survey Administrations for CMTI and its Follow-up.

Figure 4. Sample of one set of items for Intern Task Survey.

IV. My tasks as an INTERN are to learn how to:	Rank Order	Value-to-me
		high low
a. Extend school programs into the community. . .	_____	○○○○○
b. Continually evaluate the implications of schooling as an organizational enterprise. . .	_____	high low ○○○○○
c. Design basic skills materials which are particularly appropriate for low-income communities	_____	high low ○○○○○
d. Teach children reading comprehension skills. .	_____	high low ○○○○○
e. Be open, warm and sympathetic to my students..	_____	high low ○○○○○

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