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The control of non-treatment variables: Necessity or illusion?

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Summary

This paper opens with a discussion of different approaches to the control of non-treatment variables in the evaluation of nutrition programmes. It describes some of the complications of evaluating nutrition education in the Third World, and outlines how investigators have dealt with these problems. Sources of confounding variables are discussed and some suggestions are given as to how to deal with certain types of variables.

The paper is written from the perspective of direct communication, not from that of the mass-media, although many remarks also apply to the evaluation of the latter. Special attention is paid to ongoing programmes, programmes that are not easily adapted to the demands of research, in contrast to experimental programmes explicitly geared to the requirements of evaluation.

Key words: nutrition education – evaluation – research design – statistical control – non-treatment variables

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1. Theoretical outline

A distinction can be made between formative and summative evaluation. Formative evaluation concerns the operation and implementation of programmes, i.e. their daily management. Summative evaluation (also called impact evaluation) attempts to measure the effects of nutrition programmes by objective and systematic means. «Objective means» implies the use of reliable measuring instruments, for instance properly tested questionnaires, anthropometry etc. «Systematic» here refers to the selection of groups of subjects in such a way that subsequent analysis can reveal the impact of the programme, independent of other factors. Impact evaluation therefore has two major components: indicators and design.

Indicators serve to measure the degree of improvement or changes in outcomes. It is usual to distinguish between proximal and distal outcomes. In the case of nutrition education, a proximal outcome would be improved nutritional knowledge and attitudes; also changes in nutrition behaviour.

Improvements in nutritional status are already more distal, while improvement in the intellectual development of children would be truly distal. The more distal, the larger the potential array of other variables that also influence the outcome. It is customary to distinguish between treatment variables (nutrition education), non-treatment variables (other determinants of nutrition behaviour and nutritional status),¹ and outcome variables (the proximal and distal outcomes).

An important function of research designs is the control of non-treatment variables. Firstly, to ensure that these other determinants of nutrition behaviour and nutritional status do not offer rival explanations for any purported relation between treatment and outcome. Secondly, to reduce error variance and thereby increase the power or sensitivity of the study.

Different approaches to the control of variables are advocated, and the use of terms by different authors is not uniform and sometimes confusing. The following description and definition of terms is largely derived from HENNIGAN, FLAY and HAAG (1979).²

The experimental approach derives its name from the so-called experimental control achieved by means of experimental designs: measures of outcome variables are compared across two or more groups of persons who have received different amounts of treatment. These groups are formed by randomization, that is random assignment of persons so that the groups are equivalent in every respect except the treatment. Any differences in outcomes must therefore be the effect of the treatment.

Quasi-experimental designs also involve comparisons between different treatment groups and control groups not exposed to the treatment. Unlike in experimental designs, these groups are not formed by random assignment. Such non-equivalent comparison groups can be selected in different ways.³ Since the groups are not formed by randomization, the possibility always remains that other differences exist between them that offer alternative explanations for relations between treatment and outcome.

While experimental and quasi-experimental designs make use of comparison groups, statistical control is characterized by statistical adjustment of non-treatment variables. Statistical control makes use of what is known and can be assumed about the relationship between variables and the observed correlations to make statistical adjustments that remove the influence of non-treatment variables on outcome variables. Statistical control can be used when it is impracticable to create comparison groups and in those cases is sometimes termed the non-experimental approach. In other

1 Also called confounding variables, nuisance variables, extraneous variables

2 In the rest of this paper we have tried to adhere to the terminology suggested by these authors, who have tried to bring some unity to the current confusion of terms

3 See, for example, CAMPBELL and STANLEY (1966) and COOK and CAMPBELL (1979)

cases, however, experimental and statistical control can be used in conjunction. Experimental research often uses statistical adjustment to control for non-treatment variables that cannot be randomized conveniently, while in the case of quasi-experimental designs, statistical correction is often used to remove unwanted differences between comparison groups.

The statistical technique most commonly used is analysis of covariance; descriptions of this procedure can be found in textbooks such as KERLINGER (1973). Technical discussions of the use of multivariate analysis in statistical control, among them analysis of covariance and its larger brother, multiple regression, can be found in COHEN (1975) and REICHARDT (1979).

It must be stated explicitly, though, that however refined the statistical procedures, there always remains the possibility that other variables not included in the analysis may be responsible for the observed results. In this respect, both statistical control and quasi-experimental designs are flawed.

It is generally agreed that control over non-treatment variables is best achieved through randomization, while statistical control is generally considered superior to quasi-experimental designs because it gives more opportunity to remove error variance.

2. Practical experience

Of the 176 studies included in the bibliography for this workshop, 94 concern impact evaluation and attempt to measure changes in outcome variables such as knowledge, behaviour, and nutritional status (SCHÜRCH and WILQUIN, 1982). In 34 studies an attempt was made to control for non-treatment variables. One study did this by means of randomization, two studies relied exclusively on statistical control. At least 25 studies used quasi-experimental designs of various degrees of complexity. It is interesting to consider why randomization and statistical control are used so little.

In the case of ongoing programmes it is difficult to allocate subjects randomly to different groups. Firstly, the ethical problem arises whether certain children should be given inferior treatment or even be withheld from treatment. A more general problem is to ensure that mothers and children in the different groups do not become mixed. This could be solved by running separate clinics, but the minimal record keeping in most programmes often makes this impossible. A further problem in experimental designs is to ensure that the different groups are truly equal it is necessary to study and compare the groups before as well as after treatment. Since nutrition education does not show effects rapidly such studies must necessarily cover extended periods of time. Not only do such long time-periods themselves present a problem, they also make it more difficult to keep the different groups separate.

That few researchers resort exclusively to statistical control is not surprising in view of the fact that it requires advance knowledge about the determinants of nutrition behaviour and nutritional status. The more that is known about the relations between non-treatment variables and outcome variables, the more effectively statistical control can be used. Such knowledge is limited for Third World communities. Statistical control requires that fairly large numbers of people are studied, which is not always easy to realize. A further difficulty is that statistical control generally necessitates intricate computations. This requires not only powerful computer facilities but also takes a lot of time for analysis. It is not surprising therefore that researchers tend to rely mostly on quasi-experimental designs, since these are, at first sight, relatively easy to work with and the ensuing calculations less complicated. Often the researcher is simply left little choice by the circumstances prevailing in the programme.

I would like to illustrate this with some examples from our research in Kenya. In a geographical area with a homogeneous population (Central Province) we studied three different programmes, each of which presented unique evaluation problems. In each case we tried to evaluate the impact of the intervention on nutritional knowledge, attitudes, behavior and nutritional status.

A The first of these programmes was that of the Nutrition Field Workers: nurses who work as members of Mother and Child Health (MCH) teams at government health centres, where they give nutrition education to mothers attending MCH clinics and monitor children under five years of age. It turned out, however, that in practice the nutrition field workers had a lot of freedom to arrange their activities, and that their work showed considerable individual variation, while there was also a fairly high turn-over among them. Their activities further appeared to overlap those of other MCH personnel to varying extents. This made it necessary, first of all, to enlarge our focus from the effects of contact with nutrition field workers to that of contact with MCH clinics.

Nutritional effects of MCH attendance cannot be expected to occur quickly, and the limited time available for the study did not permit coverage of long periods. It was not possible, for instance, to interview the mothers when they started visiting and after a sufficiently long period had elapsed. Consequently there was no possibility for an experimental design with the required randomization. Two alternative procedures were considered: comparison between areas with and without health centres, and comparison between mothers with and without contact with MCH clinics. Most people in Central Province, however, live within travelling distance of various health facilities and it would have been unrealistic to look for an area where mothers had never attended MCH services. It would have been

equally unrealistic to look for individual mothers who had never visited an MCH clinic; immunisation rates are high, and people go to great lengths to obtain treatment for their children. Although mothers with first, new-born, children could conceivably constitute such a group, they are generally young, often recently married and in these respects differ from the majority of mothers attending the clinics. However, people do differ in their exposure to MCH services and we decided to use a comparison between frequent and infrequent visitors; this would also represent a meaningful evaluation.

The next problem was how to distinguish between frequent and infrequent visitors. They could not be selected from existing records since weight charts were not handed out and no other records were kept of the visits of individual mothers and children. We could have asked mothers how often they had attended over a certain period of time and divided them accordingly. However, there may be specific reasons for greater frequency of attendance e.g. greater motivation, higher education, which could influence the comparison between the groups. Groups were therefore selected on the basis of the travelling time needed to reach the clinics, which is a «neutral» reason for differences in frequency. We finally settled for a comparison between frequent visitors living nearby and infrequent visitors living far away (HOORWEG and NIEMEYER, 1980a).

B The second programme was aimed at children between the ages of 6 and 60 months from needy families. Once the children are enrolled in this programme, the mothers are required to pay monthly visits to the clinic, where the children are weighed, nutrition education is given and where mothers receive supplementary foods for the young child. The limited time available for the study again did not permit coverage of long periods. An experimental design with the required randomization was not possible, particularly since the rate at which newcomers were accepted was low, about 3 or 4 cases a month at each clinic.

The obvious alternative, a comparison between mothers with and without contact with the programme, would have been flawed because the participants in the programme came from the poorer section of the population. Any comparison with the general population would not only have reflected the effects of the programme but also other differences between the participants and the general population. Another, more suitable, alternative was to compare recent entrants with long-time participants. This type of comparison is only allowed if there are no differences of a social or other nature between the mothers and children in the two groups. Such differences could arise in the case of selective re-attendance or self-selection, i.e. if certain women drop out of the programme and if they have certain characteristics which distinguish them from other women, such as poverty or lack of interest. A fairly simple solution to this problem was possible in this study.

A group of participants who had been attending for more than 2½ years, and a group of recent entrants (within the last 6 months) were selected. In the course of the following year the clinic records were checked and the recent entrants who had stopped attending (10%) were excluded from the study. (This procedure was only possible by postponing the analysis for more than a year, which is not always feasible. But if it can be used, it is a simple and effective countermeasure.) This method of comparison, though, was better suited to studying the mothers than the children (indeed, a common problem with the evaluation of child nutrition programmes is the definition of the unit of study, i.e. mother or child). In this particular case the recent children were generally younger than the other children by more than 2 years. In addition, the children in the «recent» group were of somewhat better nutritional status at the time of enrollment than the children in the «long-time» group were when they joined the programme some years previously. As a consequence of these complicating factors we were forced to resort to a different research strategy for the children and we had to restrict the analysis to the group of children who had been attending for several years (HOORWEG and NIEMEYER, 1980b).

C The third programme consisted of nutrition centres where women with malnourished children are admitted for a 3-week course consisting primarily of nutrition and health education. The siblings of the malnourished children are usually admitted as well. In this case it was possible to study the same mothers and children before and after their stay at the centres, i.e. at admission, at discharge and six months later at their homes. However, about 25% of the women could not be located because they had moved away from their homes, and their new places of residence were unknown. These were mostly young women who were in the process of separating from their husbands. This important subgroup had to be omitted from the study. To interpret the findings for the remaining cases it was necessary to employ a control group not exposed to the treatment, in order to observe whether changes had occurred independent of the treatment and, if so, to measure their magnitude. Since it was obviously not practicable to deny certain children the treatment they came to seek, a control group was selected from women and children who were seen during a nutrition survey conducted among the general population at the same time. But this, in turn, introduced a potential error because the children in the control group were not malnourished. While this gave a good insight into the socioeconomic background of the cases at the centres, it hampered the assessment of nutritional progress because there was not sufficient information on the «natural» progress of children in poor condition (HOORWEG and NIEMEYER, 1982).

These examples show how the organization of the programme concerned often forces certain strategies on the researcher and how despite the best of intentions one has to accept research solutions that are less than ideal. These examples also illustrate why it is usually impossible to avoid the intrusion of all conceivable variables. WEISS (1972, p. 72) has pointed out that the objective of quasi-experimental designs must be not so much to guard against any possible source of error, but rather to control those sources of error likely to appear in a given situation.

Let us next consider which non-treatment variables are important in the evaluation of nutrition education and from what sources they originate.

3. The diversity of non-treatment variables

There are three important ways in which non-treatment variables can be introduced into any evaluation. We have already mentioned the selection of non-equivalent comparison groups. But there exist two other major sources of error that must be discussed first.

Variables accompanying treatment

There is, first, the treatment itself. It is possible that although nutrition education is regarded as the sole intervention, the treatment really consists of more than that. For example, it is not uncommon that education is accompanied by the weighing of children, which in itself may have a positive influence. In fact, in one programme the weighing was regarded as the primary intervention and the education as secondary (SISWANTO, KUSNANTO and ROHDE, 1980). In such cases it is perhaps not necessary to distinguish between the two services, but possible to regard them as a combined educational experience. It is indeed a matter of a wider influence: psychologists have long demonstrated that attention as such often contributes to the success of whatever treatment is provided. The potential error becomes more serious when the staff also provides medical care to children in poor condition or when they refer such cases to friendly medical personnel. An even more complex situation arises when mothers take advantage of their regular visits to a nutrition programme to visit other health facilities nearby. In such cases it is hard to decide which services can be credited with eventual improvements. Such complications are undoubtedly common because it is now generally accepted that nutrition education should not be given in isolation, but supported by other measures. Sound as this principle may be, it does not facilitate evaluation.

One can respond in different ways to the threat of such error. Firstly, one can accept it for what it is, and do nothing about it. The argument in this

case runs that any observed effects are, after all, the results of the intervention irrespective of how they have been achieved. This position is tenable only if such combinations of treatment variables are likely to occur more generally, and if one is careful not to emphasize the education as the sole responsible agent. An alternative approach is to draw a comparison between education programmes, operating along similar lines, so that any variables accompanying treatment are the same in each case. A more rigorous way is to use control groups that undergo completely identical routines. But this takes almost as much time and effort as randomization.

Variables introduced by evaluation

A second source of non-treatment variables is, paradoxically, the evaluation itself, particularly when knowledge and attitude questionnaires are repeatedly used. Respondents often show «habituation» effects: familiarity with the questionnaire may result in improved scores. In one of the studies discussed earlier, for instance, the control group showed the same increase on a scale of nutritional preferences as the treatment group (HOORWEG and NIEMEYER, 1982, p. 37). Indeed, attention factors of various kinds may accompany evaluation. This attention may amount to little more than home visits and interviews, but may also extend to research assistants giving advice and help to people.

Control groups not exposed to treatment but equally often examined serve well to isolate such effects. Another solution is not to visit or examine any person more than once, i.e. not to rely on interviews with the same respondents before and after the education. An example is the comparison of recent and long-time participants in one of our studies discussed above: all were seen only once by the research team. As to the research assistants, strict supervision is always necessary and regular rotation of assistants over different comparison groups may further help to spread such unwanted effects.

Variables introduced through the use of non-equivalent comparison groups

A third source of non-treatment variables lies in the use of non-equivalent comparison groups. By selecting for comparison groups of people who differ in motivation, education, income, age, health etc. virtually any determinant of nutrition behaviour or nutritional status can be artificially introduced, thereby confounding the results. Several authors have listed the many factors that may influence nutrition behaviour and nutritional status (JELLIFFE, 1966; ALLEYNE, HAY, PICOU, STANFIELD and WHITEHEAD, 1977;

HABICHT and BUTZ, 1979; WENLOCK, 1980; ZEITLIN, 1982). These vary from macro-factors such as ecology to micro-variables such as the distribution of food within the household. For our purposes it is useful to distinguish between macro-variables, including ecological and cultural differences, meso-variables, covering differences between households, and micro-variables, causing intra-household variation. In her monograph on village nutrition SCHOFIELD (1979) has used a similar framework for her analysis and reviewed many individual variables. Rather than repeating such a review we will sketch variables in broad outline, together with some possible ways to control them.

Macro-factors cover variables such as ecology, agricultural systems, dietary habits and child rearing practices. Differences between rural and urban living circumstances also fall into this category. The importance of such factors is self-evident, they affect both food supply and nutrition. Such variables generally show little or no variation for an individual throughout his lifetime (unless he migrates elsewhere or marries someone from another ethnic group). Nor do they lend themselves to meaningful quantification.

In general such factors are best kept constant. This means either limiting the study to a particular group or geographical area or treating different subgroups separately in the analysis. By thus eliminating such differences in food and nutrition behaviour they cannot offer rival explanations for observed relations between treatment and outcome. This does not imply that it is impossible to study the role of macro-factors in nutrition education, but this requires an effort beyond the means of most studies.

A second group of macro-factors is less general in nature and shows more fluctuations over time and space. They include seasonal variation, and variables such as water supply and access to medical services. Seasonal variations can be eliminated by carrying out the study over a short period, but this is often not possible for logistical reasons. In that case it is necessary to spread the examination of different groups equally over time to ensure that one group is not examined during one season and another group during another. Variables like water supply and access to medical services are, usually, satisfactorily handled by drawing comparison groups from the same or similar geographical areas.

Meso-factors affecting inter-household differences mostly concern resources and family situation but they also include certain individual characteristics. Variables such as farm size, employment, income, housing, sanitation, family size, marital arrangement, and spacing of children can be mentioned here. Individual characteristics include education, motivation, age and health of parents as well as pregnancy and lactation of the mother.

One way of dealing with such variables is to use separate but similar geographical areas as comparison groups. Thus, the macro-factors mentioned above are kept the same, while it is further assumed that the various meso-

factors are similarly distributed.⁴ However, experience has shown that comparison between different areas often results in spurious differences due to influences which affect whole villages and regions (HABICHT and BUTZ, 1979, p. 150). There is, for example, the possibility that some people migrate to the intervention area to utilize the services provided. In general, any intervention knows unique elements (such as a motivated health assistant or a co-operative village leader) that can substantially contribute to success, but that at the same time distort comparisons between villages. Of the 25 studies, listed in the bibliography for this workshop, using quasi-experimental designs, 13 relied on a comparison of groups drawn from different locations.

Although in this way it may be possible to exclude the individual and household variables as rival explanations for observed effects, they are not eliminated as sources of variance. They still cause considerable variation in nutrition behaviour and nutritional status which can obscure minor effects of the education.

In an attempt to reduce such variance as well, matching procedures are sometimes used, whereby for each individual case one or more comparison cases are selected, identical on certain non-treatment variables. A special type of matching procedure is the use of siblings as a comparison group which is indeed effective to keep most of the variables mentioned until now under control. Of the aforementioned 25 studies, 5 used some kind of matching procedure, and in 2 studies siblings were used. A disadvantage of this procedure is that the siblings nearly always differ in age while it is also difficult to give them different treatments.

Simple and appealing as matching procedures seem, they have drawbacks. Matching reduces not only the variance of the non-treatment variables but of the outcome variables as well, which leads to various statistical restrictions and complications. As anyone with practical experience with this method well knows, only a few variables can be controlled in this way, since it is soon impossible to find sufficient cases with matching characteristics. Furthermore, individual characteristics such as motivation, attitudes and personal competence are difficult to handle in this way.

Two groups of micro-factors require mention. The first concerns intra-household differences in food and nutrition: variables affecting the quantity and quality of foods consumed by individual family members together with other variables causing differences in nutritional status between members of the same household. These variables have only recently drawn the attention of researchers and our knowledge about them is small (see SCHOFIELD, 1979).

4 It has the further advantage that there is less danger that different treatment groups get mixed, a problem mentioned earlier on.

A final group of variables is particularly relevant for the evaluation of child nutrition programmes: genetic differences between children, and the incidence of infections and other diseases. As regards the latter it is usual to eliminate any severely handicapped children and children suffering from chronic diseases from studies. As regards the incidence of infections it is somewhat surprising to note how little effort is usually made to control this factor although its importance for the nutritional condition of children is widely accepted. It must be admitted that this factor as well as genetic variables are difficult to control; even the use of siblings is not always sufficient, and any attempt to deal with them effectively requires extensive examinations.

4. Conclusion

When the first evaluations of nutrition programmes in developing countries started some 10–15 years ago, few of us were aware that there was a larger development in evaluation studies going on at that time, particularly in the United States. Our research colleagues there have a way of rapidly increasing the methodological sophistication of any new field of research.⁵ Researchers in developing countries, on the other hand, often feel squeezed between such refinement and the actual field conditions under which they have to work. Although control of non-treatment variables is necessary in evaluation we know, at the same time, that it usually remains an illusion.

Nevertheless, this does not allow us to disregard the problems that non-treatment variables pose. However difficult the research circumstances prevailing in individual programmes, and however limited the opportunities for evaluation, serious efforts should always be made to deal with them. Any evaluation should take care to find out, beforehand, which factors are locally important in determining nutrition behaviour and nutritional status. Detailed attention should, furthermore, be given to the question which variables can be introduced by the selection of particular comparison groups. Thirdly, whatever design is adopted, sufficient information must be collected regarding the distribution of important non-treatment variables in different groups, and these data must be reported in detail. The degree to which the intended design was realized should be made clear as well as the deviations that have occurred. It is not true that any deviation invalidates results to the extent that they become worthless. Any differences between comparison groups can be taken into account in the interpretation of results and given weight accordingly. Results are much more severely in-

5 See, for example, COOK and McANANY (1979)

validated if no adequate information on non-treatment variables is reported.

Some authors are of the opinion that if evaluation cannot live up to the most rigorous research requirements it is better not done at all (HOUSTON, 1972). This presents a naive view of research, suggesting that social science proceeds from one ideal or crucial experiment to another. Scientific progress, however, is characterized by the accumulation of studies, many with serious imperfections, that nevertheless add to our knowledge and that raise worthwhile questions for further research. In the evaluation of nutrition programmes it is no less justifiable to adopt the same procedure – in spite of inevitable shortcomings.

RIECKEN (1979) has noted that one can only expect the quality of evaluation for which one is prepared to pay. Strict control over non-treatment variables can usually only be achieved at the cost of great financial expense or considerable interference in the running of programmes. Politicians and programme officials must weigh such costs against the insights to be gained from the research. Ultimately, when deciding on the required degree of control over non-treatment variables, the costs and benefits of the evaluation itself should be weighed against one another.

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