

**COMMUNITY-LEVEL NUTRITION INTERVENTIONS
AN ARGUMENT FOR REFLECTION-IN-ACTION**

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ABSTRACT

After analyzing longitudinal anthropometric data from eight community-level nutrition programs to determine their impact, it was concluded that conventional approaches to analysis do not eliminate indeterminacy because: 1) the data was inaccurate or inconsistent, 2) the measures or measurement methods produced misleading results, and, most importantly, 3) a lack of information about the local context of the interventions precluded the elimination of competing explanations of observed outcomes. In that analysis, as with most similar analyses, the traditional experimental approach (applying a predesigned experiment using controls in a presumably constant environment) failed because the experimental context was unstable, unpredictable and unique in each case. Furthermore, the instability, unpredictability and uniqueness of each case called for a flexible intervention strategy to cope with the changing context.

As an alternative approach to both analysis and intervention, reflection-in-action is proposed. Six features of this model are: explicit specification of the framework underlying the intervention strategy, continuous monitoring of both data gathering procedures and intervention strategies, periodic redesign of those procedures and strategies, collaboration between researchers, practitioners and subjects throughout, use of on-the-spot experimentation to test particular hypotheses and, explicit enumeration and accounting for potential factors confounding both the analysis and the intervention itself. By actively using the data for continuous monitoring, field practitioners, working with analytic specialists, are more likely to reduce or eliminate indeterminacy due to inaccurate data and/or contextual changes than would traditional researchers or evaluators who maintain distance between themselves and the intervention.

Reflection-in-action, in part, is illustrated in the context of a recent evaluation conducted in Sri Lanka where a revisit to the field with preliminary quantitative results caused modification in the interpretation of those results. Problems remain, however, in achieving full implementation of this approach. Practitioners are reluctant to commit scarce resources to data collection and analysis, researchers are reluctant to sacrifice "objectivity" by involving field people in their analysis, and institutions are unable to support projects which may vary considerably in nature and in resource requirements over time.

TABLE OF CONTENTS

ABSTRACT i

INTRODUCTION 1

FINDINGS FROM AN ANALYSIS OF COMMUNITY-LEVEL
NUTRITION PROGRAMS 4

THE MODEL OF RIGOROUS EXPERIMENT AND ITS
SHORTCOMINGS 9

THE CONDITIONS OF EFFECTIVE COMMUNITY-LEVEL
INTERVENTION 19

COMMUNITY-LEVEL NUTRITION INTERVENTION AS
REFLECTION-IN-ACTION 22

EXPERIMENTAL NORMS 29

LEARNING FROM EXAMPLES OF COMMUNITY INTERVENTION 53

PROBLEMS OF IMPLEMENTING REFLECTION-IN-ACTION 62

IMPLICATIONS OF OUR ARGUMENTS FOR THE BROADER
QUESTION OF INTERVENTION ORIENTED SOCIAL EXPERIMENTS 68

REFERENCES 76

LIST OF FIGURES

1. THE MODELS OF EXPERIMENT COMPARED	30
2. EVALUATION STUDY DESIGN CHARACTERISTICS	60

INTRODUCTION

In the analysis of social intervention, three questions are of paramount importance:

- 1) is a particular intervention having the desired effect,
- 2) how can any given intervention be improved in design and/or implementation, and
- 3) can the lessons of a single attempt at intervention be translated into better program designs in the future?

In short, can we learn from ongoing social interventions.

This paper describes an approach to both the analysis and conduct of social interventions calculated to generate satisfactory responses to these questions. Called reflection-in-action, this approach is predicated on the rapid and frequent analysis of both impact and process data during the course of an intervention. The analysis is used in the field to signal the need for mid-course corrections and to assess any changes resulting from those corrections. Due to the constantly changing contextual background for any major social intervention, the interpretation of the results of quantitative assessments of program progress must be done jointly by experienced analysts and by practitioners with intimate knowledge of the conditions in the field during the intervention.

The argument for reflection-in-action grew out of six years of "hands-on" experience in the analysis and evaluation of community-level health/nutrition interventions as well as larger scale government programs featuring

service delivery at the local level. Thus, the emphasis of the paper will be on the health/nutrition field; however, the argument for reflection-in-action is easily translated into other arenas of social intervention. (In fact, none of the authors is a specialist in health or nutrition. They share a common interest in the application of sound analytic methodology in the social sciences--an interest that has only recently focused on development problems in the third world.)

To present the argument for reflection-in-action, we will summarize the major findings from our continuing efforts to understand the dynamics of the program design, implementation and evaluation processes in the health and nutrition fields in the developing world. We will then describe the more conventional approach to studying social interventions--viewing them as rigorous experiments--and describe how poorly the underlying assumptions of this approach fare in the "real" world. By way of contrast, we introduce the notion of reflection-in-action and describe the primary features of this approach in considerable detail.

Given that the most serious drawback to the conventional approach of viewing intervention as rigorous experiment is the indeterminacy of analytic results, we then turn to the ways that reflection-in-action alleviates some of the causes of this indeterminacy. Moving from the theoretical, we follow this discussion with an illustration

June 17, 1982

REFLECTION-IN-ACTION

of the operation of reflection-in-action (at least, some elements of reflection-in-action) in the context of an ongoing nutrition intervention in Sri Lanka. Taking the pragmatist's view, we will then consider the obstacles to reflection-in-action, obstacles arising from the individuals involved as well as institutional barriers to implementing such an approach. Finally, we will look at the implications of reflection-in-action on the broader question of intervention oriented social experiments in general.

FINDINGS FROM AN ANALYSIS OF
COMMUNITY-LEVEL NUTRITION PROGRAMS

We have analyzed a large sample of community-level nutrition interventions in order to learn from them how to conduct more effective interventions of this sort. We considered the interventions as experiments aimed at improving the nutritional status of their target populations, often preschool children between the ages of zero and six. Employing conventional measures of malnourishment, we attempted first to determine whether a change in nutritional status had occurred in the course of the project's duration. We compared measurements of nutritional status in the target population before, during and after the period of intervention. Second, where a change in nutritional status could be detected, we attempted to attribute it to the intervention. This we did by formulating plausible optional explanations of the change, seeking then to discriminate among these explanations on the basis of available data. In short, we attempted to apply a widely accepted model of interventions as social experiments to the particular domain of community-level nutrition intervention.

Of the roughly 25 projects initially considered, only eight approximated the minimal criteria necessary for treatment of them as experimental interventions. One criterion which was especially decisive was the availability of time-series data on nutritional status. In addition, a few such projects were already reported in the literature

and could be considered along with the sample we had chosen for field evaluation.

In only one case did those responsible for the intervention view their effort as an experiment in the traditional scientific sense. (By traditional scientific sense, we mean as a test of a specific hypothesis through a careful application of treatment according to a pre-defined experimental design using appropriate controls.) In all other cases, the activities were first and foremost an effort to provide help to those in need. Learning from the effort was always important to the change agents but, except for the one case, the primary motivation behind each intervention design was to maximize program impact rather than to facilitate learning. Thus the interventions reviewed feature diverse philosophies and varied commitment to the collection and analysis of quantitative data to promote learning.

Because we refer repeatedly to the eight interventions by name in the following text, we provide a brief description of each here. A more complete description of each program, the data available and the findings of the analysis is included as Appendix A.

- 1) Candelaria--A program featuring home visitations by volunteer "barefoot doctors" in the town of Candelaria, Colombia. Services include education on nutrition, hygiene and the utilization of health services; weighing of children; and referrals to the medical establishment.
- 2) Candelaria Revisited--A resurvey of the village of Candelaria two years after termination of the program.

- 3) Primops--An extension of the Candelaria concept to an urban setting in Cali, Colombia.
- 4) Esperanca--A program based on the establishment of health posts in rural villages in the Central Amazon region of Brazil. Services include the provision of basic health care by a visiting doctor and/or a locally trained "barefoot doctor", nutrition and health education, and child weighings.
- 5) SCF-Honduras--A program following the Save The Children community development scheme, CBIRD, carried out in the Pespire region of Honduras. Food supplements were administered in parallel with more general development efforts.
- 6) SCF-Indonesia--Another Save The Children program, this time in the special province of Aceh in northern Sumatra.
- 7) Thailand--An experiment to test the effect of rice fortification on nutrition and health in the Chaing Mai region of Thailand.
- 8) Kottar--A community development oriented program run by the Kottar Social Services Society in Tamil Nadu, India. Services include food supplementation, nutrition and health education, immunizations and more general community development assistance.

The results of our analysis can be summarized as follows:

- 1) In only two of the projects was it possible to establish, with reliability, that a positive change in nutritional status had taken place in the target population (that is, a change clearly free of noise in the data, and independent of changes in the make-up or aging of the target group).
- 2) In none of these cases was it possible to attribute change unambiguously to the intervention: on the basis of available data, we could not discriminate among some plausible competing explanations.
- 3) Although the projects analyzed varied greatly concerning the extent to which they were designed and carried out as rigorous field experiments (according to a model which we will later describe and call "the model of rigorous

experiment"), the indeterminacy of the data was as great in the more rigorously designed as in the less rigorously designed projects.

We were able to identify three main sources of indeterminacy in the data:

- 1) Practices of data-collection, storage and processing which produced inaccurate or inconsistent data. (We shall call this "dirty data".)
- 2) Measures and measurement methods which produced misleading results. (We shall call this "inadequate measurement".)
- 3) A lack of timely, situation-specific data which would have permitted discrimination among competing explanations of change in nutritional status, given settings that were in many respects complex, unique and rapidly changing. (We shall call this "inadequate data".)

We found that it was possible by adopting certain techniques of data cleaning, processing and analysis, to reduce some of these sources of indeterminacy. By performing tests with regard not only to the range assumed by relevant variables but also their internal consistency within and between observations in a longitudinal sequence, we were able to eliminate many, if not most, mechanical errors without discarding valuable data. By exploring a variety of analytic techniques to test alternative theoretical propositions, we could quantify the effect of some confounding variables, eliminating some of the sources of indeterminacy. In this way, we were able to identify reliable patterns of change in two projects, and were able to eliminate some contending explanations of change in some others. But in other cases, and for some plausible

competing explanations in all cases, it was not possible to arrive at an unambiguous explanation of the data.

For example, in the analysis of the data from Kottar, we found obvious keypunching errors in 40% of the anthropometric data. Once we cleaned the data, we were able to convince ourselves that measurable, positive, nutritional change had occurred only after experimenting with a locally generated growth standard for Indian children. The recovery was not as rapid as that called for by international standards but was apparent when viewed with a less ambitious expectation as set by a local standard. We were able to eliminate the possibility that the observed change was due to a bias in the selection of new participants over time but we were unable to refute the possibility that the change was induced, in part, by the reduction of rice prices as a highly localized drought ended.

Reflection on this experience strongly suggests lessons that are equally important for two objectives--the conduct of informative community-level nutrition experiments, and the conduct of effective community-level nutrition interventions. And further, our experience leads us to question what we take to be the prevailing model of the proper relation of experimental research to the practice of community-level intervention.

THE MODEL OF RIGOROUS EXPERIMENT AND ITS SHORTCOMINGS

The most common approach to learning from community-level nutrition interventions is based on techniques which have proven successful in several of the natural sciences--an approach we call the model of rigorous experiment. An essential element of the model of rigorous experiment is good experimental design. As a discipline within the field of statistics, experimental design grew out of the need to sort out the variations in certain variables due to some specific action or event from the variations which normally occur in those variables. As Cox explains in his introduction to the subject,

...as soon as the effects under investigation become comparable with the uncontrolled variations, the problems we shall be concerned with become important.'

As originally conceived, statistically efficient experimental design becomes possible when the researcher can control the assignment of experimental units (the recipients of some sort of treatment; for example, preschool children, mice, or plots of land) to different treatment groups. By appropriate randomization in such assignments, it becomes possible to eliminate most, if not all, of the potential explanations for changes unique to the treated group other than the desired explanation that the treatment itself caused the changes. Such control of the assignment of experimental units to treatment groups is most easily

'D. R. Cox, Planning Of Experiments (New York: John Wiley & Sons, 1958), p. 3.

achieved in laboratory settings. In many social settings, such control is impossible.

As social scientists began to grapple with situations calling for experimental verification of hypotheses that were not conducive to the neat random assignment procedures available in the laboratory, the concepts of experimental design were extended to include quasi-experimental designs. These are more complex than the true experimental designs and less able to eliminate all competing explanations. Campbell and Stanley, whose work has contributed most to the development of quasi-experimental research in the social sciences write:

Insofar as the designs discussed in the present chapter become complex, it is because of the intransigency of the environment: because, that is, of the experimenter's lack of complete control.²

In their landmark publication, Campbell and Stanley itemize eight threats to internal validity of quasi-experimental designs and another four threats to external validity. Internal validity pertains to the question of whether the treatments considered in a particular experiment truly make a difference in that experiment. External validity pertains to the question of generalizability; that is, to the question of the degree to which the observed results carry over to other settings. Their discussion shows that even the most complex quasi-experimental designs

²Donald T. Campbell and J. C. Stanley, Experimental And Quasi-Experimental Designs For Research (Chicago: Rand McNally, 1966), p. 1.

are susceptible to challenge from, at least, some of these threats to validity.

In the nutrition field, the response thus far to difficulties encountered in applying quasi-experimental design concepts in research and/or evaluation research on nutrition interventions has been to try for better controls or more complicated experimental designs. Jean-Pierre Habicht and William P. Butz write:

Therefore, any experimental design which does not randomly distribute the intervention and its controls within a village or a region must have sufficient villages or regions covered by each treatment (replicates) so that one can estimate the probable contribution of non-specific influences at the village or regional level. Adjacent villages and regions must have different treatments, and the villages and regions should be so stratified that any other random non-specific influences are controlled for. Designs which show differences between regions or villages but do not have these required replicates must remain suspect.³

We believe that it is not possible to remove several sources of indeterminacy (eliminate the threats to validity), of the sorts which we have encountered, by the more exacting application of the model of rigorous experiment and analysis. To help explain our position, let us consider four features of that model. The first three arise because of the need to retain rigor (to look like a laboratory) in the quasi-experimental design process. The

³Jean-Pierre Habicht and William P. Butz, "Measurement Of Health And Nutrition Effects Of Large-Scale Nutrition Intervention Projects," in Evaluating the Impact of Nutrition and Health Programs, ed., Robert E. Klein et al. (New York: Plenum Press, 1979), p. 150.

fourth is added not so much for its role in establishing experimental rigor but for its role in establishing the objectivity of the analysis.

1. Controls: Ambiguous experimental data should be eliminated by the use of experimental controls. In the quasi-experimental situations persisting in most social science settings, these controls must meet two criteria. First, control groups should be established which are like the experimental group in all respects except for the presence of the intervention. Second, both the experimental (treatment) and control groups should be insulated from all changes in the situation other than changes induced by the intervention. Otherwise, it would not be possible to attribute outcomes (observed results) unambiguously to the intervention. When such insulation is not feasible, it is possible to apply assorted multivariate statistical techniques to account for their contribution to change. (The use of statistical techniques to account for variables confounding an experimental design is often called "the statistical control" model. With regard to the analysis of social interventions, statistical controls are often used in conjunction with experimental controls.⁴)

2. Pre-design of experimental conditions: The identification of matched control groups and mechanisms for

⁴K. M. Hennigan, B. R. Flay and R. A. Haag, "Clarification Of Concepts And Terms Commonly Used In Evaluative Research," in Evaluating the Impact Of Nutrition and Health Programs, ed., Robert E. Klein et al. (New York: Plenum Press, 1979), p. 393.

insulating both control and treatment groups from different external change inducing forces requires that potential sources of ambiguous data (confounding factors) be identified before the experiment is actually initiated. Otherwise, how would it be possible to take account of such factors in the design of controls? (Even in the case where one intends to apply statistical controls, failure to identify confounding factors in advance often precludes their inclusion in the analysis because data on those factors was not collected.)

3. Constancy of experimental conditions: Experimental conditions, including the intervention itself, should be held constant for the duration of the experiment. For if experimental conditions change in the course of the experiment, how will it be possible to attribute outcomes unambiguously to an intervention?

4. Separation of practice from research: Especially as regards evaluation research, it is often argued that those who deliver the intervention and collect the data should not be a party to the research design and analysis, for otherwise they are likely to bias their observations or interventions. In discussing nutrition evaluation, James E. Austin writes:

One should operate under the premise that the credibility quotient of inside evaluations is low.⁵

⁵James E. Austin, "The Perilous Journey of Nutrition Evaluation," American Journal of Clinical Nutrition 31 (December 1978): 2327

The basic premise, of course, is that objectivity of observations and analysis is to be achieved by maintaining distance between researchers and practitioners. (We call this the principle of "research distance".)

The same is true of the relations between researchers and the subjects of intervention experiment. There must be separation and distance here, as well, for otherwise the subjects might alter their behavior in response to their knowledge of the intentions of the intervenors.

In practice, however, experiments with these characteristics cannot be attained reliably in the actual environment of community-level interventions. With respect to interventions in the nutrition area, this is especially so because of the long time-frame assumed by most experiments and the high degree of interaction between nutrition in a community and the broader economic, social and political context. More importantly, even if such characteristics were attainable, they would actually inhibit the unambiguous analysis of the experimental data.

To see this, let us first consider four reasons for the breakdown of experimentation in social settings. We will illustrate each reason with a single example from our own work, although, to be sure, we could provide many more similar examples, not only from our own experience but also from the literature.

1. Instability of the experimental context: Because the experimental situation is likely to be continually changing,

often at a rate faster than the time-frame of the experiment, it is often impossible to hold experimental conditions constant for either experimental or control groups. In our informal discussions with Dr. Gershoff, one of the principles in the Thailand rice fortification experiment, he referred to the near breakdown of the normal rice market due to price differentials across the Laotian border resulting from the military disruptions in the Far East in the early seventies. Gershoff suspected that the fortified rice supplied in Chaing Mai might have been diverted to the blackmarket for cash.

2. Unpredictability: Because the changes in context are unlikely to be wholly predictable at the time of experimental design, it is not possible to build anticipation of all confounding factors into the design of the experiment. In only a year's time in Indonesia, the Save The Children staff reported major flooding, a cholera outbreak and a military uprising which disrupted the flow of services to the recipient population. Curiously, the occurrence of such events was characterized as typical but unpredictable in the remote region of Tangse in northern Sumatra.

3. Uniqueness: Each experimental situation is likely to be unique in some important respects. As a result, it is not possible to decide reliably ahead of time (according to some available model of community-level intervention) what interventions will be effective in improving nutritional

status or (as above) what confounding factors are likely to produce significant changes in the experimental context. Probably the single most distinctive characteristic of community-level nutrition interventions is the personality of the change agent and/or the organization he/she represents. Father James and Sister Lieve, in Kottar, are not easily duplicated nor is their philosophy of upgrading the overall ability of their beneficiaries in dealing with their government and the harsh realities of the local environment.

4. Local information systems: Those most likely to be able to detect confounding phenomena, and to offer plausible interpretations of ambiguous data, are precisely those closest to and most knowledgeable about, the experimental situation--the practitioners and the subjects of intervention. In Honduras, we observed in the data that later additions to the roles of food supplement recipients were nutritionally better off than the original recipients. It was only after speaking with the Save The Children representatives that we learned of a policy, enforced by the government assigned medical doctor for the region, to provide food only to moderately malnourished children.

From a methodological standpoint, at issue is the degree to which instability, unpredictability and uniqueness eviscerate any effort to apply the model of rigorous experiment to a community-level nutrition intervention. Our position is that communities in the developing world, unlike

June 17, 1982

REFLECTION-IN-ACTION

communities in the United States, have too little control over their own "nutrition system" to be anything but unstable, unpredictable and unique when it comes to nutrition. Consequently, efforts at rigorous experimentation often (perhaps, always) end up with results qualified by statements like, "but due to the occurrence of certain events, the experiment was inconclusive." Furthermore, the need to sustain experimental rigor keeps the analyst from adapting to the confounding events by modifying the research design and/or data collection system to compensate for those events.

In our experience, failure to utilize the local information system further eviscerates studies based on the model of rigorous experiment. In most analytic reviews of nutrition interventions, the data is collected and recorded by local people, in many cases by volunteers or low-skilled, low-pay village workers. The quality of the data, and therefore the confidence in the analysis, is directly related to the perception held by those workers of the usefulness of the data. A byproduct of the principle of research distance as applied to the model of rigorous experiment is that the data gatherers see no immediate use for the data and, furthermore, are not told how the data will be used (to insure objectivity). Consequently, they view the data gathering task as an unpleasant burden and the quality of the data suffers accordingly. (In Primops, we saw the percentage of observations with variables outside

June 17, 1982

REFLECTION-IN-ACTION

pre-defined ranges rise steadily with each new batch of data from a low of 6.9% to a high of 24.1%. We must assume that lack of interest in the data contributed to this sloppiness.)

THE CONDITIONS OF EFFECTIVE COMMUNITY-LEVEL INTERVENTION

The four factors which tend to prevent effective implementation of the model of rigorous experiment also constrain the design and delivery of effective community-level interventions. That is, the factors which inhibit informative experimental research also inhibit effective practice.

•Because each community situation is unique, it is impossible to predict ahead of time what type of intervention is likely to be effective in achieving a positive change in nutritional status. The features of effective interventions must be discovered in and for the particular situation. That is, the design of effective intervention must be arrived at through situation-specific inquiry: it cannot be taken, with reliability, "off the shelf" (although it may have features of intervention-types familiar through earlier practice).

Hence, all of the constraints on experimental inquiry, listed above, apply to the inquiry aimed primarily at effective intervention. In other words, an effective nutrition intervention at the community level must take the form of on-the-spot, context specific experimental inquiry.

•Because the community situation is likely to be unstable, no intervention--however effective it may be initially--is likely to be effective for very long. If an intervention is to be effective over the long term, it must be continually revised in response to detection of changes

June 17, 1982

REFLECTION-IN-ACTION

in the situation--including changes triggered by the intervention itself.

•In the design and redesign of effective interventions (as in the design and redesign of informative experiments), much will depend on the site-specific knowledge of practitioners and subjects who are closest to the data and most familiar with the context. It is they who will be in the best position to suggest explanations of surprising outcomes, detect unanticipated changes and identify (and correct) sources of error in data-collection.

In short, effective community-level nutrition interventions are likely to be ones in which intervention is treated as experimental inquiry--but experimental inquiry of a particular sort. One might describe this sort of inquiry in several ways, depending on the features chosen for attention. It might be called "contextual" inquiry, if we focus on the fact that inquiry must address itself to the unique characteristics of the particular site. We might call it "on-the-spot experiment", if we focus on conduct of derived experiments in response to sources of indeterminacy discovered in the course of inquiry. We might call it "rapid information feedback", if we focus on the on-line use of data to inform the redesign of measures, data-gathering procedures, and interventions. The term, "reflection-in-action", calls attention to the fact that inquirers must continually re-examine, in the midst of action, their framing of the problem of malnourishment, their models of

June 17, 1982

REFLECTION-IN-ACTION

the phenomena associated with malnourishment, and their strategies of intervention. Reflection-in-action names the comprehensive process by which inquirers respond to the detection of surprising outcomes by surfacing, criticizing, restructuring and testing the context-specific frames, theories and strategies which they have brought to the situation.

But reflection-in-action refers also to the model of field research which answers best to the four constraints on rigorous experiment.

COMMUNITY-LEVEL NUTRITION
INTERVENTION AS REFLECTION-IN-ACTION

Given the weakness of the model of rigorous experiment in social settings and given also the conditions of effective community-level intervention, we conclude:

- 1) in community situations in developing countries, instability, uniqueness, unpredictability, and dependence on local information systems, prevent application of the model of rigorous experiment and call for reflection-in-action;
- 2) in community situations of this kind, reflection-in-action is equally appropriate as a model of experimental research and as a model of effective intervention; and
- 3) given a model of intervention and experiment as reflection-in-action, it is necessary to revise the prevailing view of the proper relation of research to practice from one advocating separation of the two to one urging their mutual support.

This third conclusion most strongly distinguishes reflection-in-action from more conventional approaches to research. In most conventional approaches, the context of experimental research is taken to be distinct from the context of intervention. The function of experimental research is taken to be the production of reliable general propositions as guides to future intervention. Finally, in order to insure objectivity, researchers must remain distant from practitioners and subjects of intervention. We propose, on the contrary, that intervention-oriented research can be carried out effectively only through actual intervention in particular communities: that such research cannot produce reliable generalizations strictly applicable to types of community situations in which it is

undertaken': and that practitioners and subjects of intervention ought to be involved in the practice of intervention. Practice should be carried out by practitioner-researchers and research-practitioners.

According to the model of reflection-in-action, a community-level nutrition intervention would take on the following distinctive features--each being a divergence from more conventional approaches to intervention research and practice.

1. Explicit Specification Of The Framework Underlying The Intervention Strategy. Initially, description of the situation (context), framing of the problem(s) of malnourishment in that situation, and design of an initial strategy of intervention would be undertaken. This would be viewed as only a first step in the generation of experimental inquiry. One might call the description, frame and design a "model of the local nutrition system." This model serves not only as a basis for initiating intervention but also as the yardstick with which progress can be measured and the best guess at the factors most likely to interact with and confound the intervention strategy.

One would expect fewer "straight-feeding" nutrition interventions if such models were developed prior to

'In a later section, we will soften our strict separation of rigorous research from reflection-in-action by describing ways in which rigorous research supports reflection-in-action and vice versa. We will also show how reflection-in-action in one setting may contribute to future applications in other settings.

initiation of the interventions. By taking into account, explicitly, the relationship between consumption, disease and the economic behavior of a beneficiary population, change agents would be more inclined to integrate supplementary feeding with other services from the outset.

2. Continuous Monitoring Of Both Data Gathering Procedures And Intervention Strategies. There would be continuous monitoring of both the data gathering procedures and the trends revealed in the data through analysis. This would facilitate the redesign of field procedures, measures and measurement methods and facilitate clarification of the meaning of trends in nutritional status (and other indicators of success) through discrimination among competing explanations of those trends.

Data gathering procedures are notoriously inconsistent in the field. Sloppiness in those procedures can cause far larger changes in impact indicators than one would expect from any intervention. For example, it can be shown that a failure to "zero" a scale properly causing a consistent over statement of weight by as little as .3 kilogram can cause as many as 15% or more of a group of malnourished children to appear normal when malnutrition is defined by a weight for height measure.⁷ Monitoring data gathering procedures would make it possible to minimize the disturbance due to

⁷We added .3 kilogram to the weights of 579 school aged children from the Thailand data set and changed the percent children deemed normal by 12% using a weight for height measure and the NCHS-CDC standard.

faulty data gathering. Similarly, early identification of trends in the data makes it possible to recognize alternative causes for those trends soon enough to account for them or, at least, begin to measure them.

3. Periodic Redesign Of Data Gathering Procedures And Intervention Strategies. As a result of the periodic review of the data analysis, data gathering procedures (including what data is to be gathered) would be redesigned, the intervention itself might be altered, and the experimental conditions (e.g. controls) might be redefined. In other words, reflection-in-action attempts to cope with change by reacting to it rather than trying to eliminate it.

4. Collaboration Between Researchers, Practitioners And Subjects Throughout. Once again, we emphasize the need to involve all parties to an intervention in the continuous redesign process. This is likely to serve as a feasibility check for all proposed changes, as an incentive for rapid adoption of those changes and, most importantly, a review of the accuracy of the interpretation of the analysis leading to change.

5. Application Of Disaggregation Techniques And Other Forms Of Derived Experimentation. Whereas conventional analysis in the area of nutrition intervention has stressed the testing of hypotheses regarding change in large groups, reflection-in-action would encourage researchers to discriminate among competing explanations of change by using strategies of disaggregation. In describing his own work

with behavioral testing in the field of psychology, B. F. Skinner makes an eloquent plea for targeting on relevant variables through thorough examination of the response of fewer subjects to treatment rather than more.

Suppose that measurements have been made on two groups of subjects differing in some detail of experimental treatment. Means and standard deviations for the two groups are determined, and any difference due to treatment is evaluated. If the difference is in the expected direction but is not statistically significant, the almost universal recommendation would be to study larger groups. But our experience with practical control suggests that we may reduce the troublesome variability by changing the conditions of the experiment. By discovering, elaborating and fully exploiting every relevant variable, we may eliminate in advance of measurement the individual differences which obscure the difference under analysis. This will achieve the same result as increasing the size of groups, and it will almost certainly yield a bonus in the discovery of new variables which would not have been identified in the statistical treatment.*

By disaggregating to account for confounding factors, the experimenter, in effect, is designing a derived experiment' which differentiates among parts of the target population and parts of the experimental context. For example, if conventional statistical analysis shows little or no change in nutritional status in response to food supplementation, the confounding effect of the presence of

*B. F. Skinner, "A Case History In Scientific Method," Cumulative Record 1961: 91

'The term "derived experiment" is borrowed from the Piagetians who use it to describe the experiments you think up while you are engaged with a child as new questions and puzzles occur to you which had not been foreseen in your original experimental design.

diarrhea in some segment of the population can be removed by looking at those members of the population without diarrhea.

The derived experiment need not be prompted by disappointing results in the aggregate statistical analysis. The continuous monitoring of the data might suggest the need to "test" the role of some external factor which, in turn, might suggest a derived experiment on a limited number of cases to corroborate the initial suggestion. For example, if water quality is thought to be too poor in a region to allow for nutritional status improvement, a derived experiment might be implemented in which only a very few families are provided clean water. If the response of those families to the balance of the intervention becomes dramatically improved, it is an indication that the entire intervention be revamped to include the provision of good water.

6. Account For Confounding Factors In The Redesign Process. Whereas the classical statistical approach calls for rigid maintenance of controls, researchers applying reflection-in-action would not attempt to insulate experimental or control groups from confounding factors. Rather, they would attempt to detect such confounding factors rapidly in order to take them into account in the continuing redesign of the strategy of intervention.

In the light of this model of experimental intervention, two questions become critically important. First, in what ways and by what means can reflection-in-

June 17, 1982

REFLECTION-IN-ACTION

action yield clarity, objectivity, and valid interpretation of experimental data? Second, how could reflection-in-action reduce the sources of indeterminacy which we have discovered in our analyses of actual community-level interventions. These are the questions to which we will turn in the following sections.

EXPERIMENTAL NORMS

Figure 1 compares the models of rigorous experiment and reflection-in-action with respect to their objectives and methods for each of the experimental norms of validity, effectiveness, objectivity and generalizability. The two models aim at very different sets of objectives. Most fundamentally, the model of rigorous experiment aims at valid general propositions applicable to the design of nutrition interventions in types of community settings. (For example, "supplementary feeding programs should be implemented in communities of type "x", via nurse practitioners.") On the other hand, reflection-in-action aims at valid propositions applicable only to the experimental community. (For example, "in Candelaria, given the residual effects of earlier sewer and water programs, interventions undertaken in the next three to five years will have to be associated with a reduction of more than 20% in the malnourishment rate in order to be judged significantly effective.")

In the model of rigorous experiment, the effort is to achieve results that are objective in the sense that effects of intervention are kept independent of persons carrying out the intervention, or of reactions of the subjects to the mere act of experimentation. In reflection-in-action, where results are situation-specific, there is no need to separate intervention from the personalities of the change agent or from the act of experimentation. But for the sake of future

FIGURE 1

THE MODELS OF EXPERIMENT COMPARED

NORM	MODEL OF RIGOROUS EXPERIMENT	REFLECTION IN ACTION
Validity	Uses pre- and post-intervention observations: seeks to apply Mill's methods rigorously by the use of control groups, insulation of experimental and control groups, constancy of experimental conditions.	Close monitoring of processes as well as of conditions pre- and post-intervention. Rapid feedback of information to redefine experimental conditions. Draws on those close to the scene to detect confounding factors, offer plausible explanations. Uses derived on-the-spot experiments to discriminate among competing explanations.
Effectiveness	Criteria and strategy of intervention based on pre-intervention diagnosis of community situation.	Criteria and strategy revised iteratively, in response to rapid feedback of information <u>re</u> outcomes conditions, processes.
Objectivity	Sought through distancing of researchers from practitioners and subjects.	"Hawthorne Effect" expected, exploited. Effort not to eliminate experimenter bias but to detect what it is, and take it into account in interpretation of results.
Generalizability	Aims at general intervention-oriented results by using logic of experimental design to interrelate general features of context, intervention, controls, outcomes.	Experimental results always context-specific, generalizable only as elements of repertoire, themes that can inform invention of problem, theory or strategy in next case.

learning from experiment, it is important to monitor and interpret the ways in which features of person and experimentation influence behavior in the community.

In the model of rigorous experiment, the criteria of effective intervention must be designed, prior to experiment, in ways generally applicable to a type of community situation. In reflection-in-action, both the criteria and strategies of intervention are designed and redesigned, iteratively, in ways that take account of the uniqueness of the particular experimental community.

In the model of rigorous experiment, experimental validity is sought through pre- and post-intervention measurements, coupled with the use of control groups and insulating mechanisms to account for or eliminate confounding factors. In reflection-in-action, validity of experimental interpretations is sought through close observation of the processes which connect interventions, in the context of other factors, to outcomes; and it is sought through the performance of on-the-spot experiments, designed to discriminate among competing explanations of outcomes as these explanations are generated in the course of experiment.

The more specific meanings of the norms and methods of reflection-in-action will become clearer as we examine proposed responses to the sources of indeterminacy we have found in analyzing the data gathered from our sample of community-level projects.

"Dirty Data." We have named errors in the data which arise due to faulty practices of data-collection, storage and processing, "dirty data." In our work the most humorous example of "dirty data" comes from the Primops intervention where a tropical bug invaded the computer cards used for data storage and ate machine readable holes into those cards. Our experience suggests that most instances of "dirty data" are connected to the distance, in time and space, between the users of the data and those who collect it; the greater the distance, the dirtier the data.

Reflection-in-action, by definition reduces the distance between the source of the data and its users. The rapid feedback component of reflection-in-action generates the opportunity to locate errors early and to inform data-collectors and/or processors of those errors. Specific errors can be corrected and, if procedures are found to be consistently inadequate, they, too, can be modified. Moreover, project staff are more likely to be motivated to correct errors if they can see the usefulness of the data that they collect.

Factors which confound estimates of change in nutritional status. We have already partitioned the sources of indeterminacy into three categories--"dirty data," inadequate measurement, and inadequate data. The "dirty data" tends to erode confidence in all aspects of an analysis of interventions. As regards nutrition, there is a particular concern that the methods for measuring

nutritional status in the field are inadequate.¹⁰ These inadequacies tend to confound estimates of change in the nutritional status of a population over time. Let us consider three areas of inadequate measurement in the nutrition field.

1. Misclassifications can occur where measures of size and health are not coincident; for example, it has been shown that a child who has suffered chronic malnourishment, particularly during the age periods of the human growth spurt, is incapable of exhibiting the catch-up growth necessary to regain normalcy as defined by a growth standard derived from health children.¹¹ Also, it has been argued that improved nutritional intake may be applied to functions other than growth such as to metabolic functions or increased levels of activity.¹²

2. The classification of a child with regard to nutritional status may vary as a function of the

¹⁰We are referring to the use of anthropometric measurements, height and weight, to determine nutritional status. Typically, the height or weight of a preschool child is compared to a standard height or weight derived from observation of healthy children of the same age as that child. Alternatively, the weight of a preschooler is compared to the weight of healthy children of the same height as that child. Deficiency in any one of these "scores" is indicative of malnourishment.

¹¹R. Martorell, et al., "Malnutrition, Body Size, and Skeletal Maturation: Interrelationships and Implications for Catch-Up Growth," Human Biology 51(3): 387

¹²G. H. Beaton and H. Ghaessemi, Supplementary Feeding Programmes For Young Children In Developing Countries, Report prepared for UNICEF and the ACC Sub-committee on nutrition of the United Nations. October 1979, p. 35.

anthropometric standard used as the definition of normalcy. In particular, international standards derived from healthy populations in the developed world often prescribe more rapid growth than do locally generated standards. Thus, a population may appear to improve relative to the less ambitious local standard while remaining stagnant or even worsening relative to an international standard. (In our own Kottar analysis, we showed a drop in the percent of children suffering from second or third degree malnourishment over a two and one-half year period from 50.2% to 42.5% using a local standard but, using an international, sex-differentiated standard, we showed a drop from 50.4% to 45.5%)

3. Related to the choice of standard, the choice of "cut-points" for defining malnutrition can also alter the picture of change in nutritional status in a community. The selection of a level of deprivation relative to a standard for defining malnourishment is rather arbitrary. Because of the tendency for the "scores" of a population in a community to cluster about the traditionally accepted cut-off points, small shifts in those points lead to rather large numbers of children crossing the borderline between categories of malnutrition.

In addition to these three data related sources of indeterminacy which confound estimates of change in nutritional status, we can identify two other causes of

confusion. (We number these as 4 and 5, giving us a total of 5 confounding factors in all.)

4. Aside from ambiguity resulting from the choice of a standard, a set of cut-points and the misclassifications in a set of data, the analysis of nutritional change can produce different results depending on the statistical methods used and on the variables included in the analysis. The assertion that results depend on the choice of statistical methods is surely nothing new; however, in the nutrition field, that choice takes on special significance. The concept of improved nutrition in a community cannot be defined precisely. Some argue that mean percent of standard is an adequate measure of community nutritional well-being while others emphasize the need to show the greatest improvement among those initially worst off. Often, the selection of a statistical methodology is equivalent to the selection of a definition of improvement. In these cases, the statistics govern the description of change in nutritional status.

Similarly, the selection of variables to monitor change is a proxy for defining improvement. The most common anthropometric "ratios"--weight for age, height for age, and weight for height--measure different aspects of malnutrition. The latter measures acute malnutrition; the second, chronic undernutrition; and the first, a composite of the two. Often, as height for age scores increase, weight for height scores decrease. (Height is the numerator

of one score and the denominator of the other; therefore, it should not be surprising that improved height causes the scores to move in opposite directions.)

5. Finally, estimates of change in nutritional status are confounded by the method of accounting for the relationship between malnourishment and life cycle. The typical child in the developing world experiences a gradual deterioration of nutritional status from birth to some age of "maximum" risk (usually between 18 and 24 months) and then improves. Because participants in an intervention age, this pattern of growth must be acknowledged during the analysis. We have recommended that nutritional status changes be mapped by using a device called a "characteristic curve"--a graph of malnourishment against age at a single point in time.¹³ However, even with a characteristic curve, the picture of change can be altered by redefining age categories or selection of different cut-points or standards.

None of these factors are easily remedied by using either the model of rigorous experiment or reflection-in-action. The use of anthropometrics to determine the severity of malnutrition in individuals will remain inadequate; however, research into the nature of the response of each anthropometric scores to "proven"

¹³William D. Drake, Roy I. Miller and Margaret Humphrey, Final Report: Analysis of Community-Level Nutrition Programs, (Ann Arbor, Michigan: Community Systems Foundation, 1980) p. 97.

intervention might establish precedents for assessing responses observed in field settings. Also, research into the use of other tests for malnourishment, such as blood serum tests, may enable such tests to supplant the use of anthropometrics. (Most social interventions cannot afford the use of such tests nor are the skills to administer them readily available in the developing world.)

Similarly, the methodological issues concerning the choice of statistical tests and the separation of intervention effects from life cycle changes remain relevant for both models. Here, however, the model of rigorous experiment, in its pure form, calls for the application of a set of pre-defined tests for change. For example, an initial statement might be made that the experiment will be considered a success if a two-way analysis of variance on mean percent of standard, by six month age categories, demonstrates a positive statistically significant change in nutritional status over a two-year period. Reflection-in-action is far more flexible in its use of the array of available statistical methods. Results can be sought using any of the available methods and, then, corroborated or disproved using others. Under the model of rigorous experiment, the statistical analysis leads to firm conclusions. With reflection-in-action, the statistical analysis is a tool used not only to help form conclusions but also to raise questions and to redefine the experiment.

Sources of indeterminacy in the interpretation of change in nutritional status. In all of the cases we have examined, there are many plausible accounts of changes in nutritional status. The nutrition intervention itself is only one possible cause of change among others. Conversely, changes in other features of the community context may mask the effects of the nutrition intervention. The task of interpretation, then, is always one of identifying competing plausible explanations of observed phenomena and discriminating among them.

These are the main sources of confounding effects; that is, effects which may mask the effects of intervention or provide a basis for alternative accounts of change in nutritional status.

(1) Changes in the make-up of the target population. The sample of children in experimental or control groups may change during the course of experiment because of in- or out-migration, death, or, simply, movement of children in or out of the sample group (even though they remain alive and resident in the community). In every data set reviewed during the analysis of community-level interventions, changes in the composition of the target population played a role in the interpretation of the quantitative results. One of the more intriguing effects of shifting populations occurred in conjunction with the data from the Primops project. There was a marked tendency for children to "disappear" from one round of data collection to the next.

(This was probably due to a combination of factors including in- and out-migration, death, intra-barrio movement and errors in the recording of data.) A search for systematic patterns in the data revealed that a higher proportion of the malnourished disappeared in each round than of the nutritionally sound--50% of the severely malnourished as compared to about 20% of the less malnourished. The improvement in nutritional status of the population from round to round, in the aggregate, may well have been an artifact of that selective disappearance.

Because of the prolonged time duration of community-based nutrition experiments, it is not feasible to insulate the experimental group from such changes in population. It is also not feasible to find control groups which experience population changes identical to those found in the experimental group, or to insulate control groups from changes in population.

Reflection-in-action cannot prevent shifts in the make-up of either experimental or control groups; however, it provides researchers the opportunity to recognize the existence of such shifts while it is still possible to ascertain the reasons for and the magnitudes of those shifts. In action oriented interventions, change agents are usually concerned more with delivering service to those who are willing participants than in tracking down former participants to learn the causes underlying their disaffection. The concept of reflection-in-action, once

accepted by practitioners, changes this emphasis on service delivery to one of creating positive change due to the existence of those services. This is likely to cause greater concern on the part of the change agents in a) tracking their initial sample group to facilitate the charting of progress in response to intervention, and b) understanding the causes of disaffection of the sample so as to better design the package of services to eliminate those causes.

(2) Confounding changes in project environment.

Changes in project environment may be of many kinds--climatic, ecological, economic, political, social or administrative. In the following, we will limit consideration to only those environmental shifts which have come up in our attempt to interpret the data of the eight projects we have analyzed.

(2.1) Climatic. In Esperanca, portions of the baseline survey were administered in different seasons (six months apart) but the resurvey was done in a single season. Therefore, it is possible that the different magnitude of change in the matched pairs of experimental and control villages may be a reflection of the seasonal shift in nutritional status taking place ordinarily. In Kottar, we could not separate out the impact of the easing of a highly localized drought from the effects of program participation.

(2.2) Economic. Inflation, depression, or recovery from them, coupled with changes in the local economy of an

area, may all affect nutritional status. In Thailand, the rice fortification experiment was seriously hindered for a period of time when the blackmarket for rice, across the Laotian border, caused the illicit export of the fortified product.

(2.3) Administrative. Changes in the availability of other services or infrastructure often accompany intervention and confound the interpretation of analytic results. In Candelaria, the introduction of an improved water and sewage disposal system prior to the Promotora phase of the intervention may have paved the way for the work of the Promotoras; that is, it is possible that the teachings of the Promotoras were able to induce behavioral change because of the existence of the improved infrastructure.

(2.4) Social. Various changes in social structure, norms or patterns of behavior, may act to alter nutritional status. In Kottar, leaders of the Social Service Society emphasized the need for social and behavioral change and, in fact, viewed the nutrition component of their program as the incentive to bring mothers into their classes so that the Society could work to organize the mothers and modify their social behavior. It is difficult to attribute change in nutritional status to supplementary feeding or nutrition education when, according to the Kottar change agents, their greatest successes have been in organizing communities to

take advantage of all government offered services as well as their own labor and resources.

It is not possible to insulate experimental or control groups from such shifts in environment. More importantly, it is often impossible to predict, at the outset of an experiment, which changes in environment are likely to have an effect on nutritional status. After the period of experimentation, as we have discovered, the data in hand is usually insufficient to allow analysts to discriminate between an environmental change which may have influenced nutritional status and an intervention which was designed to do so.

The initial advantages of the model of reflection-in action are twofold. First, the practitioners and subjects are closer to the project environment than are researchers--closer, often, both in spatial proximity and sustained contact and in understanding. They are more likely to identify the changes in environment and the related changes in behavior, which may be affecting nutritional status. Second, when such environmental shifts are noted early in the experimental process, and when analysts discover early on that available data does not permit discrimination of the effects of such shifts from the effects of intervention, it may be possible to design derived experiments which will permit such discrimination.

Such experiments may be of two kinds, depending on the confounding changes at issue. Two strategies of derived

experiment are of particular importance. Both are strategies of disaggregation and differentiation. The first, the strategy of differentiated environment, separates out groups or regions on the basis of high and low exposure to the environmental change in question. For example, in Kottar it might have been possible to differentiate high and low drought sub-regions; both groups would still have been subject to the experimental intervention. The second strategy of derived experiment, the strategy of differentiated treatment, is really a version of the familiar "control group" experiment. Here, two groups of children would be identified, both subject to the shift in environment, but only one subject to experimental intervention. In Primops, for example, two groups of children might have been identified and followed--both subject to the benefits of improved sewage disposal but only one subject to the interventions of the Promotoras.

Both of these strategies are subject to possible difficulties. In differentiating groups by "high" and "low" exposure to environmental change, the experimenters may introduce new sources of variation. In a "low drought" region in Kottar, for example, children might have been surrounded also by a more prosperous economy or by a richer supply of foods. A similar difficulty would arise in the strategy of differentiated treatment: the non-treatment group might be subject to differences from the treatment group in more ways than one. The first difficulty might be

circumvented by a further differentiation of the sample. In the Kottar example, to take one instance, experimenters might try to disaggregate poor and less poor families in the low drought area. And the second difficulty might be circumvented by the use of randomizing techniques in the selection of the non-treatment group. But both of these methods may prove difficult or impossible to apply in the field. Constraints on resources limit the number of new treatment groups that can be set up, and also limit the number of non-treatment groups that can be observed. Continued differentiation of the sample may produce "cell" sizes too small for significant analysis. Finally, the establishment of non-treatment groups may be politically impossible or unacceptable on ethical grounds.

There is a third strategy which may be used to circumvent the difficulties noted above and may also be used directly to discriminate between environmental shifts and effects of intervention. This is the strategy of richer, more refined observation both of outcome and of process. In some cases, a richer and more refined observation of outcomes may provide evidence that weighs in favor of one plausible explanation rather than another. An observed reduction in the incidence of diarrhea, for example, may be more reliably attributable to the teaching of the Promotoras than to the improvement of the economy; and, if this reduction in diarrhea is closely correlated with improvement in nutritional status (relative both to previous nutritional

status and to other children in the sample who did not experience a reduction in diarrhea), then the effects of the Promotoras may be reliably differentiated from changes in the economy.

In other cases, a richer or more refined observation of the process of intervention--that is, the behavior of the practitioners and of the related and subsequent behavior and conditions of the families and children subject to intervention--may provide evidence which helps experimenters to discriminate among plausible explanations of outcome. For example, "recovery from drought" might be reflected in the variety and quantity of foods available to the family as distinct from the kinds of supplementary feeding provided by intervenors. But in such an example, experimenters would have to make frequent, reliable observations of food-availability at the level of the family and, perhaps, also of differential food-consumption within the family. Moreover, it is clear that such a strategy of observation would depend on the experimenters' selection of features to be observed and, at least implicitly, on a model of the ways in which various community-level factors and behaviors are causally linked to nutritional status.

(3) Artifacts of the experiment. A third category of confounding effects consists of changes introduced by the process of experiment itself. These may be of several kinds. For example, in Honduras, the change agents introduced new criteria for gaining entry into the program

which affected interpretations of change in nutritional status. Specifically, nine months into the feeding component of their intervention, they eliminated the worst cases from consideration producing, in the aggregate, a spurious improvement. In some projects, other changes may occur in the methods of data-collection, the reliability of data, or the quality of the service delivery system. In still other cases, there may be "Hawthorne Effects"; subjects responding, not to the particular contents of an intervention, but to the mere fact of being subjects of an experiment. Or, in a related effect, they may be responding to a feature of the experimental process (their relation to the experimenter, for instance) which experimenters themselves recognize as a substantive feature of intervention.

In the model of rigorous experiment, it is difficult or impossible to recognize and correct for such effects. Researchers at a distance from the actual context of intervention and observation are unlikely to detect clues of the existence of processes of this sort. If they should detect them, it would be difficult or impossible, in the course of analysis conducted after termination of the experiment, to build their awareness into their interpretation of the data. Distant from the actual delivery of services, they are unlikely to spot important features of that process which have not been named as significant in the experimental design. And the "double-

blind" or "placebo" methods sometimes used to respond to such difficulties are difficult or impossible to administer in the context of communities in developing regions.

With the model of reflection-in-action, however, researchers have direct, on-line exposure to intervention process and context. They are in a position to observe what intervenors actually do and how subjects of the experiment respond. Thus, they are more likely to be able to detect the presence of artifacts of experiment.

In some cases, their observations might lead to changes in experimental procedure (for example, by rectifying a distortion of the experimental sample). Their observations may lead them to discount spurious changes in outcome measures. When researchers suspect that subjects may be responding to the mere fact of experiment, the experimental period may be prolonged to allow for a decay in this effect. When researchers suspect that some unnamed and unanticipated feature of the experimental process is producing significant results, they need not respond by seeking to eliminate or control for that feature. On the contrary, they may seek to build it into the experimental design in a conscious and systematic way. (For example, they may seek to cultivate a particular kind of relation between experimenter and subjects of the experiment.) For the experimenters are not bound by a need to keep experimental conditions constant, or to work toward the formulation of propositions generalizable to a type of community context; their objective is specific

to the project in which they are engaged. "Hawthorne Effects", in the most general sense, need not be eliminated. However, they must be detected and described and they may be exploited in the redesign or redescription of experiments. These are functions researchers are better equipped to do when they have a continuing, on-line relation to the process of experiment.

Experimental norms and sources of indeterminacy in reflection-in-action. We are now in a position to summarize the ways in which researchers might respond, under the model of reflection-in-action, to the sources of indeterminacy we have encountered in our analysis of nutrition intervention data.

Some sources of indeterminacy, as we have encountered them, are not peculiarly related to the differences between the models of rigorous experiment and reflection-in-action. These include the choice of measures, cut-points, and standards for determining nutritional status, and the pattern of variation of nutritional status with age. Although these factors may be taken into account in the analysis of experimental data--for example, by using characteristic curves, or by deriving local standards--it is not clear that such analytic responses require a shift from one model to the other.

But other sources of indeterminacy would be significantly affected by a shift to the model of reflection-in-action. Errors in the collection and

processing of data ("dirty data") are subject to a feed-back cycle of their own. Their detection and correction would be far more likely, both from the point of view of both opportunity and motivation, if data were processed and results of processing fed rapidly back to practitioners in the course of experimentation.

Confounding shifts in context of experiment, both in the target population and in the environment, are not predictable before initiating intervention, at least not predictable with enough specificity to inform the design of experimental conditions. After intervention, when such shifts are discovered, it is unlikely that their confounding effects can be discriminated on the basis of data then available. It is possible, however, to detect such shifts in the process of experiment; and most likely to do so are the practitioners and subjects of intervention. Such detection opens up the possibility of gathering additional data and the subsequent application of statistical techniques to account for the changes in experimental context or the environment. It is also possible, some of the time, to design derived experiments, through the strategies of differentiated environment or differentiated treatment, to sort out the differential effects of intervention and environmental change.

It is also possible through close observation of the processes of intervention and responses to intervention to discover phenomena which permit discrimination among

June 17, 1982

REFLECTION-IN-ACTION

contending explanations of change in outcome measures. Such process observations may also uncover features of intervention, or its context, which inform new interpretations of the contents of intervention and suggest redesign of experiment. But, again, such observations are most likely to be made by observers who are educated by the attempt to make sense of the data; that is, to do on-the-spot research.

It is in these ways that reflection-in-action offers a route to experimental validity under conditions which cause the model of rigorous experiment to fail. However, two caveats must be kept in mind.

The first is that the objectives of reflection-in-action are different, and in a sense more modest, than those of rigorous experiment. Reflection-in-action aims at producing experimentally valid propositions only for the community context in which intervention is undertaken and only within the bounds of a particular state of that context. It does not aim at producing experimentally valid propositions generalizable to other community contexts, although processes undertaken and results achieved in one community may serve in another community to stimulate and guide reflection-in-action there.

The goals of intervention in reflection-in-action are discovered, in their specific forms, in the course of iterative, on-the-spot experimentation, just as strategies of intervention appropriate to that particular context are

discovered in that way. Thus, reflection-in-action aims at context-specific effectiveness and at context-specific discovery. The process of iterative data-collection, analysis, redesign, and re-observation has two faces. From the point of view of research, it is an iterative process of experiment, analysis and further experiment whose aim is to remove sources of indeterminacy in the data. From the point of view of the intervention, it is an iterative process of learning to produce, in the context and according to criteria of effectiveness peculiar to the context, desired changes in outcome measures.

A similar consideration informs the definition of "objectivity". Because interventions are seen as context-specific, there is no attempt to describe relationships between features of intervention, context, and outcome which are generally valid across contexts. If the experimenter influences processes and outcomes, perhaps as a consequence of his hypothesis, the task is not to remove that influence but to observe it and understand it. It is not necessary that other experimenters, in other contexts, be able to perform the same actions and achieve the same results. The task is, rather, to describe such influences and (when they are seen as positive) to incorporate them into the intervention.

The second caveat has to do with another kind of limit to reflection-in-action. There is no guarantee that, in any given context, reflection-in-action will converge on

univocal interpretation. The proliferation of derived experiments may multiply sources of indeterminacy. In such cases, inquirers might, in any case, persevere in the conduct of new experiment until a positive change in nutritional status has been achieved. The final "moves" in the sequence of interventions would then have been affirmed (that is, positively appreciated) even though the theory of intervention underlying those moves would be neither confirmed nor disconfirmed. Inquiry would then have achieved a significant though limited goal--the production of a move affirmed through its observed results. From the point of view of the practice of intervention, theory testing is always in the service of move-testing, so that such a limited goal is by no means negligible. To put the matter differently, in ordinary intelligent practice, the intervenor carries out on-the-spot experiments to test his strategies and assumptions only to the point of producing a move whose results he likes. There is always the possibility that explanations of those results might be other than the ones that informed the practitioner's intention. That possibility usually does not, and need not, bother the practitioner. It is when a present case is seen as a preparation for future projects that such possibilities are truly worrisome.

LEARNING FROM EXAMPLES OF COMMUNITY INTERVENTION

In previous sections, we have articulated two approaches to the study of community-level nutrition problems: the model of rigorous experiment and reflection-in-action. In practice, they are best thought of as the two extremes along a continuum. That is, in real situations there can be, and often is, a mixture of both approaches. One example of such a mixed strategy arises when an experiment designed to be rigorous is compromised by not having controlled for one of the critical variables and, as a result, loses generalizability and/or introduces indeterminacy. Another example, one where we have applied a mixed strategy, is in the evaluation of nutrition programs implemented at the community level.

Upon completion of the research project on the analysis of community-level nutrition programs described earlier, we believed that it would be possible to put into practice most of the elements contained in the reflection-in-action model in the context of a nutrition evaluation effort. Such a project was undertaken during 1982 with the objective of testing as much of the reflection-in-action model as possible. The project was an evaluation of the Food For Peace grant program in the country of Sri Lanka.¹⁴ The central question of the study was whether the food donation

¹⁴William D. Drake, et. al., Nutrition Programs In Sri Lanka Using U.S. Food Aid (An Evaluation Of P.L. 480 Title II Programs), (Ann Arbor, Michigan: Community Systems Foundation, 1982).

program had a favorable impact on the nutritional status of children under six years of age and, if so, by how much? Similarly, the same question was asked for children of primary school age.

One of the central features of the reflection-in-action model is the deep involvement of field-level practitioners in the interpretation of the results of the quantitative analysis. This involvement leads to on-the-spot research directed at resolving the importance of possible confounding effects. Note that this is quite different from merely providing the results of the study to practitioners--it is their subsequent involvement in further study which makes this difference. In order to implement this feature of reflection-in-action during the Sri Lanka evaluation, it was necessary to partition the project into three phases.

Phase I followed the ordinary approach of designing a study which attempted, as best as possible, to resolve the question of nutritional impact from the program. For the children under six years of age who participated in the Thripasha program⁵, a field data collection effort was implemented which ultimately generated some twelve thousand

⁵Thripasha is the Sri Lankan name for the food supplement used in the Sri Lankan Maternal Child Health Program. It means three nutrients in the local languages and consists of extruder-cooked, locally-grown corn and soya mixed with vitamins, minerals, a small amount of sugar and donated instant corn soy meal (ICSM). The children are brought to a health clinic where they receive not only Thripasha but also treatment for a variety of health problems. The health clinics in Sri Lanka are staffed by fully trained doctors and nurses.

June 17, 1982

REFLECTION-IN-ACTION

weight and age observations for eighteen hundred children. (Because the school feeding component of the Sri Lanka Food For Peace program did not call for routine observation of school aged beneficiaries, the data collection and impact analysis were limited to the Thriposha program.) Since Thriposha is distributed in Sri Lanka as one component of a package of health services, caution was taken to partition the sample in a variety of ways to control for possible differences in impact reflecting variations in the delivery system or recipient population from place to place. This partitioning of the sample was limited by the practicalities of field conditions and the realities of budgetary constraints. The sample was stratified by geographic region, by the type of health care provided, by the governmental unit responsible for staffing and funding the distribution center, by the socio-demographic characteristics of the family of the recipient child, and by the age of the child. Social and demographic family variables included age of mother and father, parental occupation as a surrogate for income, number of siblings and birth order. Of course, many more data elements could have been proposed--in fact, the number of possibilities was limitless. The decision as to which subset to collect was based on the cost and feasibility of data retrieval balanced against what our prior experience showed to be most critical. Our judgments were reviewed by local practitioners and modified to conform to their experience as

well. In short, while the initial design was not flawless, it was quite comprehensive when compared to similar evaluations done in other countries. The design showed promise of uncovering relationships between nutritional status of the beneficiaries and the extent of their exposure to the program if any such relationship existed. Phase I did not depart from the model of rigorous experiment except in one important aspect--there was a-priori recognition that there would be, at best, some indeterminacy resulting from the analysis even if measurable change in nutritional status was confirmed.

The second phase of the evaluation incorporated more of the elements of the model of reflection-in-action. Phase II consisted of an exhaustive analysis of the data gathered in Phase I. A report was prepared consisting primarily of tables, charts and graphs showing the relationships between variables deemed to be of potential importance. Characteristic curves portraying age cohort comparisons of the nutritional status of children who had participated in the program for a substantial period of time to the status of children who had just recently entered the program provided concrete evidence that participants of all ages were nutritionally better off than new entrants. Tables were included presenting evidence that the magnitude of the benefit derived from participation appeared to vary by the type of organization rendering service and by the physical setting of the clinic, rural or urban. Nutritional status

was shown to be related to several of the other stratification variables as well.

With the preliminary report completed, Phase III, the formal field level evaluation, began. Armed with specific results of the analysis, the evaluation team engaged in a dialogue with local practitioners concerning the causes of the observed results. In several instances, the interpretations of the analysis were modified substantially from what seemed obvious from the analysis. For instance, differences in outcome which seemed to be related to the type of unit providing service and type of health care offered in conjunction with the food supplement were found to be highly correlated with the clinic staffing levels per beneficiary. This competing explanation was first proposed by one of the local staff and then tested with on-the-spot observations of clinics in several different parts of the country. Thus, what appeared to be strong evidence in support of the hypothesis that the type of governmental unit administering the program played a role in causing positive response to it was reinterpreted to be evidence supporting the role that staffing levels play in insuring effective clinic operation. It is important to note here that data on staffing levels had been gathered at the onset and, initially, found to be of no importance. The reason for this lack of detection of a relationship during the early rounds of analysis was that the variables selected for testing the hypothesis were too aggregated. In short, the

study design team, even though it included knowledgeable local staff, had selected and measured the wrong variable. It was only after successive analyses that enough local perspective was gained to narrow and focus the definition of staff to a level which revealed the relationship between staff size and nutritional impact. It could be argued that had the correct variable been measured at the onset, this problem would not have arisen. However, it is our belief that the often subtle process of deciding precisely which variable is most appropriate to gather before analysis is extremely difficult. Regardless of the expertise of the analyst, mistakes and omissions are usually made and, as a consequence, the iterative process embodied in the reflection-in-action model becomes essential.

Since Phase III was concerned more with the determination of attribution rather than outcome, the issue of competing explanations was central to this part of the study. The Sri Lanka evaluation, like most such studies, contained several possible competing explanations for the observed outcome besides the intuitively appealing one that the program worked. The reflection-in-action model called for a comprehensive enumeration of competing explanations, even though some were beyond the scope of inquiry. During the four week in-country study period, the evaluation team was able to muster evidence for rejecting all significant competitors, thereby attaining, with some degree of determinacy, that the observed improvement in nutritional

status was attributable to the bundle of services associated with Thriposha distribution. Had we not been able to articulate those competing explanations and perform on-the-spot research, our evaluation would have been inconclusive.

It should be noted, however, that it was not possible to separate the effect of Thriposha from the package of other services rendered at the same time as the food supplement nor was it feasible to generalize the findings regarding food supplementation to other settings. While an evaluation based on the model of rigorous experimentation would have, in theory, had the potential for broader generalization on some dimensions, in practice, a much higher degree of indeterminacy would have been the result. Figure 2 compares the characteristics of evaluation study designs using the two models.

Adoption of the reflection-in-action model to an evaluation of the sort done in Sri Lanka provides some evidence concerning the usefulness of the approach. However, as mentioned earlier, only some elements of the model were applicable. Missing were the elements building in the evaluation and on-the-spot research as part of the ongoing program. An application including these elements remains to be done.

In terms of generalizing the results of the Sri Lanka evaluation to other settings, an additional caveat must be mentioned. Neither model would have permitted generalization to other settings with statistical validity.

FIGURE 2

EVALUATION STUDY DESIGN CHARACTERISTICS

EVALUATION STUDY CHARACTERISTICS	MODEL OF RIGOROUS EXPERIMENT	REFLECTION IN ACTION
Evaluation team composition	A balanced group of non-local experts supplemented by local staff for coordinating and data gathering functions.	Non-local experts <u>combined</u> with field level practitioners intimately knowledgeable about the detail of the contextual setting.
Phasing of evaluation study	Two major phases: (1) study design formulation and (2) field visit, analysis and report preparation.	Three major phases: (1) study design formulation, (2) analysis and (3) field visit centered around on-the-spot inquiries to resolve questions raised by the first analysis.
Timing of analysis	Comprehensive analysis of field data is the culmination of the project. Final report draft subsequently prepared and distributed to interested parties for comment.	Analysis of field data is a prelude to the formal evaluation. External study team and local practitioners possess analysis results at the onset of the field visit. Successive iterations of analysis generally are required to reduce indeterminacy.
Enumeration of competing explanations for the observed outcome	Generally restricted to those explanations amenable to analysis.	An exhaustive enumeration of competitors extending, if necessary, beyond the original bounds of the study design.
Perspective of evaluation team members	Experts strive to attribute any observed changes in the target population to the intervention relying heavily upon knowledge gained from other experiences.	Experts recognize their absence of knowledge about the local setting and the high likelihood of mistakes in attribution as a consequence. Less reliance on generalization from other cases and more reliance on knowledge from local practitioners and on-the-spot experimentation.
Willingness to admit "soft" and non-numeric evidence	Resistance to evidence which is not measurable and/or quantifiable.	Heavy weighting on "soft" evidence, especially when corroborated by triangulation with other independent information sources.

June 17, 1982

REFLECTION-IN-ACTION

Cultural differences, social and economic infrastructure differences, education levels of the target population, and the particular personal characteristics of the program entrepreneurs are all critical determinants of the success of a program and all are context specific. However, that does not prohibit us from making a judgment about the transferability of findings to other settings, especially if that judgment is restricted to suggestions about a "place to begin" an intervention. For another aspect of the reflection-in-action is the acceptance of the inappropriateness of prescribing, in advance and from afar, the long-term details of an intervention. We need to be concerned only with the starting position and the process by which successive iterations will improve the proposed program over time.

Using a framework for transferring findings of studies as described above, offers a solution to another problem which has plagued the nutrition field--namely, seeming conflicts among study results. It is far easier to accept apparent conflicts if they are the result of unresolved indeterminacies and/or context-specific elements for which no controls were available. Instead, transferability of findings comes from building a repertoire of cases which, with judgment applied, offer suggestions for future programming.

PROBLEMS OF IMPLEMENTING REFLECTION-IN-ACTION

Implementing the reflection-in-action model is not without its problems. The following section will enumerate some of the special responsibilities imposed by this model and suggest ways of dealing with them.

Both institutional and individual problems exist. Usually, at the level of the individual, the researcher and practitioner come from different educational backgrounds and are motivated by different interests--the practitioner being action oriented and the researcher more interested in the search for knowledge. Reflection-in-action calls for the attributes of both to be present in one individual or, at least, represented equally on a team. The model also calls for a blend of behaviors as well. At the field level, there is often a short-term conflict between expending resources (time or money) on gathering and analyzing data and rendering services to beneficiaries. We have seen several instances in the field where this conflict is quite real. Too frequently, this conflict manifests itself when political pressures for expanded coverage are brought to bear on the change agents; that is, where short term political interests are placed above long term effectiveness.

We believe that there are ways of turning this potential conflict into a condition beneficial to both perspectives. When the information gathered for research or learning purposes is also the same information used to operate and manage an intervention, several gains occur. First, error

correction as discussed under "dirty data" previously, becomes easier, thereby facilitating useful analysis. Second, the correction of errors not only helps the analysis but also improves program operations. Missed diagnosis of children at-risk are reduced or eliminated. Management questions, such as staffing imbalances, surface more readily and, therefore, can be corrected with proper feedback.

Finally, there is the potential for more useful dialogue among practitioners. We say "potential" because a related problem, the problem of overreaction to quantitative data, must be resolved prior to achieving improved dialogue. The best way of describing this problem is with an anecdote, one which repeated itself several times during the course of our investigations of community-level nutrition programs.

In an effort to test the information feedback elements of reflection-in-action, a summary of data on the nutritional status of program recipients was provided to the administrators of an intervention. In one instance, this summary showed a decline in the nutritional status of the target population during the previous year. Upon seeing these results, the reaction of the action-oriented practitioners was to look for program changes (including project termination) which would remedy the "problem". In this one case, an action to terminate would have been a mistake. The observed drop in nutritional well-being most probably was due to a deterioration in the physical environment. Moreover, the drop was not statistically

June 17, 1982

REFLECTION-IN-ACTION

significant and, therefore, could be attributed to random variation. Considerable effort on the part of the researchers had to be devoted to describing all of the competing explanations as well as some basic elements of inferential statistics. Finally, the logical arguments which showed the time lags between program implementation and detectable outcome had to be traced through. In sum, without a minimum critical level of expertise in experimental design and in the interpretation of quantitative analysis, practitioners can react prematurely or improperly to mathematical results and, in doing so, do more harm than good. Decisions may be based on improperly or incompletely developed "hard" data at the expense of field wisdom. Over reliance on "hard" numbers is not limited to practitioners in the field. That a similar pattern exists within funding bureaucracies will be discussed in a later section of this paper.

The remedy to this problem of overreaction in local decision making lies in integrating practitioners into the data gathering and analysis process. If practitioners participate in the entire study design, analysis, and interpretation process a few times under real field conditions, they become armed with sufficient knowledge and perspective to make proper interpretations when applying the results to decisions. Thus, the remedy to the problem of appropriate interpretation for decision making purposes is precisely the same remedy as that which is needed for

June 17, 1982

REFLECTION-IN-ACTION

conducting careful analysis for research purposes--local practitioner involvement.

Institutional problems in implementing reflection-in-action may be more difficult to overcome. If one fully accepts the model, then for most interventions it is inappropriate to specify the precise nature of the program in advance. Only the starting position or "best guess" can be stated along with the process by which successive iterations will converge on the most useful intervention design. But most funding agencies are geared to receiving requests for support in terms of services provided, staff time, supplies, materials and so forth. Generally, even if only one year at a time is funded, sponsors request fairly detailed projections of future years' needs. Furthermore, evaluation of program efficacy is considered to be an activity useful primarily for decisions about future funding levels and, therefore, in need of objectivity brought by independent reviewers working at a distance from project administration.

In order for funding sources to support interventions embracing the reflection-in-action model, requests must be deemed acceptable which do not include detailed long term program implementation plans. In turn, the absence of such plans forces the sponsor to conduct its own evaluations of program progress to change, evaluations to determine future funding level. No longer is it possible to evaluate based on a comparison of the delivery of services actually

rendered by the program against the detailed plan provided in advance. Historical emphasis upon process indicators as surrogates for difficult-to-obtain outcome indicators is not acceptable. Rather, evaluations must focus on outcome indicators and the often complex task of attributing observed outcome to the intervention.

Movement towards a funder decision process which emphasizes outcome rather than process indicators is, in our judgment, positive--providing the analysis is sufficiently complete to uncover true impact, when it exists. No longer is it necessary (and it was never desirable) to make assumptions about the causal relationship between services rendered and benefit to recipients. Under reflection-in-action, it is necessary to develop evidence of that causal relationship.

It is again interesting to note the connections among different elements of implementing the reflection-in-action model. Applying the model at the point of funding decision demands that the model be applied when evaluating the intervention. The issues of trade-off between objectivity and contextual knowledge discussed in previous sections all apply. They call for revised ways of funders relating to the interventions they support. Now, it does not make sense for funders to assemble evaluation teams comprised solely of experts rich in the knowledge of generating pithy documents and poor in local understanding. While the evaluation team approach still has a role in some instances, especially if

June 17, 1982

REFLECTION-IN-ACTION

the team includes members knowledgeable in the local situation, under reflection-in-action more emphasis is placed on developing sound evidence of program impact from locally generated data and other feedback from the field.

IMPLICATIONS OF OUR ARGUMENTS FOR THE BROADER QUESTION
OF INTERVENTION ORIENTED SOCIAL EXPERIMENTS

We have asserted that an approach to nutrition interventions which does not recognize the instability, unpredictability and uniqueness of the local environment will often be of little benefit to recipients. Further, we contend that attempts to evaluate the efficacy of such programs using the traditional model of rigorous experiment will suffer excessive amounts of indeterminacy and inaccuracy when compared with an approach relying more on detailed knowledge of local practitioners. The reflection-in-action model is both an intervention style and an evaluation process. It is a context specific, iterative approach capable of adapting to the ever changing local conditions through rapid feedback of analysis of locally generated data. What then are the implications of these arguments for the broader question of intervention-oriented social experiments? We classify these implications into three broad categories: (1) on recipients participating in a program, (2) on social scientists and practitioners and (3) on the relationship between donor and implementing agency.

Recipients Participating in a Program

It is very easy for a professional dedicated to reducing child malnourishment to assume that any help provided is a net gain to the recipient. But we have found that many interventions show little, if any, measurable improvement in the nutritional status of the target

population.' ' If one recognizes that, typically, considerable resources are devoted to such programs, it could be argued that a far better option would be to provide a direct supplement to the recipient or to fund the development of general infrastructure such as transportation and education systems.

Beyond the question of expending tangible resources is the time, energy and commitment of the persons participating in the program. The mother and child who devote a day walking to and from a health post in order to attend a well-baby clinic in the expectation of receiving an allotment of weaning food has a considerable investment in the program. Furthermore, nutrition programs sometimes call for change in firmly ingrained cultural habits such as inter-familial food distribution patterns. Even if the program does not ask explicitly for such changes, they can occur anyway often creating dependencies. For example, in on-site feeding programs, families often alter the inter-familial food distribution patterns to compensate for the fact that one family member is fed elsewhere. Similarly, family purchasing decisions change in response to the increase in disposable income associated with receiving free food or other services with the net effect being the creation of dependencies on the program. In any intervention, there are

' 'Caution must be taken to distinguish between measurable impact and impact itself. If the research design is faulty or if there is sufficient misclassification in the observed variables, true impact will be understated.

significant costs, broadly defined, to the recipient and there are not always the corresponding benefits.

The reflection-in-action model begins with the assertion that the proposed intervention may need to go through one or more iterations before it is providing a useful service. It does not presume to be a remedy--only the first step in a process. When the intervention process is conceived in this way, the relationship between recipient and program changes. The recipient of services is not automatically a beneficiary experiencing a net gain in welfare but rather a participant in a local experiment conducted with the objective of developing a strategy that will lead to the attainment of certain benefits. Under these conditions, the participant in the local experiment should be entitled to some measure of protection against hardship that could arise from participation if the program is unsuccessful. The policy implication of this argument is that an important design criterion for a nutrition intervention is that it contain a plan or process for responding to this unfavorable contingency. Provisions should be included for changing the mix of services delivered by the program if benefits derived from existing components do not live up to expectations. Also, participants should be informed of the experimental nature of the activity they are about to engage in. Finally, provisions must be made, in advance, for smoothing the transition back to the original local conditions in the

event the intervention is terminated. These provisions should include tangible resources earmarked for this purpose.

New Skills, Norms and Attitudes for Social Scientists and Practitioners

Social scientists, particularly those trained in analytic methodology, are prone too often to become enthralled with their technique. There is a tendency to retreat to that which is familiar and to that which has provided a competitive edge during past endeavours--namely, sophisticated analytic techniques. While these techniques most certainly have usefulness, they are a very small component in the repertoire of skills needed for the reflection-in-action model. Again, uniqueness, instability of environment, and unpredictability of the local condition which forces intensive scrutiny and emphasis on local, context-rich information demands that skills more akin to the anthropologist and change agent be emphasized. Even if they want to, scientists can no longer avoid the effects that their results have on decision makers. Consequently, a knowledge of and the ability to deal with the broader implications which analysis might have becomes essential. In summary, scientists utilizing the reflection-in-action model must strive to broaden their skill levels beyond those currently prescribed.

Behavior norms may have to be altered as well. Emphasis on numerical analysis of clean data as presented in current academic journals must be replaced by a norm which

exposure to research design, analysis techniques and careful interpretation of results. Our experience is that in the context of using analysis for improved local decision making, changes in the attitude of the practitioner are minimal if required at all. Active participation in two or three such analyses is sufficient to acquaint the practitioner with the requisite steps. While some formal training is highly desirable, on-the-spot training is more effective and feasible in view of the administrative responsibilities of the practitioner. Occasionally, when local administrators have had job experiences in the private sector, they have already acquired the necessary patterns and attitudes. The closest analogy to the reflection-in-action model is the behavior of the small independent businessman who continuously adjusts his mix of services or products based on an analysis of feedback obtained from customers and the local economy.

There is an exception to the case where only minimal change in attitude is required of the practitioner. Some practitioners believe, for whatever reason, that they know the proper remedy for a problem. In the nutrition field, we have heard, too often, "but how can feeding hungry people be bad?" Reflection-in-action can begin only after such attitudes are dispelled.

Donor and Implementing Organizations

The most significant implication of our arguments in this paper come in the relationship between donor and

more fully includes concern over the quality of that hard data, the integration of "soft" data sources into an analytic framework, and the ability to accept and articulate indeterminacy. More important, the need to become close to local conditions, whether it be as a participant-observer or in conjunction with local colleagues, may reduce apparent academic productivity. Productivity of a social scientist is often measured by scholarly publications. Currently, the most effective way for the social scientist to show productivity along this dimension is to emphasize analysis and written description of the interesting findings stemming from analysis--either substantive or methodological. Alternatively, the time and energy spent in developing close working relationships in a local setting are often underrecognized. Yet the reflection-in-action model demands active local participation--beyond that which is required to obtain data.

In some instances, attitudes as well as skills and norms need to be adjusted. It is very easy for scientists to be overly impressed with their capabilities, especially when much of the community in which a scientist works operates to reinforce that belief. A more humble attitude about who really has the important pieces of knowledge in a local context is essential before a healthy relationship between practitioner and scientist can develop.

From the practitioners' viewpoint, change is called for too. We have discussed in earlier sections the need for

implementing organization. According to the reflection-in-action model, proposals from prospective change agents to possible donors would be couched in terms of a proposed starting position for a nutrition intervention together with a description of the process to be used for making incremental changes toward an improved program design. This process would be based on a mechanism for obtaining and using feedback from the target population. Presumably, sponsors would make a decision to fund based on the quality of that redesign process, the knowledge embodied in the starting configuration for the intervention and, of course, the overall needs of the target population in relation to other alternatives.

Decisions regarding continuation of funding would be based on a reflection-in-action assessment of the efficacy of the program and the target population's current needs. Formal evaluations would include a synopsis of the various iterations that the program administrators had implemented, the reasons for those changes and the outcomes achieved. The field component of the formal evaluation would include local practitioners whose role would be to surface additional competing explanations for the observed outcomes and assist in resolving indeterminacies whenever possible.

The changes required within donor agencies to enable implementation of reflection-in-action are profound indeed. Beyond changes in project design and evaluation criteria, the staffs of the donor organization would be faced with a

June 17, 1982

REFLECTION-IN-ACTION

new set of political issues. It is one thing for the administrators of a Foundation to propose to its board a well-defined, apparently coherent multiyear project designed to alleviate a pressing social problem. It is another thing to propose a program which admits at the onset that it is only an attempt at problem resolution which has a low probability of success as originally conceived. The same difficulty exists, only more so, for administrators of government agencies seeking funding in a highly political environment. (Governments, even more than foundations, are bound by a yearly budget cycle and, because of their high public visibility, are subject to strong pressures to show short term, positive response to their actions.) Frankly stated, the willingness on the part of administrators in donor agencies to be fully descriptive with regard to the uncertainties associated with a given proposed remedy places them in a vulnerable position. For there will always be other administrators who, in the interest of raising the political likelihood of gaining approval, will be quite content to propose projects in the traditional less accurate but far more comforting manner.

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