Analyzing the Impact of Health Services

Project Experience from India, Ghana, and Thailand

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ABSTRACT

Appraising and evaluating the impact of a health project is highly complex. Several approaches exist, each influenced by the two major disciplines involved—health and economics. Measuring health and its progress—the emphasis of the health discipline—is essential for economic analysis. In addition, economic analysis, which emphasizes comparing benefits with costs, is needed to ensure efficiency in the use of health resources. For both disciplines, therefore, it is important to find reliable health indicators that will show the impact of health services.

Four categories of indicators are pertinent: environment (socioeconomic conditions and health policies); inputs (services offered); outputs (services received); and outcomes (changes in mortality, morbidity, and nutritional status). This paper reviews what three important controlled experiments reveal about these indicators: Narangwal in India, Danfa in Ghana, and Lampang in Thailand. The experiences of three nonexperimental projects—with no control groups—are also reviewed.

Outcome measures naturally are best for indicating the impact of services. But the three controlled experiments reviewed here show that collecting data on outcome variables is expensive, time consuming, and faced with difficulties of controlling for many nonproject influences.

All the projects—experimental and nonexperimental—showed that output indicators served well in indicating the impact of services. At Narangwal, such indicators seem to have captured what the outcome variables showed about the impact of health services.

The appraisal and evaluation of health projects still is a long way from the goals of economic analysis—comparing benefits and costs or assessing whether an approach is the most cost-effective. This paper shows, however, that a beginning can be made in that direction by using outputs of health services as surrogates for outcomes.
ACKNOWLEDGMENTS

My colleagues in the Narangwal research team from the Johns Hopkins University, especially Robert Parker and Cecile De Sweemer, helped this paper with their suggestions and comments. John Evans, Karen Hall, and Nick Prescott gave valuable comments on an earlier draft. Ethna Johnson helped by reviewing materials regarding other experiments and health projects.
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Analyzing the impact of any project in such social sectors as health and education is difficult because the project operates in an environment affected by many outside forces. Observed changes, if measurable, often cannot be precisely ascribed to the various forces causing the changes. Traditionally, two types of impact analyses are done: ex ante and ex post. Ex ante analysis is needed to determine the desirability of the project and to compare its merit with other projects. It is at the heart of project preparation and appraisal. Ex post analysis is needed to monitor and assess whether the anticipated benefit of the project matches the actual benefit. It is at the heart of project evaluation.

In addition to these types of analysis, ongoing monitoring and evaluation (often called a management information system) is also done for better management of the project. This ongoing monitoring and evaluation can also help in reassessing the operational design of the project and in introducing modifications to increase the efficiency and effectiveness of a project. Data collected for this kind of ongoing evaluation are not totally different from the other indicators dealt with in this paper. The focus here, however, is on the impact of health services, analyzed either ex post (for final evaluation) or ex ante (for appraisal).
In agricultural and industrial projects, ex ante impact analysis often is straightforward: economic analyses of internal rate of return, benefit-cost ratios, and cost-effectiveness criteria. These calculations are based on certain assumptions and definitions, which often prove later to be inadequate or inaccurate, but the conceptualization is simple. The cost of the project is considered as investment for the expected outcome in the form of output or income growth. The analysis has to focus on (a) intersectoral efficiency (for the scale of the project), (b) intrasectoral efficiency (for the design and objectives of the project), and (c) intraproject efficiency (for the components of the project). Since all these dimensions are analytically identifiable and measurable under given assumptions, economic analysis is useful for investment decisions.

In health or education projects, however, three problems cripple the analysis of impact. First, most social sector projects have both consumption and investment aspects. That creates a problem because consumption benefits from social services are subjective and hard to measure. Education, health, and family planning services are desired by consumers for the utility they derive from them. Yet these services also improve productivity and reduce fertility, contributing to output growth. In one sense, the objective function of social sector projects is to increase welfare, not merely to increase output or income. This gives rise to the problems of measuring nontangible aspects of welfare and comparing the welfare of one individual or group with another.

The second problem flows from the first and involves the political considerations of social sector projects, considerations that often make
impact analysis inapplicable. Political decisions frequently determine the size and distribution of investment in the social sectors. Although politics also enter into physical sector projects, the difference is in the nature and magnitude. In physical sectors, politicians have much less freedom than in the social sectors. For example, the location of a hydroelectric project is limited by a river's course. There is more freedom in a decision to locate a hospital. On other issues of project design, such as the size and components, the restrictions that economic analysis imposes on a physical sector project are even more binding than those in the social sector.

Third, unlike most physical sector projects, which usually have single goal, social sector projects usually have many goals. This multiplicity of goals in social sector projects makes ex ante and ex post analysis highly complex.

USE OF ECONOMIC ANALYSIS

Dunlop (1980) has recently reviewed the problems of using economic analytical techniques in health projects. He has also noted the meager progress, mostly in the context of developed societies, in the economic analysis of the impact of health projects. The main techniques of economic analysis of projects--cost-benefit analysis, cost-effectiveness analysis, linear programming, and macro simulation--have been used in health projects, but not without problems.

Cost-benefit analysis is a common technique in resource allocation. There are, however, serious problems in using it in projects in the health
sector, as in any other social sector. The most important problem is the conceptualization and measurement of the health benefit. Measuring cost is also a problem, but less so. Katz and others (1963) and Mushkin (1979) have attempted to come to grips with the theoretical issues of measurement and provided guidelines on how to construct a functional health index. These guidelines relate only to the United States and are illustratively applied to specific cases, such as ambulatory and nursing home care. In developing countries, the most common use of cost-benefit analysis in the health field has been to compare alternative programs for reducing mortality or morbidity from a specific disease. Malaria and tuberculosis are among the diseases so analyzed. 1/

Valuing the outcome for cost-benefit analysis is also a serious issue, for without such valuation the benefit cannot be compared with the cost in the health project in question or with benefits across several projects. The most common way to place a value on health outcomes so far has been to use the human capital approach, which measures economic benefit by the discounted value of incremental output attributable to improved health. The incremental output is either the result of preventing premature death or achieving a higher level of productivity by curing of illness. One obvious problem with this approach is that life is valued as the marketable output a person can produce, even in countries with high unemployment. Moreover, this approach gives a low weight to the care of women, the poor, the unemployed, and other disadvantaged groups.

1/ See Kilmer (1971), Olakowski (1972), Winslow (1973), and Dunlop (1975).
The problems of applying cost-benefit analysis to health projects are thoroughly recorded elsewhere. It is enough to mention here that the cost-benefit analysis of projects ignores the element of choice in consumer behavior. Different households are affected by health services differently because households choose to avail themselves of the services differently. So it is important to distinguish the biological and behavioral factors that affect health. To assess the impact of health inputs, it is necessary to understand how the behavioral factors affect the productivity of these inputs differently in the different population groups. Without this understanding, the valuation of the benefits of the health services can be misleading.

As mentioned before, consumers demand good health for consumption and investment benefits. The investment benefit accrues through improvement of productivity from better health. The consumption benefit accrues through the direct contribution of good health to welfare and, therefore, to the utility function of the consumer. Clarke (1979) and Tullock (1976) have attempted to approach valuation by the willingness-to-pay criterion. This is essentially a way to reveal demand. Surveys have been used with limited success to assess demand, but no method exists to get to the willingness-to-pay criterion of valuation.

Because of the problems of valuing health outcomes, cost-effectiveness is often advocated in the place of cost-benefit analysis. Cost-effectiveness is only a partial substitute for cost-benefit analysis because it searches only for the cost-minimization of a given output. It does not allow a comparison with benefits or a comparison with other projects.

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For example, see Scott (1965).
having different kinds of outcome. And like cost-benefit analysis, the analysis of cost-effectiveness also has the limitation of a micro (partial equilibrium) focus, which may be unsuitable if the health sector is examined in a macro framework.

Other techniques, such as linear programming (optimizing health outcomes for given amount of resources) and macro simulation (different health outcomes under different combinations of input) require considerable data. Barlow (1968) and Sheldon and others (1970) attempted to use macro simulation and operations research techniques in evaluating health programs, but with limited success. If the quality of data is dubious, something endemic to the data on health projects, the results of linear programming and macro simulation can be meaningless.

**EXPERIMENTAL DESIGN AND STATISTICAL ANALYSIS**

Two broad approaches exist to tackle the problem of the confounding effects of outside forces—one is experimental design, the other statistical analysis. Experimental design involves a control group (without intervention) and experimental group or groups (with intervention). These control and experimental groups must be comparable in all respects, except for the intervention. A true experimental design calls for random selection of individuals or groups to receive the service or be left out. In social experiments, such selection is not possible, and even the Narangwal project in India, analyzed later in this paper, cannot be regarded as a true experiment.

When experimental designs are used to evaluate health services, impact analysis is simply based on a systematic comparison of the health outcome in the experimental group(s) with the control group. Even if finding
comparable groups with and without health intervention is possible, other predisposing differences may still confound the outcome. Moreover, when groups or villages (as in Narangwal) are selected for experiment, the behavioral problem of self-selection may also arise. Different people may benefit differently from the health services only because they chose to use the services differently (Chernichovsky 1979). The element of choice underscores the need to use a household model to study the effects of the health intervention on the opportunities and objectives of the households—to assess (or even predict) the potential impact of health services.

Statistical analysis helps measure impact by means of a statistical control. In a multiple regression framework, a health intervention variable can be regressed along with other explanatory variables on the measured health outcome. Such a statistical design then controls for the other predisposing variables and accounts for the pure effect of the health intervention. This technique is now frequently used for impact analysis (Grossman 1972; Selowsky and Taylor 1973; Chernichovsky 1979). Because of nonzero correlation between the independent variables, the regression estimates are often biased and can be misleading. There also are troublesome issues of properly specifying functional relations as, say, linear or nonlinear, of identifying the explanatory variables, and so on.

Statistical analysis and experimental design need not be mutually exclusive. In fact, statistical analysis, when applied to the experimental data, enhances the validity of the experimental results. Even in a "true" experiment, impact can be measured (by looking at the means of the outcome variables of the groups) only for the present and for the specific contexts.
To generalize for future and wider applications of a policy intervention, other variables or controls must be used, and for that a multiple regression analysis is helpful.

HEALTH STATUS INDICATORS AND THEIR CHANGE

Health and public health specialists primarily focus on the health needs of people and favor monitoring health to see how these needs are met. Their approach to impact analysis is therefore through monitoring health indicators and their change. They would like to evaluate these changes in relation to predetermined health goals. In 1978, at the international conference on primary health care in Alma-Ata, U.S.S.R., the goal of "health for all by the year 2000" was pronounced, a goal now being pursued by the World Health Organization (WHO).

The Alma-Ata conference raised many issues that are still being debated. What exactly is meant by health? What aspects of health are to be emphasized? A definition of the term "for all" is also still sought. The goal has emphasized primary health care. The idea is to reach more people than has been possible with a curative health structure. But the discussions of "health for all" have underscored the need for suitable indicators of health that can be monitored to see improvement. These discussions are relevant to the development of impact analysis methods for health programs. Several points deserve emphasis.

- The debates and controversies have widened understanding of health indicators and of the ways to monitor progress in
indicators. But developing and understanding these indicators have not led to many practical guidelines that can be used when appraising and evaluating health projects.

The controversies indicate that there are essentially two points of view. Some favor an aggregated index of health; others point out that such an index is not only hard to construct, but counterproductive because it can be misleading about the true status of health.

Most of the discussion of health indicators arising from the declared goal of "health for all by the year 2000" focuses on national indicators within the framework of intercountry comparison. For a health project constituting only a part of the health program of a country, these discussions may not completely apply. But the basic points about the problems and complexities of constructing health indicators and monitoring their changes remain relevant for health project preparation.

Health services are not the only factors determining health; that is, the health system is a broader concept than health services or the health delivery system.

From the discussions and papers on the concepts of health indicators and their monitoring, four important categories of indicators stand out for measuring health and its improvement. 1/ The first category includes indicators reflecting the health environment—what the WHO has termed as social and economic indicators related to health, and indicators of the provision of health care.

1. The WHO—in a recent paper, "Indicators for Monitoring Progress Toward Health for All"—has proposed four categories of health indicators: health policy indicators, health status indicators, social and economic indicators related to health, and indicators of the provision of health care.
conditions relating to health—and health policy measures. 1/ The second category includes inputs to health care services. The third category includes the outputs of health services, usually expressed as use rates. The fourth category includes outcomes, reflecting long-run changes in health status. 2/

The first category includes predisposing variables—the socioeconomic conditions affecting health at the beginning of the project or program. When making cross-country comparisons, these environmental factors are indicators of health in the country. But when evaluating impact of a health project or program, these factors are predisposing variables. Then there are policy variables that may not be totally related to the health project being evaluated but that would nevertheless affect the total outcome of such a project. These, too, are predisposing variables.

The second category, which includes measures of the services offered (purely the supply side), helps in understanding the size and nature of the intervention. Physical accessibility of services is important, but socioeconomic and cultural aspects also are relevant. Physical accessibility

1. The health environment could presumably include some direct determinants of health—knowledge and skills about health—some of which could be introduced by health education.

2. Dunlop (1980) has offered a somewhat similar framework for evaluating health programs. He has suggested that the evaluation framework should consider four categories of variables: constraint, program execution measures, intermediate outcome measures, and final outcome measures.
is often defined by the distance or time needed to travel. Economic accessibility includes the ability of the individual or the community to pay for services. Cultural accessibility implies that using the available services is affected by cultural determinants. For example, in some cultures, health services offered by male workers are not used by female clients. In this situation, only health services offered by female workers would ensure accessibility for female clients.

The third category of variable—output—relates to the use of services, which can be represented by various measures: the per capita average use, the distribution of use among a target population, and so on. The most refined output variable is the effective coverage of services, which is the proportion of people in need of services who receive effective care within a given time, often a year. \(^1\) Examples of such indicators of use are the proportion of children immunized and the proportion of pregnant women who receive prenatal care or have their children under the supervision of a trained attendant.

The fourth category of variable—outcome—reflects the final impact of health services. This variable may reflect overall health status in a population or may include disease-specific variables. Since the

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1. The effective coverage (effective care received by the people in need) can be usefully distinguished from the accessibility cover (people who can use service) or just the availability coverage (people for whom service is available).
conference in Alma-Ata, the discussions about health indicators have brought out the importance of the infant mortality rate as a key indicator of health. 1/

GENERAL LESSONS OF PROJECTS

Three experimental projects—Narangwal, Danfa, and Lampang—offer insights for analyzing the impact of health services. 2/ Narangwal is a village in the Indian state of Punjab; it and other neighboring villages covered by the experiment had a population of 35,000 in 1969. Danfa, a town 20 miles north of Ghana’s capital of Accra, had a population of 60,000 in 1978. Lampang is a province in northern Thailand; its population in 1978 was 660,000.

In Narangwal, a field experiment—in which groups of villages were provided with various combinations of health, family planning, and nutrition services—was carried out during 1968–74. To analyze the impact of services, the Narangwal study used two output measures—service contacts or coverage and the volume (intensity) of use—and three outcome measures—mortality, morbidity, and children’s nutritional status. The Danfa experiment also used different packages of services in four geographical areas between 1971 and 1977. The output indicators were health service coverage and use; the outcome indicators were disease-specific morbidity and mortality. In the Lampang experiment, which is still under way, there are two experimental areas and two control areas. The experimental areas, E₁ and E₂, are based on sequential introduction of services; the two control groups are selected for

1/ James Grant (1978) has suggested an index of the quality of life, in which infant mortality rate figures most prominently.

2/ See the annex for a discussion of these experimental projects and of three nonexperimental projects.
their location—one is in the same province as the experimental groups, the other is outside. The experiment has an elaborate plan of evaluation using all types of indicators—input variables (such as time spent performing tasks), output variables (coverage and use of services) and outcome variables (mortality, morbidity, and nutritional status).

Longitudinal data were collected in these experiments through repeated surveys and service statistics. But in no way do the experiments provide replicable models for the appraisal and evaluation of health projects.

In Narangwal, the outcome variables for mortality and morbidity indicators showed the positive impact of services, but the variation in the rates was not explained by services alone. The results were complicated by the contributions of nonproject forces. A clear picture emerged only through multivariate analysis.

Predisposing variables, such as caste, confounded the results. In every case the experimental groups showed a better result than the control group. How much of the differences between the experimental groups and the control group could be ascribed to project-related factors and to predisposing variables? It is not clear. The mode of services also made a difference in the impact. Services that included more frequent home visits than others reached one section of the population (low caste) more.

Mortality rates were straightforward to estimate, although at high cost because of the need for surveys. Morbidity, however, produced somewhat subjectively determined indexes—for example, days ill. Still, the Narangwal experience showed that outcome variables could be used. But a project needs an experimental design (or at least the use of a valid comparator) and substantial resources for generating the outcome estimates.
A rough estimate of the expenses of data collection in Narangwal was Rs 65 ($8.00) per capita for the population served per year, or about four times of the cost of the most comprehensive package of services in the population part of the experiment. Data collection in Narangwal was most extensive and was designed for much broader research objective than only impact analysis. But if it is assumed that 10 percent of the data collection was for the analysis of outcomes (an assumption based on the experience of the Narangwal researchers who worked in the field), the cost of evaluation would still be prohibitive—nearly 40 percent of the service cost in the comprehensive service villages, more in others.

The experience with outcome indicators in the two other experiments is much less favorable. At Danfa, morbidity and mortality rates were collected to measure the impact of services. A morbidity base line survey was taken, but a subsequent medical examination by the physician showed a weak correlation between information collected and health examinations. Morbidity surveys were, therefore, dropped, and morbidity data were mainly collected from patient visit cards. But problems with this latter method of morbidity data collection were several: under-reporting of health problems, inaccurate diagnosis by health staff, and failures to include serious cases that bypassed the health center for the hospital. Mortality data were collected through surveys and a continuous registration of vital events. The collection of mortality data also had the problem of inaccurate reporting. An evaluation team recommended that less emphasis be placed on outcome indicators to measure performance. Consequently, intermediate or process indicators (output) were emphasized.
At Lampang the experience also shows ironically that the evaluation data collected so far may be more than is needed for project analysis. In addition, the collection of data is one of the many steps before findings are available. The time to process and analyze the data is of major importance and the time to process data has been as long as a year for some of the study components. Future data collection will be streamlined to include only essential items. Despite the elaborate evaluation design in Lampang, there have been many problems in the collection of mortality and morbidity data. These include incomplete records of vital events, small sample groups, and the difficulties of the household members to remember illness.

The experiences of three nonexperimental projects—Panama Primary Health Care Program, Bohol Maternal and Child Health-Family Planning project in the Philippines, and the Under-Fives Clinics in Ilesha, Nigeria—are similar to Danfa and Lampang. These nonexperimental projects also had problems with outcome variables. In Panama, interim evaluation used only output variables to obtain some feedback on how primary health care services were generally used and which components of the services were favored. In Bohol, before-and-after surveys were held to observe changes in outcome variables. No suitable comparator was available, however, so, no meaningful evaluation with the outcome data was possible. At Ilesha, there was some success with outcome variables (generated from household surveys and compared with comparable nonproject areas). But the cost of evaluation was high, and one of the conclusions from the evaluation is that because health outcomes are slow to change, it often is necessary to arrange for funding to evaluate outcomes after a project has been completed.
All six projects reviewed show that output variables—such as service coverage, service use, and proportion of target population served—can show the impact of projects. In the experiments, ready comparison with control groups was possible and revealing. Even in cases where control groups are not available, data from areas with similar socioeconomic characteristics can provide benchmarks for comparison (for example, Ilesha).

What is needed is a demonstrated correlation between output and outcome variables. These correlations can be estimated from the controlled experiments reviewed here. Once established, the correlation would make it possible to approximate from the output the values of outcomes. Also needed is a better understanding of the relation between the carefully designated control groups and other quasi-control groups (national averages or averages for similar groups without intervention). Again, field experiments can help establish that link.

In conclusion, the appraisal and evaluation of health projects still are far from the goal of economic analysis—reviewing the relative efficiency of different approaches to reach quantifiable targets of health improvements. Outcomes, though quantifiable, cannot be linked to the inputs needed to reach the target, as has been learned from the experiences reviewed in this paper, or to different strategies to reach the outcome. But output indicators can be useful surrogates for outcomes, and the inputs needed for the outputs can be verified from experience or from elsewhere. Obviously, there is a long way to go before the goals of economic analysis of health projects can be reached.
ANNEX

Project Experience
At Narangwal, in the Indian state of Punjab, a field experiment was carried out by the Department of International Health of Johns Hopkins University, between 1968 and 1974. The Narangwal field experiment and research had two parts: a population study, an in-depth examination of the outcome of integrating health services with those for family planning; a nutrition study, which considered the interaction between malnutrition and infections in children under three. Groups of villages were provided with various combinations of health, family planning, and nutrition services; the households in each group were observed over time.

The primary focus of the Narangwal population study was to understand whether and how the integration of family planning with different components of health care would increase the acceptance of family planning and lead to a decline in fertility. But the effects of services on health were also analyzed. The primary focus of the Narangwal nutrition study was to analyze the interaction of malnutrition and infection and trace the effects of combining various health and nutrition services.

Output and Outcome Indicators

There were two output measures in the Narangwal study: service contacts or coverage and the volume (intensity) of use. And there were three outcome measures for health and nutrition services: mortality, morbidity, and children's nutritional status. The service contacts or coverage, whether at home or a clinic, were obtained from the detailed patient and service records.
These were then standardized by expressing them as visits per week per 1,000 population. The paramedical workers were responsible for between 90 and 95 percent of health service contacts, which fell broadly in two categories—children's health services and women's health services. In a project area, the availability of nonproject services complicated the determination of project impact. In the Narangwal analysis of service contacts, the proportion of individuals who received some form of health care was included and the source of such care was identified to indicate the relative role of the project source. Table 1 shows the service coverage by project in relation to the total needs identified and in relation to other sources of service.

The emerging pattern of service coverage in relation to need shows that integrated services generally achieved generally more coverage. The percentages in table 1 also help show the coverage in areas with and without project services. But they do not fully capture the relative contribution of the project services, which often substituted for nonproject services. If the project services are of better quality, the substitution effect should indicate an improvement and a desirable impact of the project services. At Narangwal, the extent of substitution was also ascertained. It was found, for example, that about half the children's services previously provided were substituted by project sources in villages with combined family planning and children's services.

The volume or intensity of use was the second output indicator used in the Narangwal analysis. For example, the average number of "illness" visits for each woman using services in 1969 was 4.0 in villages with integrated family planning, women's, and children's services and 5.1 in villages with
Table 1: TREATMENT FROM DIFFERENT SOURCES OF CARE, BY EXPERIMENTAL GROUP, 1973-74

<table>
<thead>
<tr>
<th>Experimental groups and source</th>
<th>Percentage of ill individuals receiving care from different sources</th>
<th>From project sources</th>
<th>From nonproject sources</th>
<th>Total</th>
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<tr>
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<td>Integrated family planning, women's, and children's services</td>
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<td>Women's services</td>
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<td>42</td>
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<tr>
<td>Children's services</td>
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<td>41</td>
<td>17</td>
<td>58</td>
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<tr>
<td>Combined family planning and women's services</td>
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<tr>
<td>Women's services</td>
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<td>47</td>
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<td>Children's services</td>
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<td>3</td>
<td>34</td>
<td>37</td>
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<tr>
<td>Combined family planning and children's services</td>
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<tr>
<td>Women's services</td>
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<td>11</td>
<td>28</td>
<td>39</td>
</tr>
<tr>
<td>Children's services</td>
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<td>34</td>
<td>14</td>
<td>48</td>
</tr>
<tr>
<td>Control group</td>
<td></td>
<td>-</td>
<td>26</td>
<td>26</td>
</tr>
<tr>
<td>Women's services</td>
<td></td>
<td>-</td>
<td>31</td>
<td>31</td>
</tr>
<tr>
<td>Children's services</td>
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combined family planning, and women's services. Similarly, the average number of visits for child care was 30 in villages with integrated services and 47 in villages with combined family planning and children's services.

Mortality was analyzed as a measure of outcome. Only the death rate of children under three years of age was analyzed because the children's services concentrated on children under three, and only for children under three were there enough deaths to make meaningful analysis possible. An important measure of the effect of women's services would have been changes in maternal mortality indicator was not feasible because there were few maternal deaths.

Mortality rates were calculated for children by age—stillbirth, infant mortality (neonatal under one month and postneonatal from 1 to 12 months), and children's deaths between one and three years. The stillbirth rate was 23 to 35 percent lower in the villages with services than in the control villages. Although there were fewer deaths among children aged 1-3, the impact of services on these deaths was as important as it was on deaths at an earlier age, and caste played an important role in determining the impact of services.

For evaluating project impact, a tradeoff thus existed between the qualitative and quantitative impact of children's services when combined with women's health services or offered separately. When combined with women's health services, home visits were less intensive, with the higher caste group benefiting more than the lower. When home visits were more intensive with fewer services, more coverage of the low caste was possible.

In the Narangwal context, caste differences indicate the socio-economic status of households, a strong predisposing variable influencing the impact of children's health services.
To illustrate that predisposing variables are important, I note here the different effects of services on death rates among different castes. Only a few children of either high caste and low caste did not receive treatment for illness that led to death. But high-caste families tended to seek care earlier than did low-caste families. Sixty-eight percent of high-caste children who died received care in the first twenty-four hours of their illness, compared with about 50 percent of the low-caste children. These different rates in seeking services explain why accessible services have an impact on those who seek these at their own initiative. In cases where services are rendered through home visits in intensive coverage, the lower caste groups benefited relatively more.

In the nutrition study of the Narangwal experiment, mortality rates were also used to assess outcomes of services. The prenatal mortality was reduced significantly more in villages receiving only nutritional supplementation (31 per 1,000 live and still births) than in villages receiving only health care (45 per 1,000 lives and still births) or control villages (57 per 1,000 lives and stillbirths). This difference in mortality probably resulted in part from better nutrition of all mothers from the iron and folic acid supplements. But it was not possible to distinguish this effect clearly from another influence--food provided during pregnancy. A mother received food if workers decided she was nutritionally at risk. This illustrates the problem of the cases where the discretion and different treatment by health workers complicate decisions about what services, if any, cause a better result.

Neonatal, postneonatal, and 1- to 2-year old child mortality were reduced by a third to a half in villages where infectious disease control
services were provided (either alone or with nutritional supplementation). Villages receiving only nutritional supplementation had an intermediate effect for under 1 year of age, and an equivalent effect on mortality among children age 1-2.

Morbidity was another important outcome variable used in the Narangwal analysis. For some groups in the Narangwal experiment, morbidity surveillance was weekly. Data on the incidence of illness among children in the experimental group receiving combined family planning and children's services and in the control group villages and can be used to measure the effect of children's services on morbidity rates. The morbidity indicator used is the average duration of episodes of seven specific illness in control and the other villages where children's services were offered: fever, cough, pneumonia, diarrhea, vomiting, eye infections, and skin infections. These were selected because of their frequency and importance for children under three. For each illness, the average duration was less in the experimental villages than in the control villages. The differences range from 14 percent to 33 percent.

The measures were further refined. Using the above durations and average incidence rates for each illness, the total annual days of illness per child were estimated for infants under one year and for children aged 1-3. The result show that combined family planning and children's services reduced the amount of illness by twenty-two days a year in each age group. Sickness was reduced 16 percent for those under one year and 21 percent for those aged 1-3.

The same morbidity indicator was also used in the nutrition study, in which health care caused a significant reduction in the average duration of infectious diseases.
The indicator of nutritional status—growth of children—was also used. The Narangwal research results show that nutrition care, alone and when combined with health care, improved both the height and weight of children at three years of age. Such children weighed on the average 560 grams more and were 1.3 centimeters taller than children in control villages. Children in villages receiving only health care had mean weights and heights between those in the villages receiving nutritional care and control villages.

Among the many socioeconomic and demographic variables tested, sex and caste were shown to have an especially pronounced independent effect, which averaged 600 to 750 grams in weight and about 2 centimeters in height.

**Multivariate Analysis of Narangwal Household Data**

Since the primary focus of the Narangwal population study was the impact of integrated services on family planning, the multivariate analysis used family planning acceptance and practice as dependent variables. In the nutrition study, however, a multivariate analysis of the household data was done with nutritional status of the child (weight in kilograms) as the dependent variable and with the nutrition and health services, along with socioeconomic variables, as explanatory variables.

The regression results from the Narangwal nutrition study are significant. The estimated effects on child weight of various interventions, without controlling for the predisposing variables, produce a low explanatory power (only 6 percent of the variation in the dependent variable is explained by the independent variables). The signs of two of the three service variables are negative. This implies that a crude comparison of the mean outcomes of an
experiment can produce a confusing result. The nutritional supplementation has a negative effect on child weight and medical care, and combined services of medical care and nutritional supplementation had no effect. When such predisposing variables as age, sex, and caste are controlled for in the regression, the picture changes completely. There is a substantial increase (71 percent) of the explanatory power of the regression equation. Three service variables show positive and significant effects on child weight. The regression results confirm that controlling for predisposing variables is crucial in evaluating the impact of services.

DANFA, GHANA

The field experiment had different packages of services in four geographical areas:

<table>
<thead>
<tr>
<th>Area</th>
<th>Comprehensive health care</th>
<th>Health education</th>
<th>Family planning</th>
<th>Standard ministry of health services</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>II</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>III</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>IV</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>
In the range of potential indicators to evaluate impact of services, many similarities exist between Narangwal and Danfa. Among the input variables, functional analysis (analysis of workers' time) was included primarily to increase efficiency of services. On the basis of the analysis, community health nurses were trained to treat some common maternal-child health complaints. This enabled the physician to concentrate on more serious health problems reported. Based on the analysis, some services (health clinics for pregnant women and for children under five) were increased.

Among the output variables, indicators of health service coverage and use were used. The simple indicator of coverage showed the effect of the Danfa project services. In 1971 only a third of the population was covered. By 1973, 85 percent of the population was covered. The number of visits to clinics in the experimental area doubled during the same period. Use and coverage of child and prenatal health services increased substantially between 1972 and 1977. By 1977, 77 percent of children under five and 53 percent of pregnant women were being seen at least once each year, compared with 46 percent and 29 percent in 1972.

Among the outcome variables, morbidity and mortality rates were used to measure the impact of services. A morbidity base line survey was based on "days of illness and restricted activity." A medical examination by the physician and laboratory studies were also undertaken. A weak correlation was found between information collected and the simultaneously administered health examinations. It was not considered useful to repeat the morbidity survey at the end of the project. Morbidity data were also collected from patient encounter cards at the main health center or at subcenters and from epidemiological studies such as the malaria study.
The Danfa project examined disease-specific morbidity rates, particularly among mothers and children. In the absence of specific morbidity surveys, the project relied on health center records to determine the problems in maternal morbidity. The project could not show any significant trend in pregnancy-related complaints, and data interpretation was difficult. Problems experienced with collection of morbidity data included underreporting of health problems, inaccurate diagnosis by health staff, or not including serious cases that bypassed the health center for the hospital. Health problems may diminish but an increased number of people may come to the health center when health staff members are better qualified to diagnose and refer cases. The project was unable to measure changes in maternal morbidity.

Child morbidity data were collected from patient encounter cards, health surveys, and special studies. Several problems were apparent. Service encounter records cannot capture acute events that are not brought to the clinic--such as neonatal tetanus, which is so critical that the infant dies or is taken directly to the hospital for treatment. And many childhood illness may be misdiagnosed, so that records overstate or understate the true incidence of disease.

In the Danfa project, the frequency of malaria was measured by conducting a special survey of malaria parasitemia before and during the chemoprophylaxis program. The surveys showed that there was a 28 percent reduction in parasite rates and a 54 percent reduction in parasite density in the blood of children under six in area I, compared with the control area. The different rates cannot be attributed to program impact because of baseline differences in the two groups. The village health surveys in 1973, 1975, 1977 provide information on nutritional changes over time. The data show a similar improvement in all four research areas, including those having no nutrition program.
Other childhood diseases were recorded by health center staff on service encounter cards, but here too there was no village-based system to monitor changes in the incidence of diseases. The impact of the immunization campaigns against measles and whooping cough could not be determined because of the low incidence at the baseline. Other disease-specific incidence rates did not show any significant declines, but in the case of hookworm and ascariis there was a fall in prevalence rates among those under 5 years. Similar changes occurred in the other research areas.

Mortality data was collected by census and annual resurveys and by the continuous registration of vital events. The project also tried to collect information on the cause of death, but this was unsatisfactory. Longitudinal fertility surveys were analysed to provide an improved data base. The crude birth rate declined 33 percent between 1971-72 and 1975-76 in area I. Mortality may have been underreported, in which case the decline would be even greater. No significant reduction in mortality rates occurred in areas III or IV of the project.

The estimated reduction in child mortality was between 19-27 percent. The baseline child mortality rate was lower than expected (possibly because of underreporting), but even by adjusting rates upward, a decrease in child mortality rate of 19 percent was shown. Although the observed reduction in mortality rate was 27 percent for area I, "no statistically significant trends could be discerned in the other three project areas."

Infant and preschool mortality rates were disaggregated from child mortality rates for the project area. No significant change was observed
for any of the project areas. From 1972-73 to 1976-77 the rate in area I varied from 57 per 1,000 children under one year to 71 per 1,000.

The mortality rate of the preschool age group (1-4) fell 67 percent from 1971-72 to 1976-77: that is, from 16 per 1,000 children aged 1-4 to 5.3 per 1,000. The significant reduction observed in area I was not observed in the other three areas, but mortality rates for those areas also declined. If deaths of children aged 1-4 were underreported, the estimated reduction ranges from 54 percent to 67 percent.

The project tried to estimate the rate of pregnancy-related deaths: that is, deaths among women within 3 months of any delivery. Analysis of available data shows maternal mortality of "approximately 4 deaths per 1000 live births but with wide confidence intervals." The rate varied a great deal, and statistically significant changes could not be shown.

The size of error that may have occurred in the collection, recording and processing of mortality and morbidity data is not known. So the findings are inconclusive. The trend in death rates among preschool children and the reduction in malaria and malnutrition were significant in area I.

Several problems arose in data collection and analysis:

- There was a high turnover of population in the project areas from in- and out-migration, and this made obtaining information about vital events more difficult. Maintaining the desired sample size in the longitudinal studies was also difficult.

- It was hard to estimate maternal mortality rates accurately because of: (a) mobility of women, (b) inaccuracies in recording causes of death, (c) lack of pregnancy status
information on deaths each year, and (d) variability in the number of deaths each year.

- Vital events were incompletely reported in the project areas.
- Because socioeconomic levels were different in the four project areas, and given demographic problems, the revised project paper in 1976 emphasized proxy or surrogate indicators for measurement, although morbidity and mortality data had been collected throughout the study.

**LAMPANG, THAILAND**

The Lampang experimental project was designed to test a model for developing a low-cost health delivery system that could be replicated within the rest of Thailand. The project is an integrated rural health-care delivery system designed to reach and serve at least two-thirds of preschool children and women of child-bearing age.

In Lampang there are two experimental areas and two control areas. The experimental areas, E₁ and E₂, are based on sequential introduction of services. The first intervention area, E₁, had services in 1974-76 and included only one district (Hang Char). The second intervention area, E₂, covers seven districts between 1976-78. One of the two control groups is in the province where the experimental groups are located, the other outside. The evaluation of the health delivery system in Lampang has focused on preproject baseline measurements and on monitoring changes during the intervention period. Since the experiment is still continuing, final evaluation of the

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1. There is also a third intervention covering 4 districts, giving services for the period 1978-79. We have not considered the third intervention area in this analysis because no data have yet been used for the interim evaluation.
impact of services will be undertaken later. Pending the final evaluation, I review here the experience of using indicators in the interim evaluation.

A variety of data sources were used to collect the information needed to construct the specific indicators. Statistics on services and information project management were obtained from the provincial health information system. Other data—including health practices, health status measurements, service coverage, and service use—were obtained from surveys and special studies initiated by the project’s evaluation and research division.

Among the input variables, availability of services showed differences between the experimental and control groups. Service availability remained constant in the control group during 1974-78, whereas this increased significantly in experimental groups.

Service use indicators, constructed on the basis of surveys, were used. The data suggest a low level of government health service use for illness in the preproject period. As new categories of workers were deployed, a marked gain in service use was observed in the rural areas. The most dramatic gain was in health center visits.

**Impact of services.** Baseline health indicators were determined at the beginning of the project but the findings from followup surveys, conducted in 1980, are not reported yet.

**Mortality and morbidity.** The morbidity-related data collected through baseline Community Health Survey had several weaknesses. The data were collected by questioning the head-of-household about illness symptoms in the two weeks before the study. No clinical examination was undertaken, so figures represent "apparent morbidity." Maternal morbidity estimates were
also difficult to secure but the Community Survey interview included a question for women who delivered during the two-year period before the survey. A question regarding "abnormal symptoms" was included but this does not confirm morbidity, and in addition, recall of events has a number of weaknesses.

**Nutritional status of children.** The baseline data collection included a complex nutrition survey planned for the early stages of the project in 1974-75. But no valid baseline survey data are available because of the serious problems faced in taking these surveys (such as the shortage of technical staff needed to conduct a reliable nutrition survey). Later the nutrition survey was reduced to collecting only anthropometric data, and food habit data and the Child Health and Nutrition Surveys (CHNS) were combined. This reduced the need for highly skilled technical personnel.

The later surveys are not strictly comparable to the other Community Health Survey data collected in the beginning. In April and May 1977, the CHNS was carried out in the E2 study area; a sample of 1500 preschool age children were surveyed for age, height, and weight. In comparison to this, a nutritional surveillance program was introduced in E1 study area. Surveillance included the weighing of all preschool children in the district. The results of this surveillance show weight-for-age figures, using Thai standards, were similar to the E2 study area. Since the nutrition data from E1 and E2 areas are not comparable to the baseline, no estimate of change can be made; however, comparison with control areas regarding the current status of nutrition should be possible when nutrition data from the control areas become available.

In sum, the Lampang experimental project has an elaborate evaluation design, most of which has not yet been implemented. Results of final surveys
are not yet available. Since its design is elaborate, I list in Table 2 all the variables used, or likely to be used, for the Lampang project evaluation. Despite the elaborate evaluation design in Lampang, there have been many problems in the collection of mortality and morbidity data. These include incomplete records of vital events, small sample groups, and difficulties of household members in recalling illness.

Some problems arose because of the elaborate evaluation design. Responsibility for the broad range of evaluation tasks was diffused among several institutions and agencies. Coordination of the various evaluation components has become difficult. Nonproject professional staff, including part-time consultants, were involved in evaluation. In the early stages of the project, clearly defined evaluation needs and methods were not developed, and consultants developed their own approaches based on their perception of needs and general discussion of project needs. As evaluation requirements were identified, some adjustment in individual studies were made.

Some of the indicators were developed on the assumption that necessary data would be available from at least one of the evaluation components that had already begun. Since most of the evaluation components had been planned and were under way when the final evaluation plan was completed in 1976, indicators listed in the plan are not available from among a number of study components. This implies that even in an elaborate design, approximate and timely data collection can be a problem. Now the project staff are reviewing the problem and exploring the possibility of collecting additional data to ensure that data needs are met. They are also reviewing whether particular indicators outlined are needed.
<table>
<thead>
<tr>
<th>Task and Cost Analyses</th>
<th>Coverage and Utilization</th>
<th>Outcome Indicators</th>
</tr>
</thead>
<tbody>
<tr>
<td>(a) time spent performing tasks</td>
<td>(a) target population served</td>
<td>A. Changes in Fertility Levels</td>
</tr>
<tr>
<td>(b) work-time spent in providing services by facility and worker</td>
<td>(b) % of deliveries assisted</td>
<td>(a) crude birth rate</td>
</tr>
<tr>
<td>(c) variation of time utilized to perform each service function, etc.</td>
<td>(c) % receiving pre-natal care</td>
<td>(b) general fertility rate</td>
</tr>
<tr>
<td>(d) additional service capacity of the system</td>
<td>(f) % receiving post-natal care</td>
<td>(c) age-specific fertility rate</td>
</tr>
<tr>
<td>(e) overall government cost per unit service contact, by service and facility</td>
<td>(g) family planning acceptors</td>
<td>(d) % of first births and distribution of all first orders</td>
</tr>
<tr>
<td>(f) consumer cost per episode of illness and estimated total for health per year</td>
<td>(h) nutrition surveillance, nutritional care, etc.</td>
<td>(e) average open birth interval</td>
</tr>
<tr>
<td>(g) cost of basic government health services per capita</td>
<td></td>
<td>B. Changes in Mortality and Morbidity Rates</td>
</tr>
<tr>
<td>(h) overall provider cost by type of facility and total expenditure by service category</td>
<td></td>
<td>(a) infant mortality rate</td>
</tr>
<tr>
<td>(i) health worker to population ratios</td>
<td></td>
<td>(b) child mortality rate</td>
</tr>
<tr>
<td>(j) proportion of referred cases</td>
<td></td>
<td>C. Changes in Nutritional Status</td>
</tr>
<tr>
<td>(k) proportion of referred cases completed</td>
<td></td>
<td>(a) proportion of under 6 years old malnourished</td>
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<td>(l) length of hospital stay</td>
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</table>
NONEXPERIMENTAL PROJECTS AND PROGRAMS

In this section are the experiences of a few nonexperimental projects. Analyzing the impact of health services is even more difficult when there is no control group to compare the results obtained from the groups subjected to intervention. Table 3 records three experiences: the Panamanian Primary Health Care (PHC) Program; the Bohol Maternal-and-Child-Health-based Family Planning in the Phillippines; and the Under-Fives Clinic in Ilesha, Western Nigeria. In most cases the evaluation reports suggest that, according to most of these indicators, the projects had significant impact.

In the case of the evaluation of the primary health care (PHC) in Panama, the objective was to describe the initial impact of the program and to establish bench mark for future studies. No comparators were used to assess the results. The evaluation showed that 53.2 percent of the families sought medical help from the PHC program. Of the cases using the medical help, 54 percent declared that the help was 'always effective'; 41 percent said the help was 'sometimes effective'; only 5 percent considered the help 'ineffectual.' The evaluation also showed that curative services of the PHC program were used more; sanitation activities had good participation; but preventive actions obtained attention well below expectation. This interim evaluation, thus, mostly used output variables with the purpose of obtaining some feedback--how the PHC services were generally used and which component of the services were favored.

Evaluation was an important part of the Bohol project, where extensive data on 'output' and 'outcome' variables were collected. 'Before' and
<table>
<thead>
<tr>
<th>Project</th>
<th>Type of Data</th>
<th>Indicators Used to Evaluate Project</th>
</tr>
</thead>
<tbody>
<tr>
<td>Primary Health Care Program, Panama</td>
<td>General Socio-Economic data. Output and Outcome data Quality of Service data</td>
<td>(Community and family member data were collected by a sample survey of households of all the rural communities involved in the program). 1. Data related to community and family members. 2. Mortality data over the last year. 3. Family morbidity over the previous two weeks. 4. Maternal child care. 5. Community organization and head of households participation. 6. Head of household knowledge and participation in health assistants activities. 7. Satisfaction with health assistants behavior. 8. Socio-economic data related to possessions, economic activities, housing conditions, contact with mass media. 9. Perception of the health assistants activities most important tasks, performance of responsibilities, etc.</td>
</tr>
<tr>
<td>Bohol Maternal- and-Child-Health-based Family Planning Project (MCH-FP) Philippines (1974-79)</td>
<td>Primarily Input and Output Data</td>
<td>(a) General Health Services 1. Number of service providers, trained staff clinics. 2. Number of health staff with transportation. 3. Number of clinics with basic equipment. 4. Number of villages visited by audio-visual teams. 5. Extent of community participation. 6. Number of non-MCH/FP services provided monthly. (cont'd)</td>
</tr>
<tr>
<td>Project</td>
<td>Type of Data</td>
<td>Indicators Used to Evaluate Project</td>
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<tr>
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<td>---------------------------------------</td>
<td>-------------------------------------</td>
</tr>
<tr>
<td>Bohol Project</td>
<td>Primarily Input and Output data</td>
<td>(b) Maternal Health</td>
</tr>
</tbody>
</table>

1. % of priority women identified by name.
2. % of women receiving at least one health service.
3. % of traditional birth attendants trained.
4. % of pregnant women who consulted health staff during pregnancy.
5. % of pregnant women receiving pre-natal care early in pregnancy.
6. % of pregnant women receiving basic package of pre-natal services.
7. Average number of prenatal visits per woman.
8. % of pregnant women receiving FP advice during pregnancy.
9. % of trained birth attendants delivering in which bamboo sliver was used to cut umbilical cord.
10. % of women practicing FP after delivery (by type of care and attendant).
11. % of women getting post-partum care.
12. % of pregnant women seen by trimester.
13. % of deliveries by type of attendant (doctor, nurse, midwife, trained or untrained birth attendant).

(cont'd)
<table>
<thead>
<tr>
<th>Project</th>
<th>Type of Data</th>
<th>Indicators Used to Evaluate Project</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bohol Project</td>
<td>Output and Outcome Data</td>
<td>(c) Child Health</td>
</tr>
<tr>
<td></td>
<td></td>
<td>1. % of priority children identified by name.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>2. % of priority children given at least one health service.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>3. Number of sick and well children and infants seen monthly.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>4. % of priority children given BCG and DPT immunizations</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5. Mean age when children first attend clinics</td>
</tr>
<tr>
<td></td>
<td></td>
<td>6. % of last liveborn children who were breastfed and mean duration of breastfeeding.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7. % of children who are severely malnourished (0-6).</td>
</tr>
<tr>
<td></td>
<td></td>
<td>8. % of infants of low birth weight.</td>
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<tr>
<td></td>
<td></td>
<td>(d) Morbidity</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Number, type and duration of illnesses in households in specific period.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(e) Fertility</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Crude birth rate and other fertility rates (age-specific, total gross reproduction rate) of ecological areas.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(f) Mortality</td>
</tr>
<tr>
<td></td>
<td></td>
<td>1. Crude death rate</td>
</tr>
<tr>
<td></td>
<td></td>
<td>4. Infant mortality rate</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5. Number of stillbirths</td>
</tr>
<tr>
<td></td>
<td></td>
<td>6. Fetal mortality rate</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7. Number of deaths due to Tetanus Neonatorum</td>
</tr>
</tbody>
</table>

(cont'd.)
<table>
<thead>
<tr>
<th>Project</th>
<th>Type of Data</th>
<th>Indicators Used to Evaluate Project</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>2. Vital events registration over a one year period</td>
</tr>
<tr>
<td></td>
<td></td>
<td>3. Utilization of services.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>4. Immunization status of children.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5. Interview with mothers regarding child health.</td>
</tr>
<tr>
<td></td>
<td></td>
<td>6. Laboratory and special examination on a subsample of children.</td>
</tr>
</tbody>
</table>
'after' surveys were conducted to observe changes in 'outcome' variables. In addition, extensive household data on clients were gathered, and a few small studies were done on selected aspects of service delivery. The results of the evaluation showed that the Bohol project achieved significant improvement in output and outcome variables; no suitable comparator was used, however. The experience of the Bohol project shows that household surveys are needed in addition to service statistics to generate dependable output data (Williamson 1980).

The Under-Fives Clinics at Ilesha focused on the health of children under five by providing prevention and curative health services, as well as by supervising all children under five. The purpose of the evaluation (using household surveys, vital records, clinic records, and so on) was to find out how far the goals of the project were attained. For this evaluation, a comparison was made with another nearby area, which was not covered by the project. The results showed that in outcome, the health of project-area children was demonstrably better than that of children in the nonproject area. Mortality rates among children 1-4 years old were significantly lower. Among survivors, there were significant differences in growth, as indicated by percent of standard weight for age. And in height for age, upper arm circumference, and so on, the children in project area scored higher. Similarly in the incidence of disease, the children in the project area did better (for example, 14 percent had malaria in the project area compared to 52.5 percent in the nonproject area). The output data (immunization roles, percent of eligible children registered and attending clinics, and so on) also showed
that the project area was doing demonstrably better than the nonproject areas. Despite some success with outcome variables (whose results are consistent with output variables) the cost of evaluation was high, and one of the conclusions reached from the evaluation is that because health outcomes are slow to change, it often is necessary to secure funding to evaluate outcomes after the project. Evaluation need not wait, however, till the health outcomes can be measured and a stable source of finance is assured (Cunningham 1980).
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