International Journal of Epidemiology

Vol. 22, No. 5 (Suppl. 1)
Printed in Great Britain

A Critical Analysis of the Design, Results and Implications of the Mortality and Use of Health Services Surveys

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The papers in this supplement present a comprehensive assessment of the Mortality and Use of Health Services (MUHS) surveys designed to document the impact of the Combatting Childhood Communicable Diseases programme in sub-Saharan Africa. The papers are a testament to the spirit of genuine scientific inquiry that pervaded all aspects of this body of work. In this paper, we provide an independent view of the design, results and conclusions of the MUHS surveys. Our commentary falls naturally into three components: design and methods, interpretation of the results, and implications for future studies to evaluate with precision the impact of health interventions in difficult field conditions.

DESIGN AND METHODS

Three issues are addressed: which are the appropriate indicators of intervention impact; how can short-term changes in mortality best be measured; and how can short-term changes be attributed to specific interventions in the setting of increasingly rapid socioeconomic change?

Indicators of Outcome

Ewbank[1] presents three purposes for programme evaluations: first, to prove efficacy of a new technology such as ORT or measles immunization in carefully controlled clinical trials; second, to evaluate the performance of routine application of technologies in national programmes, and third, to demonstrate the impact on mortality and morbidity of these technologies applied in real world conditions.

The importance of the first form of evaluation for new technologies such as vitamin A supplementation or new vaccines is widely appreciated. Likewise, a considerable literature is emerging on the third type, routine evaluation of health services using programme process indicators.[2,3] The series of papers in this supplement are a good example of the second type of evaluation. We are in full agreement with Ewbank[1] that there will always be a need to examine the health impact of new technologies as they are applied in real world conditions. Technologies proven in carefully controlled clinical trials may not have the expected or hoped for impact in wider applications. The technology may be fragile, competing health risks may dramatically reduce the ultimate benefit, and unexpected interactions between different diseases and the host may be important.[4,5]

For the third more exacting form of evaluation, what are the most appropriate indicators of health outcome or benefit?[6] Mortality change as the key criterion for the assessment of programme impact has many appealing features. It is an unambiguous measure of impact, largely free of subjective bias which is not the case for morbidity measures. It is a gross measure, however, and therefore encompasses both the direct ('programme' in family planning evaluation jargon) as well as the indirect ('non-programme') effects. But it avoids many of the technical complications inherent in other measures which describe qualitative changes such as improvements in the delivery of care.

Health interventions can have significant health benefits beyond mortality reduction. Improvements in the quality of life or reductions in suffering through lower rates of morbidity are worthy outcomes in themselves. Measures of change in health status may also be more sensitive to recent improvements in health conditions. As argued below, mortality is a difficult yardstick for the measurement of short-term changes in health. On the other hand, there are a host of measurement problems associated with using morbidity

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indicators to evaluate health impact; these are discussed at length by Murray and Chen.[7] In brief, there are as many technical problems with this strategy of morbidity and disability measurement as those outlined below for mortality.

How should we measure short-term changes in mortality? There are two quite separate dimensions to this problem: the problems associated with the measurement methods themselves, especially those based on responses to retrospective questions put to mothers during household surveys; and difficulties with data collection in field conditions. The method which is likely to be most satisfactory for impact evaluation requires the full reporting of births and deaths prospectively over a number of years as in a long-established vital registration system, where the population at risk by age and sex is also known. Unlike the retrospective methods to be discussed, these 'current' measurement systems have fewer problems of dating events accurately. Full vital registration systems do not exist at the national level in sub-Saharan Africa. It is nonetheless an ideal to which assessors of intervention programmes have to aspire, since the alternatives are less trustworthy because of methodological flaws and the often unpredictable difficulties with the collection of reliable information.

The main alternative to a complete vital registration system is to measure mortality directly by recording all births and deaths over a period of years in restricted areas. This can be done in two different ways. One is to establish a system of continuous demographic surveillance, as has been successfully done in areas such as Matlab, Bangladesh,[8] Keneba, Gambia[9] or Niakhar, Senegal.[10] The alternative is to collect information on these vital events with one or a series of retrospective surveys. The continuous recording method is more demanding and cannot be applied in very large populations, but the resulting data are generally more reliable and richer in detail than similar information collected retrospectively. Unfortunately, the chances that the act of measurement may change the phenomena being studied are also greater.

As a less expensive alternative, many programme managers resort to household surveys to measure mortality levels and trends. These usually involve a survey pre-intervention followed by a second survey after a number of years of programme implementation. Births and deaths are calculated retrospectively, either in aggregate form (e.g. total children born alive and total deaths) or individually. In the latter case, the most common approach is a birth history. All these methods are referred to as 'retrospective' since they rely on the memory of mothers to provide information on past events.

Studies have indicated that there are systematic biases associated with the retrospective data collection approach as well as problems of omission or the misdating of some events. There are now a number of studies which have been conducted around the world by the Demographic and Health Surveys[11] as well as by WHO[12] which demonstrate the fragility of the data on births and deaths even when great care has been taken with the data collection and coding. Some of this experience has been summarized by David and Hill.[13]

The widely applied Brass method, and its subsequent adaptations, measures general trends in childhood mortality over a 20-year period preceding the survey (see Hill[14,15] for a full critique of the method and other alternatives). The estimates are based on reports on total children ever born alive and numbers still living or dead; dates of birth and death are not obtained at interview but instead are estimated from demographic models.[16,17] The method has two key weaknesses. First, it does not produce estimates for exact time periods since it relies on models and moving averages to spread the births and deaths of children to women of different ages over a period of years in the pre-survey period. Secondly, the data from women 15–19 and even those 20–24 used to estimate child mortality close to the date of the survey are often affected by selection effects. These young women are not typical of all women giving birth in a calendar year. For the assessment of general trends before the survey, the Brass-type estimates are nevertheless very valuable; for identifying changes in a particular year, especially years close to the interview, the method is of little help.

Data collection in field conditions can have a dramatic effect on the quality of results. It is clear that the interaction between respondent and interviewer plays a key role in the quality and consistency of the data generated. Whilst we may experiment with variations in the data collection methods, full and truncated birth histories, inversion of the order of questions and so on, investigators in the field recognize that quite different responses will be obtained by a skilled and agreeable interviewer compared with someone who is surly and rude. Reinterview surveys have shown that individual responses can fluctuate in a surprising way. Because such reinterview surveys are rarely conducted, the surveys in Liberia reported in this volume are a notable exception, the net effect of individual changes in responses on the aggregate results is poorly defined. Ultimately, the quality of field work is often assessed by the plausibility of the results. This rather circular

form of evaluation raises the spectre of a serious but unintentional publication bias. It may happen that only surveys conforming to expected patterns are published and disseminated. Fortunately, publication bias is not a serious issue in the MUHS surveys which are fully reported in this supplement, except for an effort in Togo stopped early in the process.

One awkard feature of mortality change is that interseasonal and interannual variations in death rates can be substantial. When the data are of good quality, the monthly and annual rates show an unsettling variability which bears little relation to the smooth curves generated for the same population by the standard indirect techniques of demographic estimation. This fact, together with the very unpredictable outcome of even the best designed and controlled field work, lies at the heart of the problems faced by programme managers in estimating the mortality impact of their interventions. Annual fluctuations in child mortality will probably mean that an effect must be observed for several years before it is detected. Most deaths of children in developing countries are still due to infectious diseases which come in epidemics. Epidemics of diseases such as malaria, measles, whooping cough, meningitis, typhoid, poliomyelitis are rather chaotic, although some regularities can be found in the season at which they occur and sometimes in their annual periodicity, such as major outbreaks every 3 or 4 years. For instance, a measles vaccination may not show any impact without a measles epidemic.

Attributing mortality change to health interventions. The challenge is to show that mortality ended up being lower as a result of the intervention than it would have been if no intervention had taken place. Estimating what might have happened if no intervention occurred is the approach adopted. Figure 1 illustrates the basic problem: q_c is the mortality in the absence of intervention and q_i is mortality with the intervention. For the same area, one can never know both q_c and q_i. One approach is to use the quasi-experimental design initially proposed for the MUHS surveys. q_c is measured in control areas matched as closely as possible for social, economic and demographic factors. q_i is measured in the intervention areas. While one can never be sure that the control and intervention areas are matched for all possible confounding factors, the quasi-experimental design is one of the only methods that can allow us to attribute changes in mortality to a specific health intervention. A variety of methods have been proposed but all are confounded by well established long-term trends in mortality.

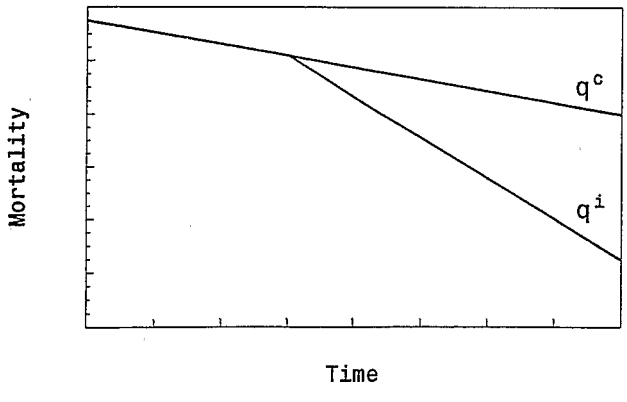


FIGURE 1 *Hypothetical comparison of mortality before and after intervention*

Experience of the last 15 years has shown that mortality has been declining at a substantial pace in almost all developing countries studied. Table 1 summarizes selected demographic and health surveys in sub-Saharan Africa. Declines of the magnitude documented in this volume for Liberia are not uncommon. The demographic and health survey for Liberia shows that 5q0 decreased for the entire country at a rate of nearly 10% over the same period as the CCCD project was implemented. And it shows a slightly higher rate of mortality decline before these interventons were widely used in Liberia. When there is a strong secular trend in mortality, ascribing mortality change to a specific intervention is more difficult.

Some epidemiologists argue that the impact of the field application of a new technology should be measured in a randomized control trial. Part of this type of evaluation, however, is to test if field application of a technology produces the anticipated outcomes. Success depends on factors beyond the individual level. Local institutions, management, and community level factors can influence the impact of the programme. These factors cannot be tested in a traditional randomized controlled trial design; newer methods which randomize communities or villages can be used to control for community level factors.[18] Such design methods, however, require extraordinarily large sample sizes since each community or village is a single data point.

Because of the limitations of the retrospective survey methods of measuring child mortality, measuring both q_i and q_c with enough power to detect significant changes is difficult.

One proposed way to disentangle direct programme effects from other indirect outcomes is to document changing patterns of cause of death. There are technical problems associated with linking changes in deaths due to a single cause as with general mortality rates. Despite these problems, data on changing causes

TABLE 1 *Change in child mortality (5q0) in selected African countries with demographic and health surveys*

Country	Time period	5q0	% change
Botswana	1973–1977	89	
	1978–1982	60	−32.6%
	1983–1988	53	−11.7%
Burundi	1972–1976	224	
	1977–1981	234	4.5%
	1982–1987	152	−35.0%
Ghana	1973–1977	187	
	1978–1982	152	−18.7%
	1983–1987	155	2.0%
Kenya	1974–1978	106	
	1979–1983	93	−12.3%
	1984–1989	89	−4.3%
Liberia	1971–1975	275	
	1976–1980	243	−11.6%
	1981–1986	220	−9.5%
Mali	1972–1976	360	
	1977–1981	311	−13.6%
	1982–1987	249	−19.9%
Senegal	1971–1975	287	
	1976–1980	236	−17.8%
	1981–1986	191	−19.1%
Togo	1973–1977	206	
	1978–1982	159	−22.8%
	1983–1987	158	−0.6%
Uganda	1973–1977	180	
	1978–1982	200	11.1%
	1983–1988	180	−10.0%
Zambia	1977–1981	151.9	
	1982–1986	162.2	6.8%
	1987–1991	190.7	17.6%
Zimbabwe	1973–1977	92	
	1978–1982	104	13.1%
	1983–1988	75	−27.6%

of death may still be a useful measure of impact. There is now a considerable literature on cause of death assignment using verbal autopsies (as opposed to hospital autopsies) for use in surveys or small scale studies.[19,20]

It can be argued that data on causes of death may also increase the level of significance of the difference between mortality in the populations without the intervention and with the intervention. To illustrate this point, let us assume that measles accounts for 15% or total mortality in a population of children where the total mortality rate is 150 per 1000. If an absolute change of 11 per 1000 in overall mortality were to occur, due to a 50% relative change in measles mortality, would imply a 7.5% relative change in overall mortality which is not detectable with 1000 children. While the change in total mortality is not statistically significant, the change in measles mortality is statistically significant at the 5% level.

There are two major limitations of focusing on changes in specific causes of death; one is the potential interaction between causes of death. Garenne *et al.*[10] have shown that a health intervention such as measles immunization can have quite unexpected effects such as increasing mortality from diarrhoea, despite the expectation from other studies that measles immunization would decrease diarrhoea deaths. Several trials of reducing cardiovascular disease mortality through reductions of blood cholesterol levels have had the unexpected effect of reducing cardiovascular disease mortality but not changing all-cause mortality. The reductions in cardiovascular deaths have been matched by increases in injury mortality.[21] In the example cited above a statistically significant decline in measles mortality could be hypothetically accompanied by an increase in mortality from all other causes which would not be detected with the smaller sample size. Cause-specific mortality can be useful but it can also be misleading if all-cause mortality has not also undergone a statistically significant change in the same direction.

The other problem is the unreliability of the identification of the cause of death when the cause in question comprises a small proportion of all deaths. Even assuming very high levels of specificity and sensitivity for the cause of death attribution (higher than found in most trials[22]) when the cause of death in question amounts to less than about a quarter of all deaths, the confidence limits of the estimates are very wide indeed and probably too wide for even quite large studies.

In summary, the ideal design for evaluating an intervention would include an estimate of the long-term trends occurring previously in the same population, a documentation of the changes in mortality during and immediately after the intervention over a period of say 5 years, a control group, and perhaps data on causes of death that were targeted by the intervention. All these conditions are rarely met, and conclusions are usually based on partial information.

THE RESULTS OF THE CCCD SURVEYS IN LIBERIA AND ZAIRE

Kingandu, Zaire

The results of the Zaire and Liberia surveys are summarized in Table 2. In Zaire, the programme raised

TABLE 2 *Summary of MUHS surveys in Liberia and Zaire*

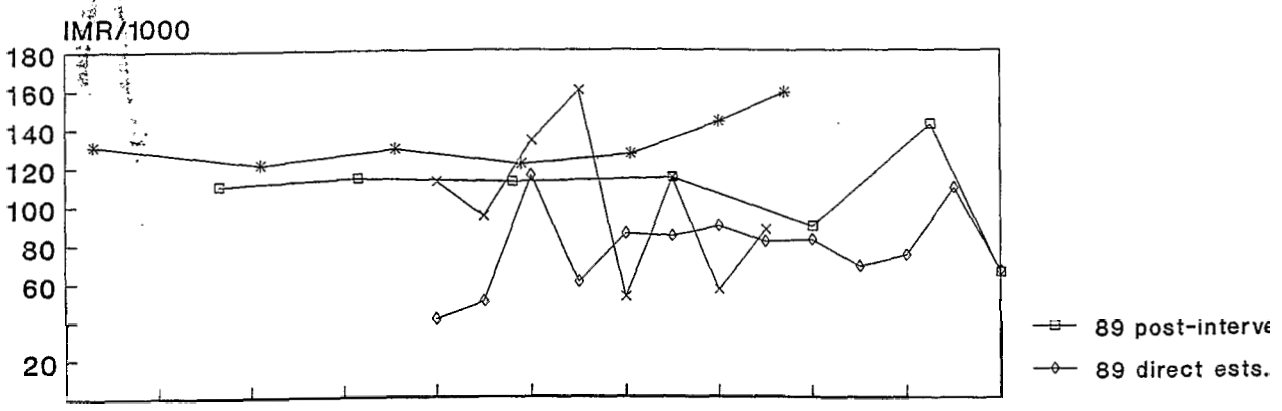
	Liberia	Zaire
Baseline survey		
Control areas	No	Yes
Method	Truncated pregnancy histories	Complete pregnancy histories
5q0 in baseline survey	316	111–117
5q0 in reinterview survey	359	191
Programme indicators (% covered before, after, absolute change)		
Measles immunization	13,33,20	22,74,52
Mothers giving ORS/SSS for diarrhoeal episodes	6,4,–2	?,53,?[a]
Children with fevers treat with antimalarials	71,72,1	47,44,–3
Repeat survey		
Control areas	No	Dropped
Method	Truncated pregnancy histories	Complete pregnancy Histories
Results compatible with baseline survey	Yes	No
5q0	302	147 (1985–1989)
Statistically significant decline in 5q0	No but Yes with adjusted rates	No

[a] The baseline survey in Zaire did not measure the use of SSS in children with diarrhoea so that the change cannot be reported. However, only 36% of mothers knew of SSS so that use must have been between 0 and 36%.

measles coverage by nearly 50%, and coverage with other antigens also increased. While the number of mothers who reported knowledge of 'sugar-salt' solution for children with diarrhoea also increased, many fewer mothers could adequately describe the proper ingredients or recipe for preparing sugar-salt solution. Unfortunately, the percentage of mothers giving sugar-salt solution to children with diarrhoea was not measured in the baseline survey. Coverage in the third component of the programme—antimalarial drugs for children with fever—did not improve.

The focus of the rest of this discussion is whether the surveys illustrate mortality impact attributable to these changes in health service provision, most notably the increase in measles immunization. The mortality data from the reinterview survey of July 1985 (replacing the seriously flawed baseline data of October 1984) and the post-intervention survey of August 1989 are shown in Figure 2. Both the indirect Brass-type estimates and the period rates calculated directly from life tables constructed from birth histories are shown on this graph. The retrospective data from the paper by Chahnazarian *et al.*[23] have been used in preference to the data published in the paper by Taylor *et al.*[24] The graph shows infant mortality trends but the form of the graphs based on the indirect estimation methods is broadly the same for under-5 mortality because the estimates are derived using model life tables in which the relationship between under-1 and under-5 mortality is almost constant.

The data provoke several comments: first, the indirect estimates, based on the proportions dead of children ever born alive, produce higher estimates of mortality than the directly calculated rates. Second, the trend in infant mortality seems very flat during the whole pre-intervention period. Thirdly, the direct estimates from 1989 and from 1985 are not consistent with each other. The data from 1989 appear to be of better quality, presumably because interviewers had become more skilled at obtaining the month and year for births and deaths. Finally, the direct and the indirect estimates give very irregular results for the key period between the baseline survey (the date when the intervention began) and the post-intervention survey of 1989. Part of the explanation for this is undoubtedly the small numbers of women and of births. Altogether,



Brass method, West model life tables

FIGURE 2 *Direct and indirect infant mortality estimates from the 1985 and 1989 surveys*

the data no not provide a strong basis for the examination of short-term trends in infant or child mortality.

The quality of the data is obviously an important factor. Taylor *et al.*[24] in this supplement have already referred to the problem of missing dates in the birth histories and have pointed out the tendency for more missing dates amongst the dead children. The imputation procedure, which estimates missing dates based on circumstantial evidence or on random numbers, reduced the overall level of underestimation of mortality in the direct estimates but there is a strong possibility that some of the imputed dates resulted in misallocation of some births and deaths by period and age at death. The small size of the sample introduces additional random errors, making ascertainment of trends over short periods very difficult.

Chahnazarian *et al.*[23] ultimately use only the results of the 1989 survey to assess the change in child mortality with the introduction of the programme. At a significance level of $P < 0.05$, only changes in the mortality rate for 1-year-olds to 5-year-olds was significant. Other indicators including the mortality rate from 6 months to 3 years, the infant mortality rate, and the mortality rate from birth to age 5 did not decline significantly. The fact that some indicators of child mortality declined significantly while most did not illustrates the tenuous nature of the data.

Discussion of the possible role of the CCCD programme in causing the declines in 1–5 year old mortality is problematic. Without a control area, trends in mortality cannot be ascribed to the intervention with any confidence. Chahnazarian *et al*[23] attempt to use the estimates of ${}_4M_1$ and ${}_{30m}M_{6m}$ in a regression on time and a dummy variable for the presence of the programme. The results for ${}_4M_1$ were not significant; but, they report a significant programme effect for ${}_{30m}M_{6m}$. This result is sensitive to the precise specification of the time series model used; the results cease to be statistically significant if the pre-intervention period is restricted to 1989–1984, if the results are adjusted for serial autocorrelation or if heteroskedasticity is treated differently.

Any further discussion of the possible programme impact in Zaire is thus likely to continue to be inconclusive. This is not to say that the increasing coverage of measles immunization achieved through the programme had no effect on mortality; indeed, we would expect that there has been some real benefit. Rather, the demographic data, closely examined, simply do not support any claims that the project altered mortality trends in any dramatic way during the brief period under discussion. The impact of the programme may well be apparent over a longer period of study.

Liberia

In Liberia, the programme impact on process indicators was minimal at best. Measured measles coverage increased by only 20% while no improvement in diarrhoea or fever treatment was detected. Analysis of the demographic changes accompanying the programme must be made in light of the relatively modest changes in measles coverage and few other improvements.

The design of the surveys in Liberia followed the same broad pattern as in Zaire—an unsatisfactory baseline survey in November 1984 followed by a resurvey in May 1985. The post-intervention survey took place in May 1988. The difference was that a second reinterview survey was conducted after the post-intervention survey in order to check the accuracy and completeness of the 1988 data. Considerable care went

into ensuring that the same clusters were used for the interview and the reinterview surveys in both 1984–1985 and in 1988. Brass-type estimates were used to check the general level of childhood mortality in the 1985 survey only; broadly, the series of 1(x) values from the direct and indirect estimation methods seem reasonably close although the data presented in Table 2 of the paper by Becker *et al.*[25] do not allow us to make simple comparisons since the time location of the two 1(x) series differs and the basic data for the re-estimation are not published in the paper. Figure 3 in the same paper showing confidence intervals around the direct estimates of infant mortality is a useful reminder of the importance of sample size in these studies. In the smaller reinterview surveys, the variances of the estimates are even larger.

The one to one comparison of events in the pregnancy histories described by Becker *et al.* provide some rare insights into the nature of the data errors. In this case, the omission of dead children was five times higher than for the living, a tendency widely cited but rarely documented. Given that few surveys conduct such careful evaluations of the reliability of mothers' responses, the wide fluctuation in mothers' responses to similar questions puts a tremendous premium on careful field work. More pessimistically, the results call into question many birth history surveys conducted in other parts of Africa and the developing world.

With careful correction and adjustment, it seems that a decline in under 5 mortality from 1984 and 1988 can be documented. But there are signs that a decline may have already been under way before 1980 (see Figure 3, Becker *et al*[25]). Even when mortality for this period is estimated for the 1988 survey, it is impossible to discern a sharp improvement in mortality coincident with the intervention period. In the absence of a control area, attributing the mortality decline to the mild increase in immunization coverage achieved through the programme is virtually impossible.

CONCLUSIONS

The MUHS surveys provide an opportunity to re-examine the major questions surrounding the evaluation of health programmes. Do we need to evaluate the field application of technologies proven in controlled trials? Despite the difficulties of the task as illustrated by the work in this supplement, we must continue to attempt to assess the impact of new programmes. While all would agree on the need for routine evaluation of all health programmes, the need for rigorous scientific evaluation of the impact of programmes is less widely accepted. We feel there remains a need to evaluate rigorously the impact of a new technology or strategy in field conditions. Too many social, economic, managerial, political and cultural factors can alter the effectiveness of proven technologies in real field conditions. It would be reckless to simply assume that measles immunization or any other intervention has the effect predicted from controlled clinical trials in all settings. To pursue the example of measles immunization, for most communities we do not really know the share of mortality attributable to measles either directly or indirectly. Nor do we know the effects on other causes of death of decreasing measles mortality through immunization. Given the magnitude of the investments, the health impact of many major programmes such as the Expanded Program of Immunization and of Primary Health Care have been infrequently demonstrated. We need objective evaluation of the effectiveness of the field application of proven technologies and the cost-effectiveness of these technologies. Unfortunately, this supplement does not provide information on the costs of the CCCD programme so that cost-effectiveness cannot be estimated.

Accepting the need for scientific evidence of impact, what indicators of health outcome should be used? Health interventions should improve health; measures of impact may include mortality, morbidity and disability. In the MUHS surveys, only mortality was evaluated. Given the technical problems of demonstrating mortality impact over a brief period, more attention may need to be paid to measuring objective changes in morbidity and disability. Recent trends in the cost-effectiveness literature have focused on measures of health outcome that incorporate both mortality, morbidity and disability.[26] These studies use a family of indicators that measure years of healthy life lost.

The available methods to measure mortality change are severely limited. Prospective continuous recording of births and deaths is probably the most versatile tool for measuring short-term mortality changes. Such population laboratory methods are costly and not widely applicable. Retrospective methods can only be used to prove impact convincingly when the programme has been operating over a long period—5 or more years—and the effect is large.

The determinants of child mortality are complex. Socioeconomic factors such as the level of maternal education have been repeatedly shown to be major determinants. Murray and Chen[27] argue that the secular trend in mortality levels is more likely to be related to the accumulation of stocks of health promotive knowledge and infrastructure than to short-term actions. If long-term changes such as the ac-

cumulation of health promotive knowledge and behaviours through expanding education, contact with health services, and other forces are critical to health change, then attributing short-term changes in mortality to programme impact is extremely difficult. The only objective way to attribute mortality change to programmes is with a quasi-experimental design with control areas. Even this method is subject to a host of limitations as control and intervention areas can never be fully matched for all confounders. The MUHS surveys ultimately did not include control areas. Even if the mortality decline had been more impressive than the changes demonstrated, attributing these to the programme without a control area would be impossible. Objective evaluation calls for rigorous methods in all aspects of the analysis.

As a body of work, the MUHS survey programme is substantial and significant. It is one of the rare examples of an attempt to measure rigorously the impact of a child health programme. Unfortunately, CCCD programme produced only a moderate increase in immunization coverage and little change in the use of sugar-salt solution for diarrhoea and no change in the use of antimalarial drugs. Not surprisingly, this modest change in health service delivery had little convincing impact on child mortality. The MUHS surveys do teach us how difficult it is to prove scientifically the impact of health technologies applied in real field conditions. This supplement serves as a warning to programme managers seeking convincing evidence of the effectiveness of their work. The papers, we hope, will encourage others to adapt old techniques or to develop new ones appropriate for the special needs for health impact assessment. There is always a risk that poor assessment procedures will lead policy makers to make erroneous choices in the future. A failure to demonstrate impact convincingly is not the same as convincing evidence of no impact.

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VOLUME 22. SUPPLEMENT 1. 1993

ISSN 0300–5771

Official Journal of the
International Epidemiological
Association

Monitoring Child Survival Programmes in Africa: The Africa Child Survival Initiative

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OXFORD UNIVERSITY PRESS