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Measuring the Impact of Water Supply and Sanitation Investments on Diarrhoeal Diseases: Problems of Methodology

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Blum D (Ross Institute of Tropical Hygiene, London School of Hygiene and Tropical Medicine, Keppel Street, London WC1E 7HT, UK) and Feachem R G. Measuring the impact of water supply and sanitation investments on diarrhoeal diseases: problems of methodology. *International Journal of Epidemiology* 1983, **12**: 357–365.
A review of the published literature on the impact of water supply and/or excreta disposal facilities on diarrhoeal diseases, or on infections related to diarrhoea, reveals several methodological problems that hamper the drawing of definitive conclusions from these studies. This paper examines eight of these methodological problems: lack of adequate control, the one to one comparison, confounding variables, health indicator recall, health indicator definition, failure to analyse by age, failure to record usage, and the seasonality of impact variables. It is suggested that an evaluation of the impact on health of environmental interventions may best be undertaken by the combined efforts of engineers, social scientists and epidemiologists in 'opportunistic' settings and that the intervening behavioural processes so necessary for health impact to occur should be a primary focus of such evaluations.

Efforts to demonstrate health effects of improved water supply or excreta disposal facilities, alone or in combination with other environmental modifications, date from the mid-1800s. At present, there are over 50 studies published in the English language on the relationships between water supply and/or excreta disposal, and one or more of the following health indicators:

- incidence rates of diarrhoea and/or dysentery;
- prevalence rates of excretion of one or more bacterial or protozoal enteric pathogens (most commonly *Shigella)*;
- prevalence rates and/or intensities of intestinal helminthic infections;
- nutritional status;
- prevalence rates of eye or skin infections;
- mortality rates.

These studies vary in methodology: some are cross-sectional, others longitudinal; some incorporate specific interventions, others investigate the impacts of existing differences in water supply or sanitation. Although it is believed intuitively that modifying the environment by improving water supply and excreta disposal facilities, alone or as part of an intervention package, will reduce disease transmission, the results of these studies do not necessarily demonstrate this effect. While most of the studies do claim to show an improvement in one or more health indicators, critical review of the papers raises serious doubts as to the validity of their conclusions.

Forty-four published studies on the impact of water supply and/or excreta disposal on diarrhoea, or on infections related to diarrhoea, are tabulated in Table 1; this list is intended to be comprehensive, although a few relevant studies may have been omitted inadvertently. Studies that focus solely on non-diarrhoeagenic agents or on non-diarrhoeal illness have been excluded, as have studies published in languages other than English. Studies focusing only on helminthic infections have also been omitted, except for those few which investigate the relationship between environmental variables and infection with *Trichuris trichiura*, a significant diarrhoea-causing agent. The results of each study are not summarized in Table 1, but rather attention is drawn to their methodological problems. The methodological problems assessed in Table 1 are lack of adequate control, the one to one comparison, confounding variables, health indicator recall, health indicator definition, failure to analyse by age and failure to record facility usage. Some of the judgements made are clear-cut, while others are more subjective. The purpose of Table 1 is not to criticize particular studies but to highlight the high frequency with which certain methodological problems occur in the health impact literature. This focus on methodology, rather than findings, is important because there has been a tendency in the past to summarize the findings of several studies, irrespective of their quality, and derive aggregate estimates of the impact of water supply or sanitation on disease. Such aggregates have little meaning if they include data from studies with serious methodological flaws. This paper is [devo]ted to a discussion of each of eight methodological issues; the seven listed above, which are incorporated in Table 1, plus seasonality, which is discussed separately.

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TABLE 1 *Summary of the literature on the impact of improved water supply/or excreta disposal facilities on diarrhoeal disease or infection. A focus on methodological problems*

Country	Health indicator(s)	Type of study	Environmental variable(s) or intervention(s)	Methodological problem(s)[a]	Source (ref no)
Bangladesh	*Vibrio cholerae* infection	longitudinal	Water supply	3, 7	1
Bangladesh	Diarrhoea and cholera incidence	record review	Water supply	3	2
Bangladesh	Diarrhoea/dysentery prevalence	cross-sectional	Water supply, excreta disposal	3, 4, 5, 6, 7	3
Bangladesh	Incidence of diarrhoea, cholera, shigellosis	longitudinal, record review	Water supply	3, 4	4
Bangladesh	Cholera incidence	outbreak investigation; longitudinal	Water supply, excreta disposal, food ingestion outside the home	3, 7	5[b]
Bangladesh	Diarrhoea/dysentery incidence	intervention, longitudinal	Water supply, excreta disposal, health education	(information not yet available)	6[b]
Bangladesh	*Vibrio cholerae* infection	longitudinal	Water supply, excreta disposal, food, kitchen premises	3	7
Bangladesh	Cholera incidence	record review	Water supply	3, 6	8
Bangladesh	*Vibrio cholerae* infection, cholera incidence	longitudinal	Water supply	3	9
Colombia	Prevalence of diarrhoea, vomiting, colds, head lice	cross-sectional	Water supply, excreta disposal, crowding	3, 4, 7	10
Costa Rica	Diarrhoea incidence; prevalence of bacterial enteric pathogens, intestinal helminths, protozoa	longitudinal	Water supply, excreta disposal, housing, crowding, flies, meat and milk quality	3, 4, 7	11, 12
Egypt	Helminth prevalence and intensity, protozoa prevalence	intervention, longitudinal (intervention village); cross-sectional (control village)	Water supply, excreta disposal, refuse disposal, health services	1, 2, 3	13, 14
Ethiopia	Incidence of gastro-enteritis, respiratory infection, total morbidity	longitudinal	Water supply, excreta disposal	7	15[b]
Guatemala	*Shigella* prevalence	cross-sectional	Water supply, excreta disposal	3, 7	16
Guatemala	Diarrhoea incidence	longitudinal	Water supply, excreta disposal, flies	3, 4, 5, 7	17
Guatemala	Incidence of diarrhoea, skin infections, respiratory disease, eye infections; food wastage	intervention, longitudinal	Water supply, excreta disposal, health education	2, 4	18
India	Diarrhoea incidence	intervention, longitudinal	Excreta disposal, health education	1, 3, 4	19
India	*Entamoeba histolytica* prevalence	cross-sectional	Water supply, excreta disposal, household cleanliness, animals and pets	3, 6, 7	20
India	Gastroenteritis and infectious hepatitis incidence	outbreak investigation	Water supply	3, 5, 6	21[b]
Japan	*Trichuris, Ascaris,* hookworm prevalence/incidence	intervention, longitudinal	Nightsoil treatment, chemotherapy	3, 7	22
Kenya	Morbidity, diarrhoea and gastrointestinal proportional morbidity, helminth and protozoa prevalences	intervention, longitudinal	Water supply, excreta disposal, health education	4, 5	23, 24[b]

TABLE 1 (Cont)

Country	Health indicator(s)	Type of study	Environmental variable(s) or intervention(s)	Methodological problem(s)[a]	Source (ref no)
Kenya	Prevalence of diarrhoea, trachoma, fungal infections, anaemia, parasite ova	cross-sectional	Water supply	3, 4, 5	25[b]
Lesotho	Incidence of diarrhoea, eye diseases, skin diseases	record review	Water supply	3, 5	26
Panama	Prevalence of enteropathogenic *E. coli*, *Shigella*, *Salmonella*	cross-sectional	Water supply, excreta disposal, housing	3, 7	27
Philippines	Cholera incidence	intervention, longitudinal	Water supply, excreta disposal	1, 2, 3, 7	28
St Lucia, West Indies	Diarrhoea prevalence; *Ascaris* incidence/ intensity; *Trichuris* prevalence/intensity; nutritional status	intervention, longitudinal	Water supply, excreta disposal, chemotherapy for *Ascaris*	2, 3	29
Singapore	*Trichuris*, *Ascaris*, hookworm prevalence/ intensity	cross-sectional	Water supply, excreta disposal, housing, refuse disposal, pests and vermin	3	30
United States Arizona	Diarrhoea prevalence, amoebic prevalence	record review	Water supply, excreta disposal	2, 3, 7	31
Arkansas	Amoebic prevalence	cross-sectional	Water supply, excreta disposal, housing	2, 3, 7	32
California	Diarrhoea incidence, *Shigella* and *Salmonella* prevalence	longitudinal and cross-sectional	Water supply	3, 4, 7	33
California	*Shigella* prevalence	longitudinal	Water supply, excreta disposal	7	34
Colorado	Diarrhoea prevalence	cross-sectional	Water supply	3, 4, 7	35
Georgia	*Entamoeba histolytica* prevalence	cross-sectional	Excreta disposal	3, 7	36
Georgia	*Shigella* prevalence	longitudinal	Water supply, excreta disposal, housing and surroundings, flies	7	37
Georgia	Diarrhoea incidence, *Shigella* prevalence	intervention, longitudinal	Excreta disposal, flies	3, 4, 5	38
Kentucky	Diarrhoea incidence; *Shigella*, *Salmonella*, helminth, protozoa prevalence	longitudinal	Water supply, excreta disposal, flies	4, 5, 7	39
North Carolina	*Entamoeba histolytica* prevalence	cross-sectional	Water supply, excreta disposal, refuse disposal	3, 7	40
South Carolina	Helminth and protozoa prevalence	longitudinal	Excreta disposal, housing	1, 3	41
Tennessee	*Trichuris*, *Ascaris*, hookworm prevalence/ intensity	cross-sectional	Excreta disposal	—	42
Tennessee	Protozoa prevalence	cross-sectional	Water supply, excreta disposal, crowding and household cleanliness	3	43
Virginia	*Trichuris*, *Ascaris*, hookworm prevalence	cross-sectional	Excreta disposal	1, 3	44
Venezuela	Diarrhoea incidence	longitudinal	Water supply, excreta disposal, flies	3, 4, 7	45
Zambia	Diarrhoea and *Salmonella typhi* incidence	record review	Water supply	1, 3, 7	46

TABLE I (Cont)

Country	Health indicator(s)	Type of study	Environmental variable(s) or intervention(s)	Methodological problem(s)[a]	Source (ref no)
Seven countries (Bangladesh, Egypt, Iran, Mauritius, Sri Lanka, Sudan, Venezuela)	Diarrhoea incidence: intestinal bacterial, helminth, protozoa prevalence	cross-sectional	Water supply, excreta disposal, flies	3, 4, 5, 7	47

[a] Key to methodological problems (see text for explanation).
[b] Final report (or more detailed report) required to comment more fully on methodological problem(s).
1—Lack of adequate control.
2—One to one comparison.
3—Confounding variables.
4—Health indicator recall.
5—Health indicator definition.
6—Failure to analyse by age.
7—Failure to record facility usage.

METHODOLOGICAL PROBLEMS

Environmental interventions, by their very nature, are problematical to evaluate. Epidemiological evidence that a given treatment (in this case, intervention) has produced a specific effect is best established in the context of a randomized controlled trial where the treatment (or intervention) is randomly allocated to individuals (or areas). However, environmental interventions are not as a rule introduced on a random basis, but rather as a result of political, economic, humanitarian and other considerations.

Although random allocation of environmental interventions may often be impracticable, there are situations in which it can be undertaken; these situations may become more common as impact evaluation is increasingly taken into account in the overall planning of environmental interventions. In such cases, health impact studies could be improved by attention to the methodological problems discussed below. Where allocation is not random but is influenced by confounding factors which can be identified and controlled, the issue of sound epidemiological method is not so clear-cut. In these cases, we believe that impact evaluation may still yield useful results for planners and resource allocators if the methodological problems discussed below are taken into account in the execution of the study, and the factors influencing allocation are recognized and controlled.

Lack of adequate contol (Problem 1, Table 1)
Control observations are essential for meaningful interpretation of data. Two kinds of control problems arise in some of the studies. First, there is the complete absence of an external control sample. Without an adequate control sample, there is no way of distinguishing between health improvements resulting from water supply or excreta disposal improvements and health improvements that would have occurred in any case due to other factors of social, economic and environmental change. Even if no health improvements are detected, no conclusions can be drawn because it might be that health would have deteriorated without the water or sanitation investment or, conversely, that health would have improved and the water supply or excreta disposal facilities increased transmission of certain infections.

The second problem of inadequate control occurs when, although there is a control sample, the comparability of the control and intervention samples for the health indicators under consideration is not established prior to the intervention, either because no baseline data are gathered, or if gathered, show the two samples to be different from the outset. Any observed post-intervention differences in health cannot then be ascribed to the intervention.

The one to one comparison (Problem 2 Table 1)
Even when control communities are selected and monitored for comparison with intervention communities, it is common practice to select a single control community and compare it to a single intervention community. Community in this context usually means village. The provision of water supplies, latrines (especially communal latrines) or health education is typically a community-wide activity. Each individual or household is not an independent unit of study, and the sampling unit is the community or village. A 'one village to one village' comparison is analogous to basing a conclusion on the effect of a treatment solely on the differential response between two individuals, one treated and one untreated. The sample size in each category is one and hence no statistically valid conclusion can be drawn.

Most of the intervention studies in Table 1 are hampered by the one to one comparison problem. The cross-sectional studies, by contrast, typically include measurements on several villages in each environmental category. The high costs of intervention studies encourage the adoption of only one intervention community, but costs must be weighed against the validity of the results. The individual or household is often taken as the unit of study and it is assumed that these units are independently affected by the intervention. The statistical tests in this situation use the inter-individual variation (or equivalently, the within-village variation) to compare mean values for the villages, without taking into account the additional component of inter-village variation. This approach is only valid if the inter-village variation is zero. This fact can only be demonstrated by examining several randomly selected villages in the intervention and control groups, in which case a multi-village study is carried out and there is no need to assume zero inter-village variance.

A way to avoid the one to one comparison problem is to study only one village and identify one or more internal control groups defined, for example, by their utilization of facilities. Inter-village variance is not relevant and the variance between individuals or households within the village is the important factor. Other problems arise with this approach, however. First, there may be confounding differences between the intra-village comparison groups which are reflected in differing disease rates unrelated to the intervention. Baseline studies prior to the intervention are necessary to assess these differences. Second, conclusions reached by studying only one village may not be generalizable to other villages.

Some of the studies in the literature have partially dealt with the one to one comparison problem by comparing one intervention community with multiple control communities. This approach at least allows one to compute the expected variation between the control communities and then compare the intervention village with the mean of the control villages and see if the difference is outside the range of expected variation. However, it is important to know the criteria by which the one intervention village was chosen.

To deal effectively with the one to one comparison problem, it is necessary to increase the sample size. If the sampling unit is the village because one is studying a village-wide intervention, it will be necessary to study several villages in each environmental category, or more practically, a sample of households in several villages.

Confounding variables (Problem 3 Table 1)

Inadequate control of confounding variables is a major problem in all but a few of the studies listed in Table 1. Controlling for the myriad of variables that might conceivably influence the selected health indicators is probably an impossible task except in the context of a randomized intervention with adequate numbers of villages in both groups. Inevitably it is necessary to select those variables which are likely to have an important confounding effect. The selection of confounding variables for measurement will depend on the health indicator selected. For example, in the case of studies on excreta disposal facilities and diarrhoeal disease, major confounding variables include water supply, socio-economic status, and levels of education.

Confounding variables may be dealt with in several different ways. Intervention and control group selection can be based on comparability with respect to confounding variables. Intervention and control groups can be selected on some other basis and confounding variables can be controlled at the data analysis stage by comparing matched sub-samples. These two approaches can be combined. In every case, the essential feature is to define and measure the confounding variables at the start of the study, and to measure them systematically throughout the study. Measurement of confounding variables during a study is important since unanticipated non-environmental interventions may occur which would in themselves influence the selected impact indicators.

Although, strictly speaking, all of the studies in Table 1 fail to control for all potentially confounding variables, some studies do better in this respect than others. The confounding variable problem is not listed for those studies which made an explicit effort to control for important confounding variables.

Health indicator recall (Problem 4, Table 1)

One of the commonly used indicators of impact is diarrhoeal morbidity, particularly in young children. This information is usually obtained by making regular visits to each household and asking an adult present to recall the diarrhoea episodes experienced by members of the household over the past so many days or weeks, or since the last visit by the interviewer. The question may require information on the presence or absence of diarrhoea over the defined period or on the number of discrete episodes of diarrhoea.

There are a number of reasons why the information so obtained may be unreliable or incomplete. First, the diarrhoeal history of family members may not be known to the respondent. Second, there may be an unwillingness to divulge this information even if known. The only obvious motivations for admitting diarrhoea are fear of the interviewer, the promise of treatment, or good rapport with the interviewer. The motivations for denying diarrhoea are embarrassment about a 'dirty' disease or fear that an admission will lead to a request to submit a blood or stool sample. There is evidence that diagnoses that involve social threat or shame are poorly reported.[48]

Third, the ability to remember diarrhoea is limited even when an adult is asked to recall his/her own diarrhoea history or when a mother is asked about her own children. The optimal recall period to adopt in diarrhoea surveys is the shortest that can be handled logistically. The maximum recall period consistent with reasonable accuracy is controversial and is influenced by several factors: who is responding and for whom, the severity of diarrhoea of interest, and the uses for which the data are obtained. Some believe that one week (possibly two) is the maximum recall period for which reliable answers on diarrhoea can be obtained, while others urge a 24-hour, or possibly 48-hour maximum; we take the latter view. Studies are needed to confirm or reject this opinion. It may be argued that, if it is differences between intervention and control groups that are sought, a certain amount of unreliability of recall may be tolerated. However, it is likely that this unreliability of recall occurs non-randomly between the intervention and control groups due to the presence of intervention activities in one group and not the other.

One solution to the health indicator recall problem is to keep the recall period as short as possible, as discussed above; an alternative approach is to avoid recall entirely and instead rely on evidence of infection as an impact indicator. Recent advances in the identification and isolation of important diarrhoeagenic agents and in serodiagnosis make infection an increasingly attractive impact indicator when the necessary laboratory facilities and expertise are available.

The recall periods for diarrhoea used in the studies in Table 1 are tabulated in Table 2. In Table 1, recall periods exceeding 48 hours are considered a methodological problem.

TABLE 2 *Recall periods for diarrhoea used in some studies*

Country	Recall period	Source
Bangladesh	2 weeks	3
Bangladesh	1 week	4
Bangladesh	24 hours	7
Bangladesh	1 week (1st interview); 24 hours (subsequent interviews)	9
Colombia	1 week	10
Costa Rica	1–2 weeks	11
Ethiopia	24 hours	15
Guatemala	2 weeks	17
Guatemala	2 weeks	18
India	1 week	19
Kenya	2 weeks	23
USA: Colorado	12 weeks (maximum recall period)	35
Georgia	4 weeks	38
Kentucky	4 weeks	39
Venezuela	4 weeks	45
Seven countries	4 weeks	47

Health indicator definition (Problem 5, Table 1)
All health indicators must be precisely defined. 'Diarrhoea' in particular lends itself to imprecise interpretation which may vary from respondent to respondent within the same study if particular care is not taken to standardize a definition which is applicable to that cultural setting. A number of studies in which 'diarrhoea' was used as a health indicator fail, at least as reflected in the published reports, to define diarrhoea and appear to be measuring impact on a vaguely defined illness.

In infections where both exposure and immunity contribute to clinical outcome, infection may be a more sensitive measure of the efficacy of an intervention than clinical outcome. However, even when indicators of infection are used, they must be precisely defined. For instance, *Entamoeba histolytica* cysts in stools are often not defined with respect to their differentiation from *Ent. hartmanni* cysts. Coliform counts are reported from water supplies without definition of what is meant by a coliform.

Failure to analyse by age (Problem 6, Table 1)
The diseases and infections considered in environmental impact studies are, without exception, unevenly distributed among various age groups. Most types of diarrhoea, for instance, have their highest incidence in young children. In addition, the impact of an environmental intervention depends on behaviour and on usage of some new facility and both these are also dependent on age. It is therefore necessary to adopt an age-specific approach to data analysis and this has not always been done. In some situations it will be appropriate to restrict the whole study to young children.

Failure to record facility usage (Problem 7, Table 1)
Water supplies or latrines, in themselves, have no impact on health. All health improvements depend on how the new facilities are used and by whom they are used. While this statement seems self-evident, many of the environmental impact studies fail to record facility usage and assume that the presence of a particular water supply or excreta disposal facility is synonymous with usage of that facility. An effort should be made to substantiate any such assumption. Facility usage may be documented by asking people about usage and/or by observing usage. Observational data will be more reliable, especially data on quantity of water used or use of excreta disposal facilities. Questionnaire information on source(s) of water used may prove reliable but should be corroborated by observational data where possible. Few of the studies in Table 1 include observational data on usage, especially systematically obtained observational data, and fewer still use the information in an explanatory fashion.

This lack of behavioural observation in these studies is remarkable. Its consequence is that there are many reports of what health benefit was associated with which intervention, but very few insights into why. If a health improvement is recorded, one cannot with confidence say why if usage was not carefully studied. If no health benefit was recorded, it may have been because the new facility was not used, but this cannot be known unless usage was recorded. In studies in which usage was recorded it was usage by adults, whereas in the case of diarrhoeal diseases it is usage by young children that is more relevant.[49]

Observing usage is critical in assessing the impact of sanitation interventions where questionnaire information is likely to be particularly unreliable. The provision of new latrines does not guarantee their use and there have been many examples of new latrines being totally unused or used only by certain sections of the community. It is essential to record defaecation behaviour in detail and especially to record the defaecation behaviour of that age-group that both suffers most from the diseases or infections being investigated and is the prime source of infection for others. In the case of studies in diarrhoeal disease, it is necessary to record the defaecation behaviour in young children, which is the group least likely to use the new facilities.

Studies in Table 1 that concentrate on the relationship between water supply and health and that fail to indicate that usage was either observed or queried have been given a '7'. Studies that solely or additionally investigate the relationship between excreta disposal facilities and health have been given a '7' if no observational data, or surrogate observational data (such as evidence of faecal pollution on premises) was collected.

Seasonality

Diarrhoeal diseases, and their associated infections, are markedly seasonal in most parts of the world. This is also true, although less well documented, for some parasitic infections, such as ascariasis and hookworm infection. While several studies give insufficient detail for comment, the remaining studies listed in Table 1 do consider, to varying degrees, seasonality in the collection of health variables; hence seasonality is not included as a methodological problem in Table 1. However, almost all the studies could benefit from a more explicit consideration of seasonality. For example, health variables may be measured at the same time of the year in intervention and control communities but there is typically no indication whether this time of the year represents the peak season for the health variables being measured, or even why the season was selected. Few studies analyse their data in a way that would expose possible differences in impact in different seasons. Even rarer is an explicit appreciation that the variables of intervention and usage—such as water quality, water source and latrine usage—may be highly seasonal.

While the most reliable information on seasonal impact indicators and usage is obtained by observations taken continuously through the year, such observations are labour- and cost-intensive and may be impractical. A more practical solution may be to define the extreme points in the year, often the wet season and the dry season, and to undertake synchronized observations at both periods. Whichever way the longitudinal data are collected, they should be analysed by season. If only once yearly observations are possible for logistical or other reasons, then these must at least be taken during the same period each year, preferably during the period of peak incidence of the health indicator under study, and during the same period in the intervention and control communities.

DISCUSSION

Health impact evaluation of improved water supplies and excreta disposal facilities will be increasingly demanded by health planners and economists. While the International Drinking Water Supply and Sanitation Decade (1981–1990) may carry a momentum of its own, unless it can be shown that such improvements confer some benefits, either in time savings or improved health, there may be a backlash against large resource allocation once the Decade is over. Since many people will remain unserved in 1990, such a backlash will have far-reaching repercussions. This paper points out that health impact evaluation of environmental interventions is a difficult undertaking. The methodological problems inherent in studies on water supply and sanitation and diarrhoea are very considerable and there are no easy answers or short-cuts.

Much money and effort have been wasted in the past conducting studies with inconclusive outcomes. It is preferable in general to concentrate on 'opportunistic studies'—studies that seize on experimental opportunity thrown up by an existing water supply or sanitation programme—than to launch elaborate 'set-piece' studies—studies that create a tailor-made situation and launch special interventions for the explicit purpose of studying their impact. Set-piece studies will always be very costly and are in danger of investigating an intervention that is not representative of the interventions that government agencies are actually implementing, or could reasonably be expected to implement.[50] Concentration on 'opportunistic studies' is not inconsistent with the concept of random allocation, which, as discussed earlier, should be attempted whenever and as far as possible. If impact evaluation is anticipated and taken into consideration early in the intervention programme planning process, then it may be possible to introduce whatever intervention programme is already planned into areas on a random basis. For example, an intervention programme may be formulated as a matter of government policy, but the implementers may retain flexibility concerning exactly where and when the programme is introduced. If a programme has been designed for a particular area, and the whole area is to be eventually served, then the intervention may be introduced, if not in a random order, then at least at a geographical starting point which is randomly selected. Then, villages or sections not initially involved (or a random selection of these villages or sections) can serve as controls in the interim, limiting bias between intervention and control areas.

In addition to concentrating on 'opportunistic' settings, it is often preferable to study a limited selection of health and environmental variables in the context of a very specific hypothesis about how a given intervention may affect a given infection. The recent study on the effect of hand washing on secondary spread of shigellosis in Bangladesh[51] is a good example of this approach.

Many processes intervene between an intervention and an impact. The majority of studies have failed to collect any substantial information on intervening processes. Usage of new facilities is an important intervening process and has rarely been systematically recorded. There are two reasons for always including detailed investigations of intervening processes into any environmental health impact study. First, if there is no information on intervening processes it will be possible to state what the health impact was, but never why. Second, data on intervening processes are simple and cheap to collect compared to data on health variables.

There will be many situations in which useful studies can be conducted that focus on intervening processes and omit measurement of health variables because it is not logistically feasible or it is too costly. Alternatively, health variables can be measured once it has been shown that the intervening processes have been affected. Where health impact assessment is considered essential for determining the ultimate worth of an environmental intervention programme, usage studies remain important explanatory tools as well as evaluation indicators in themselves.

Many intervening processes are concerned with behaviour and usage and it follows that the study of these intervening processes requires social scientists. Regrettably, collaboration between engineers (who study interventions) and epidemiologists (who study health impacts), on the one side, and social scientists (who study intervening processes), on the other, has been limited. There are few examples in the literature of studies jointly planned and conducted by epidemiologists, engineers and social scientists. This major failure in inter-disciplinary collaboration must be overcome before there can be a new generation of studies providing fresh insights into the role of environmental interventions in diarrhoeal disease control.

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