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THE IMPACT OF IMPROVED WATER SUPPLIES AND EXCRETA DISPOSAL FACILITIES ON DIARRHEAL MORBIDITY, GROWTH AND MORTALITY AMONG CHILDREN

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ABSTRACT

Many studies have tried to associate water supply and sanitation facilities with health status. The published results are confusing and contradictory because of methodological problems. This review attempted to establish a cause-effect relationship between water and sanitation and diarrheal morbidity, nutritional anthropometry and mortality by systematically assessing reports according to research design and health outcome. It found that excreta disposal facilities were more effective than improved water supplies for lowering diarrheal morbidity and mortality rates and improving child growth. It also found that water supply improvements by themselves may have limited effectiveness in improving health. A conceptual framework has been developed to reduce methodological problems when evaluating present and future water or sanitation programs.

Key words: anthopometry; child mortality; diarrhea; research design; review; sanitation; water supply

INTRODUCTION

Diseases related to poor excreta disposal facilities and inadequate or contaminated water, especially diarrheal diseases, are still a major health problem in the less-developed countries. Diarrheal diseases impose a heavy toll in terms of the number of children affected and the severity of the consequences. From 500 million to 1 billion episodes of diarrhea occur annually among African, Asian, and Latin American children (1). Diarrhea leading to dehydration, electrolyte imbalance, and sometimes hyperpyrexia is the major cause of death in young children: estimates range from 3.9 to 18 million deaths per year (1, 2). Diarrhea may also debilitate children slowly through anorexia, improper feeding, decreased absorption of nutrients, and increased catabolism. Growth faltering or ultimately death may occur if the effects of diarrhea are severe and protracted.

Most of the pathogenic agents causing diarrhea in infants and children are transmitted via the fecal-oral route. That is, fecal pollution of the environment leads to the oral ingestion of pathogenic agents. The fecal-oral route is also the pathway of other non-diarrheal disease agents, such as poliomyelitis, ascaris, trichuris, and strongyloides stercoralis. Inadequate waste disposal is associated with all of these diseases (figure 1).

Water quality and availability are also related to the persistence of diarrheal diseases. Diseases thought to be primarily related to water quality are cholera, typhoid, and giardiasis, because water acts as a vehicle for transmission for the disease agent. They are referred to as water-borne diseases (3, 4), although they can also be transmitted by contaminated food and hands. Some pathogens leading to diarrhea thought to be related more to water availability are Shigella, Entamoeba

histolytica, enterovirus, and rotavirus. These diseases are called water-washed diseases (3, 4); it is believed that the contamination of food, utensils and hands by these disease agents would be prevented by providing sufficient water, even of poor quality.

The United Nations has launched a worldwide effort to break the fecal-oral transmission of these diseases by declaring the 1980s to be the International Drinking Water Supply and Sanitation Decade (5). The intention is to provide everyone with access to safe drinking water and waste-disposal facilities by 1990. This task is formidable, because of the great number of people who need this service and because of the cost of accomplishing this. In 1975 only an estimated 22% of the rural population in less-developed countries, excluding China, had access to safe water, and only 15% had access to adequate waste-disposal (6). Urban areas were somewhat better off: 77% of the people had access to safe water and 75% had access to adequate excreta disposal. Thus, more than 500,000 people a day would need to be provided with improved water and sanitation facilities during the Decade in order to achieve the U.N. goal. The ten-year cost to accomplish this has been estimated at $60 billion for adequate worldwide water supplies and $300-600 billion for sanitation (7). The per-capita cost is $200-400; a cost that exceeds the yearly per-capita income of most of those in need.

Investments of this magnitude could be justified by showing that such improvements in water and sanitation have been cost effective. These calculations are difficult to do with any precision because the necessary denominator, effectiveness, is elusive. So despite the fact that the steps needed to determine cost-effectiveness appear

straightforward, the true benefits of water and sanitation improvements are not known.

This paper reviews studies that have looked at improvements in water and sanitation in terms of health benefits. In the Results section we tabulate the published studies relating water and sanitation to morbidity, growth and mortality. The tables present information about study location, indicators, comparison groups, and results. A more detailed heuristic review of selected studies identifies major design and methodological issues. The major issues identified are used in the Discussion section to develop the algorithm by which water and sanitation studies can be assessed. The use of this algorithm reveals factors which are useful in ascertaining how well the reviewed literature establishes an effect of water and sanitation on health outcomes. This algorithm should be useful in identifying important issues which future study designs should address.

METHODS

Studies examining water and/or sanitation conditions have been included if they quantified differences in diarrheal morbidity, or a specific pathogen, nutritional status, or mortality. Sanitation is used in this review to describe excreta disposal conditions. The review attempts to be exhaustive of all studies since 1950; however, in a few cases, the data were too difficult to obtain or were too meager. Some studies did not specifically analyze water or sanitation conditions, but they provided information on these conditions and health.

In the Results section all of the studies considered are categorized according to their outcomes: diarrheal morbidity (table 1A), nutritional status (table 1B), or mortality (table 1C). The studies reporting each outcome are described according to location, the type of comparison, the

ages under observation, and the results reported, and are evaluated to reveal the major design and methodological factors that could explain the results. The review of available results in table 1 only describes selected studies to exemplify major methodological and design issues. Each study is then systematically judged in the Discussion section according to an algorithm developed from the insights gained from the results. This algorithm is related to each study's design and reported health associations. It identifies studies which best succeed in passing the criteria necessary for establishing a cause-effect relationship between water or sanitation and health.

REVIEW OF PUBLISHED RESULTS

Association of water and sanitation and diarrheal morbidity

Most studies of improvements in water and sanitation have tried to show effects on diarrheal disease or infection rates (table 1A; (8-45)). The results are by no means consistent: in come cases, strong positive results are obtained, but in others, the reverse of the expected benefits appears.

A study of infant diarrhea in two Indian villages in Arizona (38), one with indoor plumbing (treatment village), and one without (control), reported that hospital admissions for diarrheal disease declined after indoor plumbing was installed. There are a number of major confounding issues in this study, but one in particular should be noted. Random assignment of the treatment group did not occur. One village cooperated with the Public Health Service in constructing the indoor plumbing facilities, while the control village resisted any attempts to improve

their condition. Therefore, the reported differences cannot be ascribed to plumbling conditions alone. They may also be related to characteristics of self-selection by the participants, their interest in or attitudes toward health.

All studies reported in table 1A, which have evaluated interventions, were not randomized (18, 20, 26, 27, 30, 31, 32, 34, 35, 38, 39, 41, 45). It is never possible to identify all possible differences between comparison groups which may influence the results. Randomly assigning interventions to villages can transform the uncertainty about these differences into a statistical probability statement.

In a study from Guatemala (21) no differences in diarrhea attack rates were reported for infants from families with and without privies. Children 1-5 years of age from families with privies were reported to have lower attack rates than children from families without privies. Since the design of this study suffers from a major flaw, the results are inconclusive. A protracted epidemic occurred while the study was conducted, which the authors believed may have been measles. Since measles has a high attack among infants, a measles epidemic, unrelated to water or sanitation, may have masked results in the infant age group.

It is not always possible to anticipate non-specific problems such as measles, and it is also not always possible to identify these problems if they occur. Replicating the number of villages in a study can diminish the effect of non-specific influences, such as a measles epidemic. Replication cannot, however, control for systematic bias described above. Replicating the number of villages in a study cannot correct for lack of randomization. Similarly, randomization cannot control for lack of replication.

A study in Haiti (23) examined diarrhea prevalence among children from homes using less than one can of water per person per day and compared this rate to children from homes using more than one can of water per day. No significant differences were reported although the better off group had 11% less diarrhea than the worse off group.

A major problem with this study was the measurement of diarrheal disease, because the disease recall period was over 240 days long. Since underreporting of diarrhea increases as the recall period is lengthened and more underreporting can occur when there is more diarrhea to underreport (46), the differences between the two groups were probably underestimated. Thus, bias in the measure of the outcome indicator, diarrhea, prevented the investigators from finding true differences.

The underreporting of diarrhea in field surveys is only one way that a diarrheal morbidity indicator can mask benefits from improved water supplies. A study from Lesotho illustrates two other ways (31). This study examined reductions in the ratio of clinic reported diarrhea to all non-water related diseases for all ages and among children under four years of age after improvement in village water supplies. Although children under four years of age from improved villages were reported to have less diarrhea than children from unimproved villages, this difference was not statistically significant.

The investigators used this ratio to provide a measure of disease which was independent of differential village attendance at clinics and population size. If children from unimproved villages were farther from a clinic, they may have underreported diarrhea more than those from improved villages (47). However, if the diseases in the denominator of the ratio were not differentially underreported, or to a lesser extent,

the ratio used to compare diarrhea rates between the two villages would underestimate true differences. This possibility was not examined.

Underreporting could have also occurred even if the ratio corrected for village attendance and population size. The ratio may have been equal, but the numerator and denominator may have been unequal. That is, all diseases, including diarrhea, could have been higher among children from unimproved villages. In this case, reductions in diarrhea could have occurred, but use of the ratio precluded finding any differences.

One study from Bangladesh (13) examined cholera and non-cholera diarrheal rates among families that did or did not use public tubewells that were free of coliforms. Families were defined as tubewell users if they used tubewell water five times more often than other water sources (canal or tank). The three tubewells provided water for 88 families, averaging 6.7 persons per family and 3.3 below the age of 15. Diarrheal incidence data for 11 years, obtained from a hospital, were correlated with water use.

Tubewell users were not reported to have lower cholera or non-cholera diarrheal rates. In fact, rates among tubewell users were higher than among non-tubewell users. This was also true when water supply use was stratified by educational level (families with or without high school graduates). Families with high school graduates though had less cholera and non-cholera diarrhea than the less-educated families.

There are several possible reasons for negative findings associated between tubewells and health. Each well had two or three mechanical breakdowns per year, and service from the well was interrupted for about 3 weeks during each breakdown. Thus, the tubewells were inoperative half of the time. In addition, some of the families classified as tubewell users reported using surface water (1800 coliforms/100 ml) for drinking.

It seems clear that tubewell users were still consistently exposed to polluted water for domestic hygiene and drinking. In this case it was not that the tubewells failed to reduce diarrhea, but that people were still exposed to fecally-polluted water.

It should be noted that better education was associated with reduced cholera and non-cholera diarrhea. This suggests that certain extraneous factors could produce positive associations or mask true differences when they exist. This study did not identify any other factors to include in the analysis. Most studies in table 1A did not control for known extraneous factors which could have produced the findings reported.

An example of how some factors can mask true differences is illustrated from a study in Panama (33). The authors examined bacteriological evidence of Shigella, Salmonella, enteropathogenic E. coli, and Edwardsiella tarda among children under 10 years of age and compared the pathogen prevalence rates with types of waste-disposal sites and sources of drinking water. Flush toilets had a higher association with infection than any other type of excreta disposal site (ground, pit privy, and river or sea). Piped water was reported to be worse than streams or wells, but better than rain water.

This unexpected finding was explained by the fact that most of the faucets and flush toilets were located in substandard dwellings or multi-family tenement slums in Panama City. It is likely that the facilities were shared and were not adequately maintained. The authors also cited lack of knowledge about personal hygiene as masking the expected benefits of plumbing. Since population density, feeding patterns, and medical care are likely to differ between rural and urban areas, these factors should be measured and included in the analysis.

A study (20) conducted in two villages in rural lowland Guatemala examined, among other outcomes, the impact of piped water on diarrheal morbidity. Each household in one village initially received a piped water supply with health education and latrines in subsequent years, while the control village received no inputs. Comparison of diarrhea rates before and after the intervention for all ages combined showed no differences.

A reanalysis of these data (49), by different age groups, found statistically different diarrhea rates for all age groups except infants and those over 45 years of age. The differences in these two reports were due to methods of analysis.

A panel was convened to resolve this discrepancy (50). One issue the panel addressed concerned the methods of analysis. The reanalysis used a Chi-square technique with episodes of diarrhea as the unit of analysis. Chi-square testing requires independent observations, but episodes of diarrhea were not independent since some individuals suffered repeated bouts of diarrhea. Thus, the Chi-square statistic was artificially inflated and the probability of finding a significant difference by chance was not 5%, as was expected, but was higher. This issue points to the need to use the correct test statistic and to correctly calculate the chance of finding significant differences when none exist.

This study suffered other methodological problems which were also addressed by the panel. The problems identified were similar to those discussed above, failure to randomize and adequately replicate the villages studied so that factors such as family size and income confounded the results, and inadequate standardization of measurement.

Hollister et al. (43) compared differences in Shigella prevalence rates among individuals and families in migrant-labor camps in Fresno

County, California, which had various types of water and sanitation facilities. Families with one positive child under 11 years of age who had Shigella were classified as positive. Some subcamps were equipped with inside water faucets; other facilities were available for communal use. Other subcamps had no indoor plumbing. These subcamps could exist in one large mirgrant-labor camp and were scattered within each camp, not clustered together. Shigella prevalence was lower among individuals and families in the subcamps with indoor faucets when compared to subcamps with no indoor plumbing. When the authors matched these two types of subcamps for a number of factors, the difference in Shigella prevalence between the two was reported to be greater than in the unmatched analysis.

Age and migration of parents could have confounded these results. Subcamps with no indoor facilities had many more children under 11 years of age than did subcamps with indoor faucets, 61% vs. 7% respectively. If the children in the subcamp without plumbing were younger than the children in the subcamp with plumbing, they would be more likely to be classified as positive for Shigella since children under five have higher positivity rates than children over five (22). Age could have confounded the results from the family-level analysis in another way even if Shigella positivity rates were constant over the first 10 years of life. Families with many children under 10 would be more likely to be classified as positive than families with few children under 10, since one positive child constituted a positive family. A number of studies failed to control for age at all (8-9, 11-13, 20, 24, 26, 29, 32, 36, 45). Two studies did not observe children under 5 years of age (16, 37); in one study it was unclear what age was observed (27), and eight studies combined data on children below and above 5 years of age (15, 22, 28, 33, 41-44).

Positivity rates among families may also have been affected by migration. During the 6-month period of investigation, the number of migrant workers declined from 175,000 to 25,000. If younger, less well-settled parents with more children move more often than older, well-settled parents with fewer young children, this could affect positivity rates. The relationship between Shigella prevalence and the parents' length of settlement was not investigated. It is possible that migration or family age distribution rather than water and sanitation were responsible for this finding.

A study in India (27) examined the effect of bore-hole latrines on diarrhea prevalence among children. It was reported that diarrhea was lowered by the introduction of the latrines, although no statistical tests were done. Seasonal effects on diarrhea could have produced this reduction with or without the latrine improvements. Preintervention diarrhea rates were examined only during the time of year when the incidence of diarrhea was rising and at the peak (48). Postintervention observations included the time of year when diarrheal incidence was falling and at its lowest. Thus, the different seasons alone could have accounted for this reduction.

These nine studies illustrate the most important design and methodological problems in the studies of diarrheal morbidity listed in table 1A: lack of randomization and inadequate replication of the unit of intervention, errors in the measure of diarrhea, inability to verify usage of improved services, failure to identify known extraneous factors, failure to include those factors in a multivariate analysis, and inappropriate statistical analysis. Failure to account for these problems could

have accounted for the reported differences in some studies or masked true differences not found in other studies.

Association of water and sanitation to nutritional status

The use of nutritional status, as measured by anthropometry, has a biological and statistical base for inclusion in studies evaluating different water and sanitation conditions. There is good evidence that diarrhea contributes to growth faltering (51-53) and that analyses of nutritional anthropometric indicators may respond as readily as diarrheal indicators (54). Four studies (35, 55-57) examined the association between water and sanitation and nutritional anthropometry, and are reported in table 1B. One of these (35) also examined diarrheal morbidity. Problems identified above also apply to these studies but some of these problems will be addressed in the context of nutritional anthropometric studies.

A longitudinal study in St. Lucia (35), in which about 75 babies in each of three valleys were followed for up to two years, reported an association between water and sanitation conditions and mean growth increments for height and weight. The control valley had one standpipe for every 350 people. The water valley had an individual water supply system installed for each family. The water and excreta valley had an individual water supply system and water seal latrines installed for each family. No real differences could be detected between the control group and the water and excreta group when mean growth increments in kilograms were examined during the first two years of life. In the water group babies 3-6 months of age put on more weight when compared to control babies. Thereafter, no differences were seen. In fact, the water group appeared to be better off than the water and excreta group, although not

statistically. The control group was reported to be significantly shorter and lighter than the other two groups from 5-16 months of age.

Failure to replicate the number of valleys within a treatment in this study affects this analysis. The variance term used in the analysis was the within-child variance, not the between-valley variance which should have been used. The between-valley variance has two components: variance due to children within a valley and variance due to valleys within a treatment. Since no estimate of the between-valley variance can be calculated when only one replicate per treatment group is measured, the within-child variance term must be used. Since the within-child variance is lower than the between-valley variance, the test statistic is inflated. Consequently, the probability of finding a significant difference is increased, when in fact one does not exist.

There is another issue in this study which suggests that the results are inconclusive. If water or sanitation does affect growth, mediated by diarrheal morbidity, then this study should have measured differences in diarrheal either concurrent with or preceding growth differences. In two valleys, the control and water group, growth differences were detected only during 3-6 months of age. In these same two valleys no differences in diarrhea prevalence were detected during the first year of life (see table 1A). During the second year of life when diarrhea rates in the two valleys were different, no growth differences occurred. In fact, the mean differences in height and weight became smaller, that is, catch-up growth seemed to occur in the control group which was suffering more diarrhea. In this example the association between water and health is spurious because of the lack of collateral evidence.

A study in Colombia (55) reported the effect that a number of factors, including water and sanitation, had on growth parameters of poor children under 5 years of age. Families connected to sewage systems had taller and heavier children than did families with only latrines, and families with a shower or bath had fewer malnourished children than did families without these facilities. A number of factors were also associated with height and weight of children: dwelling size; persons per bed; education of parents; and general cleanliness of the dwelling.

When a multivariate regression analysis was performed, including other socioeconomic factors, the sanitary scale, which was undefined, was still reported to significantly affect children's height, but not weight. The multivariate analysis did not separate out the individual effect due to water supply or excreta disposal facilities. The number of malnourished children associated with poor water and poor excreta disposal was very similar. Consequently, no statement may be made regarding the important component, water or excreta disposal. Since water and sanitation interventions may have different design requirements, costs, and effects on health, it is important to identify the responsible component of the intervention. This same argument holds for differences in water quality and quantity.

A recent analysis from Fiji (56) examined how differences in height and weight among preschool children were affected by excreta disposal facilities. In rural areas the presence of a flush toilet in the home was reported to significantly improve both height and weight of children. No differences could be detected in urban areas where the children were not malnourished and could be considered to come from a well-off group. In this case no statistical differences in the urban sample does not mean

that the addition of flush toilets would not improve growth of malnourished children. Growth parameters could not respond to any improvement in excreta disposal facilities because the children were already well-off. It is important to measure differences in populations which can respond to environmental interventions.

A 1978 study in Nigeria (57) examined weight-for-age (W/A), height-for-age (H/A), and weight-for-height (W/H) differences between two groups of preschool children who used water from protected or unprotected wells. Protected water was obtained from dug wells 20-40 feet deep with at least 18 inches of surrounding parapet. This water was reported to be clean, plentiful, and easily accessible. Unprotected water came from wells without parapets, and usually shallow, or from ponds or pools dug in river beds, located in all cases, at least one-half mile from the households.

The authors reported differences in measures of wasting (W/H and W/A) between the two groups, but no differences in stunting (H/A)--although stunting was prevalent in both groups. In this example the H/A figures were inaccurately reported. The breakdown of the H/A categories were (a) greater than 95% of standard reference values, (b) 90-94% of standard, (c) 89-95% of standard, and (d) less than 85% of standard. This overlapping of categories makes the interpretation of results difficult, because children were misclassified according to their stature.

Factors identified in the section on diarrheal mobidity also apply to these four studies: lack of randomization; inadequate replication; failure to identify, measure and control extraneous factors; inappropriate statistical analysis; and failure to verify usage of services. Other important issues have been identified: support of findings with collateral evidence; measuring populations which can respond to the

intervention; and calculating and using the correct statistical error term in the analysis.

Association of water and sanitation to mortality

Ten studies have examined the association of water and sanitation with infant and childhood mortality (58-67) Although mortality is a more distal response indicator than morbidity and anthropometry to water and sanitation conditions (figure 1), reductions in mortality should follow improvements in water and sanitation since diarrhea and malnutrition predispose a child to death.

Many of the problems reviewed in studies analyzing diarrheal morbidity and nutritional anthropometry also apply to mortality studies: lack of randomization; inadequate replication; measurement errors; analysis issues; and classification problems. Some of these issues will be discussed in the context of selected mortality studies.

A study in Kenya (63) attempted to determine the district- and household-level determinants of mortality. The 1969 population census data were used for district analyses to predict the probability of a child's surviving to age two. The source of data for the household analysis was the 1974 ILO/University of Nairobi Household Survey. A benefit from sanitation was reported at the household level.

In the district-level analysis the percentage of adult literacy (PAL) was used as a proxy for drinking water, toilet facilities, personal hygiene, and nutrition. It was reported that PAL had a statistically positive effect on children's life expectancy--except when the number of hospital beds per 1,000 persons was included in the regression analysis, which suggests that medical care may be more important than water supplies or toilet facilities. An ecological study of this nature uses the

geographic unit (e.g., districts) to measure exposure (e.g., toilets) and disease (e.g., mortality). Since an ecological study contains no information on the exposure of individuals nor the exposure of individuals who died, attributing geographic characteristics to individuals cannot be done. In the context of this study no statement may be made about the causal association of PAL to mortality. Since risk factors tend to cluster into geographic units, there is also an increased risk of confounding. Another ecological mortality study has this same problem (66).

In the household-level analysis, data on type of water supply and sanitation facilities were available. Only pit latrines were reported to be associated with reduced mortality: at the 10% level for the rural sample, but at the 5% level when the rural and urban samples were combined. This suggests that sanitation may be more important in urban areas. The presence of a toilet was reported to have a greater magnitude of benefit than a pit latrine did, but this was not significant at the 5% level. Piped or well water did not appear to have a beneficial effect on mortality rates.

In this analysis household characteristics were determined only for 1974, while births could have occurred as early as 1960. A retrospective study of this nature is subject to misclassification bias according to exposure, presence or absence of toilets. Because a temporal sequence between toilets and mortality events cannot be ruled out, a spurious association between toilets and mortality would have occurred. In other words, the association between sanitation and mortality may be merely coincidental and not causal. Three other studies suffered this problem as well (58, 66, 67).

An Egyptian study (61) examined neonatal and infant mortality rates in a control village that had no improvements of any kind, and in four other areas that had one or more of four improvements: wells and latrines, fly control measures, preventive medical care, and installation of refuse disposal. The author reported that only fly control, not water and sanitation improvements, seemed to reduce mortality rates.

This study suffered from a number of major flaws already described: failure to randomize, no replication of villages within a treatment, and failure to verify exclusive use of improved facilities. Another possible explanation for these negative findings was the overall poor condition of the villages. A sanitation score, developed after the interventions, was used to indicate the degree of improvements. A high score reflected large improvements and a low score reflected poor conditions. Sanitation scores indicated that, despite improvements, poor conditions continued after the interventions. In this case any reductions expected in mortality rates may have been offset by the deplorable conditions in general. This is tantamount to saying that no intervention occurred since any improvement that was made could not have sufficiently reduced pathogen ingestion to reduce mortality.

A descriptive study in Sri Lanka (66) reported the association between regional variation in water and sanitation conditions and infant mortality rates (IMR). The region in which only 2% of the households had tap water had an infant mortality rate that was similar to a region in which 30% of the households had tap water. The situation was even worse for latrines. Numara Eliya, the region with the highest percent of households possessing latrines, 42%, had the highest IMR of all regions in Sri Lanka. Conversely, the region in which only 3% of households had latrines had an IMR 60% lower than the Numara Eliya rate. Conversely,

regions which relied heavily on well water had lower IMR's than regions which did not rely on well water.

Despite a number of flaws in this report, one particular problem should be mentioned. The IMR had been declining in Sri Lanka for a number of years before these data were collected . In this context differences in IMRs reflect the general improvements in health during the preceding years and the differences cannot be ascribed to water or sanitation conditions at a particular point in time.

A retrospective study from Sri Lanka (67), in which the estate sector was analyzed separately from the rest of the country, analyzed the statistical interaction between types of sanitation, mother's literacy and father's education at different periods in an infant's life. The presence of toilets was reported to be significantly associated with lower mortality. When the presence of toilet facilities was examined along with data on mother's literacy and husband's education, a synergistic interaction was found: that is, toilets plus higher parental education were associated with significantly lower childhood mortality than would result from adding the reduction in mortality as an effect of toilets and good education.

A problem in interpreting results of this study, if the impact is real, is the degree of mortality underreporting. The study pooled mortality rates from 1950-1975, but used toilet conditions and educational levels only from 1975. If mortality underreporting in the worse-off group was greater than the better-off group, this would suggest that the actual differences in mortality were even larger than the reported differences. In this case the magnitude of improvements to be expected from the installation of excreta disposal facilities would be underestimated;

hence, its cost-effectiveness relative to other interventions would be underestimated.

Another retrospective study, this one from Malaysia (64-65), analyzed the effect of toilets and piped water on infant mortality. The effect of breastfeeding was also considered. The presence of toilets was associated with lower infant mortality, particularly in the postneonatal period. Piped water had a beneficial effect for the 2-6 month age group but not for other times during infancy.

When statistical interaction of toilets and breastfeeding was analyzed the presence of a toilet was reported to be even more beneficial for children who were not breastfed; it did not have an added benefit for children who were breastfed. The statistical interaction of piped water and breastfeeding also revealed that piped water lowered infant mortality if the babies were partially breastfed, but the effect of toilets was stronger than the effect of piped water. It should be noted that mothers who breastfed tended to have poor sanitation facilities, and those who did not breastfeed generally had good facilities.

A number of extraneous factors which were controlled in the analysis included: income; birthweight; parity; mother's age; preceding interpregnancy interval; year of birth; and ethnicity. The analysis, however, was done at the child level, not the community level, and regional controls were not included in the analysis. Because the analysis, which was done at the child level, did not take into account the large clusters from which the data were collected, the chance of finding a statistically significant difference when one did not exist, was not 0.05 as reported, but was higher. Because intracluster correlations were not taken into account, the variance used when testing differences was artificially lowered.

Study design and methodological issues identified in the morbidity, growth, and mortality present studies could lead to spurious associations reported in table 1. These issues will now be systematically structured to judge every study in table 1 according to study design and the reported statistical associations.

DISCUSSION

Overall, the studies examining the association between improved water and sanitary facilities and diarrheal disease have reported conflicting and inconsistent results. Of the studies on nutritional anthropometry, height and weight appeared to be affected more by water than sanitation conditions. The studies on childhood mortality suggest that waste disposal facilities may be more important than water facilities, at least in lowering mortality. Regardless of the type of association reported--whether a positive one, negative one, or none at all--each study must satisfy certain criteria for internal validity before any implications can be drawn.

Systematic criteria applied to research findings expose biases when they occur and help distinguish between causal and indirect associations, thus permitting assessment of the plausibility of the findings. A previous algorithm (68) has been extended and used to show whether or not the findings associated with different water and sanitation conditions satisfy these criteria for validity.

The studies are grouped according to the statistical associations reported and the study design to determine which criteria need to be satisfied, table 2. Two groups of studies reporting positive results, experimental trials and observational surveys can yield data associating

water and sanitation conditions to health (figure 2). An experimental trial is defined as one in which random assignment of the intervention took place. If random assignment of interventions does not occur, the study must be considered observational. An observational survey may either examine an intervention which was not randomized or examine pre-existing differences in water or sanitation conditions.

Experimental trials provide more plausible results than observational studies. Because experimental trials make probability statements of causality of association, and observational surveys make probability statements of plausibility of association, two sets of criteria have been developed. Table 3A lists the criteria necessary for judging the experimental trial. All studies which reported positive associations and evaluated interventions are included in table 3A. The criteria for and inclusion of observational surveys, table 3B, will be discussed below. Assigning the intervention in a strictly random fashion decreases the possibility that associations are due to some confounding variable inherent in the treatment or control group when the intervention is applied. Furthermore, results from a randomized trial can be used to make inferences about the causality of association, because this type of study permits assigning a probability statement to the possibility that the association described is not due to confounding--even when some possible confounding factors are not measured or even recognized. This issue is checked in table 3A, part B1a. A randomized trial can use cross-sectional data or include before-after measurements; in the latter case it is called a before-after randomized trial.

Although randomized trials can assign a probability to the likelihood that an intervention caused a result, randomization alone cannot

identify which component associated with the intervention was responsible. In order to identify the responsible component additional techniques must be used when designing experimental trials. First, randomization should be done across enough independent units so that statistical probability tests can be applied. In trials involving water and sanitation measures, the unit of intervention is not the child even though the objective of the intervention is to improve children's health. The appropriate unit of intervention that must be sufficiently replicated is the village. Since children within a village are subject to any influences which affect an entire village, they cannot be considered independent observations. For instance, if a measles epidemic affected an entire village, it would mask or exaggerate the true effect of any intervention if the village was the only one included in a study. This issue is checked in table 3A, part B1b.

Second, if statistical testing is done using the incorrect unit of replication, then probability statements will invariably be wrong. Child level variances are smaller than village level variances and if child level variances are used in the analyses, then statistical probability statements of association due to chance alone will be higher than those reported. Since the statement regarding the probability of causality of association is high, the plausibility of the findings is diminished. This issue is checked in part B2 of table 3A.

A third important issue concerns the application of the intervention to the recipients. Water or sanitation interventions may be accompanied by additional inputs such as health education. In this case the intervention assumed, water or sanitation, was not the intervention implemented; the intervention was water or sanitation and health education.

Thus, the true benefit of water or sanitation cannot be estimated. This issue is checked in part B3 of table 3A.

Fourth, the study should be conducted in a manner that prevents systematic bias from occurring when measuring the outcome. For example, measurement of diarrhea might be biased if the enumerators know which villages received the intervention and which did not. This can be avoided by blinding the assessment of the outcome, in which the enumerators do not know which villages have received the intervention or ignore this knowledge. This issue is checked in part B4a of table 3A.

A randomized trial should also control for behavioral differences associated with the intervention. If those villages receiving a water supply act differently because they know they received the intervention and are being observed, this could bias the study's results. In this case, a reduction in diarrheal rates would be caused by a change in behavior that would not be duplicated unless the intervention were applied in an experimental context. This can be controlled by applying a placebo intervention to the nonintervention groups, so that all groups are aware that they are receiving an intervention but are blinded as to which one. This issue is checked in part B4a of table 3A.

Even though random assignment of an intervention occurs, there may be participant self-selection after randomization. If enumerators measure participants, who choose to be measured, then a biased estimate of the health outcome would be obtained. This would diminish the plausibility of the findings. This issue is checked in part B4b of table 3A.

Studies in which the unit of intervention is randomly assigned, sufficiently replicated, and blinded by assessment and by assignment, are known as double-blind randomized trials. This is the only type of study

that permits assigning a probability statement that the association found is a causal one. However, it is unlikely that a double-blind randomized trial can be conducted for water and sanitation interventions, with the possible exception of water quality studies.

Experimental trials should measure known and potential confounding factors. This should be done to verify that randomization was successful in distributing confounding factors equally across comparison groups (see part B5 of table 3A). Even if some potential confounding factors are not measured or even recognized, this type of study permits assigning a probability statement to the possibility that the association described is not due to confounding.

Experimental trials can increase the probability that an association is causal if confounding variables are identified and taken into account either in the design of the study (e.g., matching) or in the analysis of the results. Confounding factors that are likely to affect interpretation in studies of water and sanitation conditions can be identified on the basis of the biological pathways that lead from the intervention to the outcome (see figure 1). The most relevant confounding factors that might affect the outcomes can then be measured (figure 3). If all steps change concurrently as predicted by biological theory, the plausibility that the changes are due to the intervention is increased. Analyses that control confounding, analytic studies, are more valuable than those presenting only descriptive reports of associations.

These criteria can now be applied to studies evaluating water and sanitation interventions that reported a statistically significant association to see how well they met the criteria of an experimental trial. Table 3A indicates that even though several studies were intended as

intervention trials, none were conducted as randomized trials and, therefore, none can be considered truly experimental. Since these studies did not use true random assignment, they must be evaluated according to the criteria for observational surveys in table 2B as well.

Observational surveys can only make probability statements of association, but different types of surveys may be used, Figure 2. The cross-sectional observational surveys use survey data to examine the concurrent presence or absence of good water or sanitation facilities in relation to an outcome, such as diarrhea. Before-after surveys look for a reduction in diarrhea following a water or sanitation intervention. Both types of surveys could find associations that are due to factors other than improved water and sanitation. For instance, a before-after survey could detect a decline in diarrheal rates due to a general regional improvement in the standard of living and, consequently, in health and hygiene. In order to avoid this type of secular confounding, data from areas without the improvements can be included in before/after comparisons with data from the improved areas. This type of survey is differentiated from the before-after survey by the presence of the comparison group, and this design, in turn, increases the plausibility of the findings. However, it is impossible to identify and measure every possible confounding factor that could affect the intervention and comparison groups, so there is always a possibility in observational studies that the reported effects are not due to the intervention but to some other confounding factor. The factors most likely to confound results are listed and checked in part B1 of table 3B.

Two techniques can be used to control for confounding factors. By design, comparison groups can be matched on certain identified factors or these factors can be controlled by multivariate statistical techniques.

Analytical studies, in which confounding is controlled by the above techniques, yield more plausible results than mere descriptive reports. These issues are checked in parts B2 and B3 of table 3B, respectively.

As in experimental studies, measurement errors must be minimized. Blinding by assessment remains an issue, but blinding by assignment is not an issue in observational surveys. If blind assessment is not carried out, potential biases can be estimated and controlled. Measurements can be standardized by stratifying or randomizing the enumerators across comparison groups.

Improved services may not always be used. There are many possible reasons that services are not used and these include breakdowns, lack of knowledge on how to use the services, easy access to other sources of water (e.g., streams or ponds), and sanitation problems (e.g., odors and darkness of improved pit latrines). If positive associations were reported, but usage of services was not measured, then the plausibility that the association found was due to improved water or sanitation services is diminished. This issue is checked in part C1 of table 3B. The plausibility of association can be increased if more than one health outcome is measured (see Figure 1). If different health outcomes change concurrently as predicted by biological theory, then the plausibility is increased. This issue is checked in part C2 of table 3B.

Table 3B examines observational studies that reported a statistical association. There were a few studies that reported a change in outcome but did not calculate statistical associations. If the change in the outcomes of such studies compared was greater than 50%, the study is included in table 3B. Studies reporting a change of less than 50%, and

present no statistical significance figures, are grouped with those reporting no association and are discussed later. Some studies reported both positive and negative findings, either for different indicators or for different ages. These studies are also included in table 3B.

According to table 3B, there are a few studies that do satisfy some of the criteria necessary to increase the plausibility of the reported associations. Most of these are mortality studies (60, 63, 64, 65, 67), and their results suggest that improving facilities for excreta disposal is more important than water supply. This is in accordance with what is known about fecal-oral transmission of pathogens (figure 4). If excreta is effectively removed from the environment, fewer pathogens are available for ingestion. Once pathogens have entered the environment, they may take many pathways to ingestion, and are thus more difficult to control.

When the level of exposure to fecal contamination from the environment is high, the effect of drinking water quality or water accessibility may be different when compared to low levels of exposure. For example, water quantity improvements would likely have an impact when exposure is high. When exposure is low, the magnitude of the impact may be greater (19) or smaller (40, 64) when compared to levels of high exposure. This magnitude may depend on the preintervention exposure levels (69). The case of water quality improvements may be different. If exposure is high, water quality improvements may have little or no impact. Conversely, water quality improvements may have a measurable impact when exposure is low. No study addressed this issue, but it suggests an analysis for statistical interaction. The importance of water quality may be questioned in light of the number of sources of infection, figure 4. Water quality by itself may do little to reduce disease incidence

unless it was accompanied by other effects to reduce the ingestion of pathogens from sources not affected by the quality of the drinking water.

The evidence in favor of excreta disposal facilities is advanced when results from two studies examining nutritional status are considered (55, 56). In these studies children's body size was associated with excreta disposal facilities even when other factors were controlled in the analyses. These two outcomes--anthropometry and mortality--offer collateral evidence that excreta disposal facilities can affect health.

One study in diarrheal morbidity (16) reported that hygienic bathrooms were associated with less diarrhea among grade schoolers when compared to unhygienic bathrooms. In another morbidity study (42) diarrhea was reduced when water was more readily accessible, but was the same when well or piped city water was used.

The next question to be considered is whether the studies reporting a negative association between sanitation and health counterbalance these inferences. Because no negative study was randomized, all of these studies are considered observational surveys. Some issues described above for positive studies also pertain to negative studies; they include documentation of usage of services, measurement biases, and control of identified confounding factors by either matching or statistical analysis. Studies that fail to show an association must also be evaluated in light of three additional criteria that apply only to negative findings.

First, was the outcome likely to be changed by improvements in water and sanitation? That is, were conditions such than an outcome could respond to the intervention? For example, in an area where most children are breastfed, the addition of good water is not likely to reduce infant

mortality rates. Breastfeeding reduces exposure to pathogens and confers immunity when pathogens are ingested.

The second criterion deals with sample size. Were there enough replications of the unit of analysis so that the power of the test was sufficiently large to find differences in the groups compared?

The third criterion is control for possible negative confounding. A negative confounder that is related to both the intervention and the outcome would wipe out any true benefit from the intervention. For example, improved water supplies may draw many people to a particular area, creating a crowding problem. Overcrowding, which enhances the spread of pathogens directly from one person to another, will mask the true effects of improved water. Negative confounding may prevent an outcome from occurring, whereas positive confounding may allow an event to occur independently of the intervention.

Examination of studies reporting negative associations (table 3C) reveals that all suffer from one or more of these flaws. Only two studies attempted to control for confounding factors in the analysis (13, 14); both were in Bangladesh and examined the health impact of the tubewells. In both of these studies tubewell users used other polluted sources of water because of tubewell breakdown. The confirmed high exposure to other pathogens from other sources would suggest that inadequate tubewell use was insufficient to improve health.

In many of the other studies reporting negative associations the investigators failed to measure confounding. In this case it is difficult to know if negative confounding affected study results.

Only three studies included more than two villages in their comparison (11, 31, 33). In two of these (31, 33), the analysis was done at the

individual level and both were confounded by other factors. One study (31) used an inappropriate measure of diarrhea and the other study (33) was confounded by urban/rural differences. The study which reported data at the village level failed to control for age differences. No ages were reported in this study.

In summary, most studies reporting negative associations are flawed by either not measuring age (11, 13, 24, 32, 45) or reporting on too large an age range (28, 33), or by an inability to specify which age was included in the analysis (27). The remaining studies are flawed by failure of the intervention to occur (18, 61), regional differences in the comparison groups (17, 59, 66), or failure to control for identified confounding factors (15, 20, 23). As a result, there are no reported negative associations suggesting that water or sanitation will not improve health. These negative results have much less plausibility than reports of positive associations.

Upon analysis of all parts of table 3, it becomes apparent that the true effect of water or sanitation improvements remains difficult to estimate. There are a few studies (16, 19, 40, 42, 55, 56, 60, 63, 64, 65, 67) which report results that (a) support the hypothesis that excreta disposal is more important than water interventions, (b) support the hypothesized pathway between water and sanitation improvements and better health, and (c) do not have severe flaws (16, 42, 55, 56, 60, 63, 64, 65, 67).

Estimates of the magnitude of improved health are still difficult to calculate, but this should be less difficult in the future if methodological issues are considered in the design and analysis of health impact evaluation. The biolgoical theory and epidemiological techniques

are advanced enough to provide better estimates of the cost-effectiveness of water or sanitation programs. Failure to do so could result in a reallocation of available funds away from water and sanitation projects and toward other projects. If estimates of the cost-effectiveness of water and sanitation projects are not made, this is a very real possibility. In short, many of the water and sanitation programs being planned and implemented as a result of the Decade, should be evaluated so that estimates of the cost-effectiveness of water and sanitation improvements can be made relative to other interventions.

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Table 1A. The effect of water supply and excreta disposal conditions on intestinal infections, infestations and diarrhea.

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[a] Country code: BGD = Bangladesh; BRA = Brazil; COL = Colombia; CR = Costa Rica; EGY = Egypt; ETH = Ethiopia; FIJ = Fiji; GUA = Guatemala; HAI = Haiti; IND = India; IRN = Iran; KEN = Kenya; LES = Lesotho; LIB = Libya; MAL = Malaysia, NIG = Nigeria; PAN = Panama; PHI = Philippines; SRL = Sri Lanka; STL = St. Lucia; SUD = Sudan; UAR = United Arab Republic; UK = United Kingdom; USA = United States; VEN = Venezuela; ZAM = Zambia.

[b] W = water supply; E = excreta disposal.

Year Country[a] (Ref.)	Type of Indicator	Type of Comparison[b]	Age in Months	Before Intervention or Control	After Intervention or Treatment	Difference	Percent Reduction	Statistical Significance Reported (Calculated)
1982 BGD (8)	cholera - percent positive	W - contaminated vs. uncontaminated	All ages					
		drinking		48.9	22.5	23.4	48%	$p < .05$
		cooking		51.8	13.9	37.9	73%	$p < .001$
		bathing		50.9	14.3	36.6	72%	$p < .001$
		washing		50.9	14.3	36.6	72%	$p < .001$
1980 BGD (9)	cholera - percent positive	W - non-tubewell vs. tubewell users	All ages	11	12	-1.0	worse	(NS)
		- contaminated vs. uncontaminated		11.3	1.2	10.1	89%	$p < .001$
1977 BGD (10)	diarrhea/ dysentery - percent positive	W - ground vs. tubewell	0-12	4.3	3.4	0.9	21%	-
			12-120	4.7	5.8	-1.1	worse	-
		- surface vs. tubewell	0-12	9.1	3.4	5.7	63%	-
			12-120	11.0	5.8	5.2	47%	-

1977 BGD (11)	cholera - % pos.	W - other vs. tubewell	All ages	1.0	4.3	-3.3	worse	p < .001
	Shigella - % pos.			1.5	0.9	0.6	40%	NS
	diarrhea - per 1,000			117.7	133.5	-15.8	worse	-
1977 BGD (12)	cholera - attack rate per thousand	W+E - non-tubewell and surface latrine vs. tubewell and sanitary latrine	All ages	4.1	1.6	2.5	61%	p < .01
1976 BGD (13)	cholera - incidence rates	W - non-tubewell vs. tubewell	All ages	8.4	14.2	-5.8	worse	p = .08
	diarrhea - per 1,000			3.2	7.5	-4.3	worse	p = .07
1972 BGD (14)	cholera - percent positive	W - tubewell far vs. near	12-48	39.1	12.5	26.6	68%	(NS)
			48-60	20.0	0.0	20.0	100%	p < .001
	- classical		0-12	17.6	27.3	-9.7	worse	(NS)
	- el tor		12-48	40.0	43.8	-3.8	worse	(NS)
1978 COL (15)	diarrhea - prevalence	W - unpiped vs. piped	0-72	42.4	45.3	-2.9	worse	NS

1978 COL (16)	diarrhea - prevalence	W+E - hygiene scale	school children	-	-	-	44%	p < .0001
1965 CR (17)	diarrhea - morbidity rate per 1,000	W - worst vs. best district	0-48	1515	1450	65	7%	-
		E - worst vs. best district		905	1456	-552	worse	-
1953 EGY (18)	E. histolytica Giardia lamblia - percent positive	W - control + E (wells and latrine)	12-48	16	16	0	0%	NS
		- treatment		32	51	-19	worse	-
1978 ETH (19)	gastroenteritis - days sick per year	W - community vs. private	0-24					
		< 10 lcd		25	30	-5	worse	-
		> 10 lcd		27	9	18	67%	-
		E - public vs. private						
		< 10 lcd		28	31	-3	worse	-
		10-20 lcd		37	13	24	65%	-
		> 20 lcd		30	8	22	73%	-

1978 GUA (20)	diarrhea - prevalance per 1,000	W - unpiped vs. piped	All ages	39.7	39.5	0.2	1%	NS
1963 GUA (21)	diarrhea - incidence per 100 persons per year	W - public vs. private faucet	0-60	38.9	32.4	6.5	17%	NS
		E - without vs.	0-12	52.8	80.7	-27.9	worse	NS
		with privy	All ages	22.4	16.8	5.6	25%	$p < .01$
1957 GUA (22)	Shigella - percent positive	W - community vs. private	0-120	9.4	6.3	3.1	33%	$p < .05$
		E - areas with <50% vs. >50% of houses with privies and flush toilets		11.2	4.8	6.4	57%	$p < .05$
1980 HAI (23)	diarrhea - percent positive	W - <1 vs. >1 can per person per day	0-60	28.7	25.5	3.2	11%	NS

1978 IND (24)	gastroenteritis - percent	W - area with 27% vs. 48% house connections	All ages	4	0	4	100%	(NS)
1977 IND (25)	diarrhea - incidence per 100 per year	W - well vs. taps in homes	0-60	21.5	23.5	-2	worse	NS
		- street taps vs. taps in homes		36.8	23.5	13.3	36%	p < .01
	Shigella - incidence per 100 per year	W - well vs. taps in homes		10.3	6.0	4.3	42%	p < .001
		- street taps vs. taps in homes		15.3	6.0	9.3	61%	p < .001
1971 IND (26)	gastroenteritis	W - pre-post chlorination	All ages	11.0	1.1	9.9	90%	p < .01
		- control		10.6	17.7	-7.1	worse	NS
	dysentery	W - pre-post chlorination		7.3	1.0	6.3	86%	p < .01
		- control		7.4	10.5	-3.1	worse	NS

1970 IND (27)	diarrhea - 3 month moving averages	E - none vs. bore-hole latrine	Children	9.4	7.4	2.0	21%	-
1966 IRN (28)	diarrhea - percent positive	W - unpiped vs. piped	0-84	48.7	36	12.7	26%	$p < .001$
	Shigella - percent positive	- unpiped vs. piped		7.5	4.5	3.0	40%	NS
1972 KEN (29)	diarrhea - percent positive	W - surface vs. piped	All ages	19	3	15	79%	$p < .1$
undated KEN (30)	diarrhea -	W - far + unpure vs. near + pure	0-12	23.3	18.8	4.5	19%	-
			12-36	16.6	6.3	10.3	62%	-
			36-72	7.0	7.3	-0.3	worse	-
1976 LES (31)	diarrhea - cases of diarrhea ÷ all non-water cases	W - poor vs. good quality	0-48	.52	.48	.04	8%	NS
1978 LIB (32)	*E. histolytica* - percent positive	W - poor vs. good quality	All ages	.13	.11	.02	15%	-
	G. lamblia - percent positive			.009	.009	0	0%	-
	Bacillary dysentary - percent positive			.84	.58	.26	31%	-

1971 PAN (33)	Shigella - percent positive	W - stream vs. piped	0-120	1.5	4.3	-2.8	worse	-
		stream vs. well		1.5	2.0	-0.5	worse	-
		E - pit privy vs. flush		2.9	5.0	-2.1	worse	-
		ground vs. pit privy		1.0	2.9	-1.9	worse	-
1978 PHI (34)	cholera - rates per 1,000	W - poor vs. improved	0-48	542.2	213.7	328.5	61%	-
		E - poor vs. improved		542.2	321.1	221.1	41%	-
		W+E - poor vs. improved		542.2	193.1	349.1	64%	-
1981 STL (35)	diarrhea - percent positive	W - public vs. household	7	35.4	35.9(23.7)	-.4(11.5)	worse(32%)	-(-)
			10	30.9	33.4(17.7)	-2.5(13.2)	worse(43%)	-(-)
		(W+E - public vs. household)	13	19.8	16.1(7.2)	3.7(12.6)	19%(64%)	-(-)
			16	22.1	13.0(11.1)	9.1(11.0)	41%(50%)	-(-)
			19	19.1	10.1(3.8)	9.0(15.3)	47%(80%)	-(-)
			22	22.9	8.8(3.3)	14.1(19.6)	62%(86%)	-(-)
			25	5.7	5.4(5.2)	.3(0.5)	5%(9%)	-(-)
1978 SUD (36)	diarrhea - avg. days ill/person/6 months	W - <26 lcd vs. >40 lcd	All ages	15.6	7.5	8.1	52%	$p < .001$

1966 SUD (28)	diarrhea - % pos.	W - unpiped vs. piped	0-84	26.6	21.8	4.8	18%	(NS)
	Shigella - % pos.	- unpiped vs. piped		14	6.4	7.6	54%	$p < .01$
1966 UAR (28)	diarrhea - % pos.	W - unpiped vs. piped	0-72	38	10	28	74%	$p < .0001$
	Shigella - % pos.	- unpiped vs. piped		9.4	3.4	6	64%	$p < .01$
1978 UK (37)	diarrhea - % pos.	W - pre (supply cut 17 hr) vs. post (no restriction)	School children	5.6	3.3	2.3	41%	$p < .001$
		- pre (supply cut 12 hr) vs. post (no restriction)		4.9	3.2	1.7	35%	$p < .001$
		- pre (no restriction) vs. post (no restriction		3.9	3.5	0.4	10%	$p < .005$
1969 USA (38)	diarrhea - avg. clinic visit per child	W - indoor plumbing	0-12	2.0	.85	1.15	58%	$p < .05$
		- control		3.1	2.60	0.50	16%	NS

1960 USA (39)	diarrhea - attack rate per 100	W - low vs. high quality	0-24	21.6	9.4	12.2	56%	-
1959 USA (40)	Shigella - % pos.	W - off vs. on premises	0-60	6.0	5.8	0.2	3%	(NS)
		- out vs. in premises		5.9	2.4	3.5	59%	($p < .05$)
		E - out vs. in premises		2.4	1.1	1.3	54%	($p < .05$)
1957 USA (41)	diarrhea - incidence rate per 1,000	E - latrine	0-120	17.3	10.4	6.9	40%	(NS)
		- control		20.3	20.5	-0.2	worse	(NS)
	Shigella - percent positive	- latrine		6.0	3.9	2.1	35%	(NS)
		- control		2.8	7.3	-4.5	worse	(NS)
1955 USA (42)	Shigella - percent positive	W - far vs. near	0-120	5.8	4.1	1.7	29%	$p < .025$
		well vs. city		5.2	4.8	0.4	8%	NS

1955 USA (43)	Shigella -% individuals positive	W - out vs. in homes (unmatched)	0-120	5.8	3.0	2.8	48%	($p < .05$)
		- out vs. in homes (matched)		5.9	1.2	4.7	80%	($p < .05$)
	-% families positive	- out vs. in home (unmatched)		11.0	6.2	4.8	44%	($p < .05$)
1953 USA (44)	Shigella - percent positive	W - >15 vs. <15 people per faucet (out of home)	0-120	9.2	5.3	3.9	42%	$p < .0001$
1966 VEN (28)	diarrhea - % pos.	W - unpiped vs. piped	0-84	43	39	4	9.3%	NS
	Shigella - % pos.	- unpiped vs. piped		7.6	3.0	4.6	61%	$p < .001$
1976 ZAM (45)	diarrhea - % clinic cases/1,000	W - unpiped vs. piped	All ages	338	212	126	37%	-

TABLE 1B. The effect of water supply and excreta disposal conditions on nutritional anthropometry.

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[1] W/A = weight/age, H/A = height/age, W/H = weight/height.

[2] R = Best-worst values of the indicator reported in the literature (58).

%R = The higher the value, the better the conditions or change in conditions.

[3] NS = not significant at the 5% level.

[4] H/A was incorrectly reported.

Country Year (Reference)	Type of Indicator	Type of Comparison	Age in Months	Physiological Range[2] ≡ R	Before Intervention or Control		After Intervention or Control	
					Level	%R[1]	Level	%R[1]
Colombia 1975 (55)	W/A - % > 90%	Excreta - latrine vs. sewer system			26		47	
	standard	Water - no bath vs. bath			22		51	
	H/A - % > 95%	Excreta - latrine vs. sewer system	0-32		26		48	
	standard	Water - no bath vs. bath			20		53	
Fiji 1983 (56)	W/A - mean percentile	Excreta - non-flush						
	Urban				102		100	
	Rural				95		102	
	H/A - mean percentile	vs. flush	0-60					
	Urban				100		100	
	Rural				99		103	

Change		Percent Change	Statistics Reported (Calculated)
Level	%R[1]		
21		45%	$p < .01$
29		46%	$p < .0005$
22		57%	$p < .01$
33		62%	$p < .0005$
-2		worse	NS[3]
7		7%	$p < .0001$
0		0%	NS
4		4%	$p < .005$

Nigeria	W/A - % > 75%	Water -		50	69
1978	standard	far vs.			
(57)	H/A[4]- % > 90%	near	6-48	80	69
	standard				
	W/H - % > 80%			63	90
	standard				

16	$p < .05$
worse	NS
27	$p < .01$

St. Lucia	Weight-growth	Water - public	3-6	.7	1.32	31%	1.59	70%
1981	increment	vs. private	6-9	.7	.92	74%	.99	81%
(36)	(kg)		9-12	.5	.67	54%	.69	58%
			12-15	.6	.70	83%	.71	85%
			15-18	.5	.62	84%	.71	102%
			18-21	.3	.66	153%	.58	127%
			21-24	.4	.66	115%	.58	95%
		Water + excreta -	3-6	.7	1.32	31%	1.43	47%
		public vs.	6-9	.7	.92	74%	.99	84%
		private	9-12	.5	.67	54%	.70	60%
			12-15	.6	.70	83%	.63	72%
			15-18	.5	.62	84%	.63	86%
			18-21	.3	.66	153%	.67	157%
			21-24	.4	.66	115%	.62	105%
	Height	Water - public	6		66		66.5	
	obtained	vs. private	12		72.5		73.5	
	(cm)		18		78		78.5	
			24		82.5		83.0	

.27	39%	$p < .01$
.05	7%	NS
.02	4%	NS
.01	2%	NS
.09	18%	NS
-.08	worse	NS
-.08	worse	NS
.11	16%	NS
.07	10%	NS
.03	6%	NS
-.07	worse	NS
.01	2%	NS
.01	3%	NS
-.04	worse	NS
.5		NS
1.0		$p < .001$
.5		NS
.5		NS

TABLE 1C. The effect of water supply and excreta disposal conditions on mortality.

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[a] See Table 1B.

[b] BF = breastfeeding

Country Year (Reference)	Type of Indicator	Type of Comparison	Age in Months	Best-Worst = R [a]	Before Intervention or Control Level	%R
Brazil 1983 (58)	child mortality regression	W - unpiped vs. piped - water x education interaction	children	-	-	
Brazil 1980 (59)	infant mortality correlation with sewage disposal	E	0-12	-	-	
Costa Rica 1982 (60)	child mortality regression coeff. on prob. dying	E - latrine vs. septic tank	0-23	-	-.0821	
Egypt 1952 (61)	infant mortality per 1,000	W - poor vs. wells	0-11	184	175	14%
		W+E - none vs. wells & latrines			175	14%
	neonatal mortality per 1,000		0-1	225	93	64%
					93	64%

After Intervention or Treatment		Change		Statistical Significance Reported
Level	%R	Level	%R	(Calculated)
-		-.290		$p < .0001$
		.102		$p < .05$
-		+.52	worse	$p < .009$
-.2163		-		$p < .05$
206	0%	-31	worse	NS
203	0%	-28	worse	NS
94	64%	-1	worse	NS
85	68%	8	9%	NS

India	death rates - ?	W - pre-post	all ages	
1962	diarrhea	chlorination		1.03
(62)	dysentery			.39
	cholera			.27
	typhoid			.22
Kenya	mortality -	W - well	0-23	
1980	regression coeff.	piped		
(63)	for survival	E - pit latrine		
		flush		
Malaysia	mortality -	W - unpiped vs.	.260-1 2-6	
1984	reduction in	piped	7-12 0-12	
(64)	mortality per	E - nonflush vs.	.267-1 2-6	
	1,000 live	flush	7-12 0-12	
	births			

.59		43%	-
.30		23%	-
.07		74%	-
.08		63%	-
	-.0108	worse	p < .1
	.0056		p > .1
	.0160		p < .05
	.0234		p > .1
	-.62		p < .05
	+.2		NS
	-3.2		NS
	-3.0		NS
	-1.3		NS
	-11.3		p < .025
	-7.2		p < .1
	-28.8		p < .001

Malaysia	mortality -	W - unpiped vs.	
1982	reduction in	piped	
(65)	mortality per	full BF[b]	2-6
	1,000 live	partial BF	
	births	no BF	
		full BF	7-12
		partial BF	
		no BF	
		E - nonflush vs.	
		flush	
		full BF	2-6
		partial BF	
		no BF	
		full BF	
		partial BF	7-12
		no BF	

4.6	worse	NS
-1.8		NS
-7.6		NS
+.6	worse	NS
-4.3		NS
-14.8		$p < .01$
+4.8	worse	NS
-20.3		$p < .005$
-67.4		$p < .001$
-1.3		NS
-10.8		$p < .05$
-18.4		$p < .05$

Sri Lanka	mortality -	W - tap	0-12	184	39	88%
1980	regional per	well			91	58%
(66)	1,000 live births	river			91	58%
		E - latrine			36	89%
Sri Lanka	mortality -	E - nonflush plus				
1980	per 1,000	no educ. vs.				
(67)	live births	flush plus				
		some educ.				
		intervention	0-1		52	
			1-12		24	
			13-60		42	
		no facility vs.	1-12		31	
		buck. or	13-60		31	
		cesspool				
Sudan	mortality -	W - < 40 lcd vs.	0-240		.33	
1978	deaths per	> 40 lcd				
(36)	household					

41	86%	-2	worse	-
26	95%	65	35	-
41	86%	50	27	-
91	59%	-55	worse	-

24	28	(p < .001)
16	8	(p < .001)
25	17	(p < .001)
20	11	(p < .001)
16	15	(p < .001)
12	.21	

TABLE 2. A list of questions asked about study findings depending upon the type of study design and whether or not differences in outcomes were tested and found.

WAS THERE A STATISTICAL ASSOCIATION REPORTED?

YES

EXPERIMENTAL STUDIES

A. Control for confounding
   1. Was randomization properly executed?
   2. Was analysis done with:
      a. proper unit of observation?
      b. proper variance term?
   3. Was study blinded by
      a. assignment?
      b. assessment?
   4. Was randomization confirmed?

B. Analysis for congruity
   1. Was intervention confirmed?
   2. Were other concomitant outcomes measured and analyzed?

OBSERVATIONAL STUDIES

A. Control for confounding
   1. Were major confounding variables measured?
   2. Was there matching by design?
   3. Did statistical analysis deal with confounding?
   4. Were measurements blinded?

B. Analysis for congruity
   1. Were measures of different water/sanitation conditions confirmed?
   2. Were other concomitant outcomes measured and analyzed?

NO

EXPERIMENTAL AND OBSERVATIONAL STUDIES

A. Control for confounding
   1. Was intervetion (experimental studies) necessary?
   2. Did intervention (experimental studies) occur?
   3. Were differences large enough to produce a change in health (observational studies)?
   4. Was randomization (experimental studies) carried out?
   5. Were measurement biases controlled?
   6. Was sample size adequate?
   7. Did statistical analysis deal with confounding?

Table 3A. Summary of ability to satisfy design characteristics for experimental trials showing benefits from water or sanitation improvements

A. Description of study

1. Type of intervention

2. Unit of randomization

B. Control of confounding

1. Randomization

a. Was intervention randomly assigned?

b. Were units of intervention sufficiently replicated?

2. Was analysis done with the variance of the unit of replication

3. Was intervention assumed the intervention done?

4. Was study double blind?

a. If no, were measurement biases estimated or controlled?

b. If no, was participant self-selection after randomization controlled?

5. Was randomization confirmed through comparison of the distribution of confounding variables?

a. Exposure to infection

1) Education

2) Income

3) Rural / urban

4) Seasonality

5) Crowding

6) Secular trend

b. Susceptibility to infection

1) Age

2) Sex

3) Diet

4) Breastfeeding

c. Prevention of outcome

1) Medical care

C. Analysis for congruity

1. Measures of intervention confirmed

   a. Water quality

   b. Water quantity

   c. Utilization of services

2. Measures for consistency

   a. Morbidity

   b. Nutritional status

   c. Mortality

Table 3B. Summary of ability to satisfy design characteristics for observational surveys showing benefits from water or sanitation improvements

A. Description of study

1. Type of comparison

2. Unit of comparison

B. Control of confounding

1. Were major confounding variables measured?

a. Exposure to infection

1) Education

2) Income

3) Rural/urban

4) Seasonality

5) Crowding

6) Secular trends

b. Susceptibility to infection

1) Age

2) Sex

3) Diet

4) Breastfeeding

c. Prevention of outcome

5) Medical care

6) Other

2. Was there matching by design

3. Did statistical analysis deal with confounding?

4. Were measurements blind?

If no, were potential biases estimated or controlled?

C. Analysis for congruity

1. Measure of comparison confirmed

a. Water quality

b. Water quantity

c. Utilization of services

2. Measure for consistency

a. Morbidity

b. Nutritional status

c. Mortality

Table 3C. Summary of ability to satisfy design characteristics for studies reporting negative finding

A. Description of study

1. Type of comparison

2. Unit of comparison

B. Contribution of treatment of intervention

1. Was intervention necessary?

2. Did intervention (treatment) occur by documentation?

a. Water quality

b. Water quantity

c. Utilization of services

C. Control of sample size

1. Was expected difference specified?

2. Was variability within comparison groups described?

3. Was sample size adequate?

D. Control of negative confounding by design

1. Randomization assignment performed?

2. Were measurement biases estimated?

E. Control of negative confounding by statistical analysis

1. Were confounding variables measured?

a. Education

b. Income

c. Rural/urban

d. Seasonality

e. Crowding

f. Secular trend

g. Age

h. Sex

i. Diet

j. Breastfeeding

k. Medical care

l. Other

2. Matching for factors in E1

3. Did statistical analysis deal with confounding factors other than age?

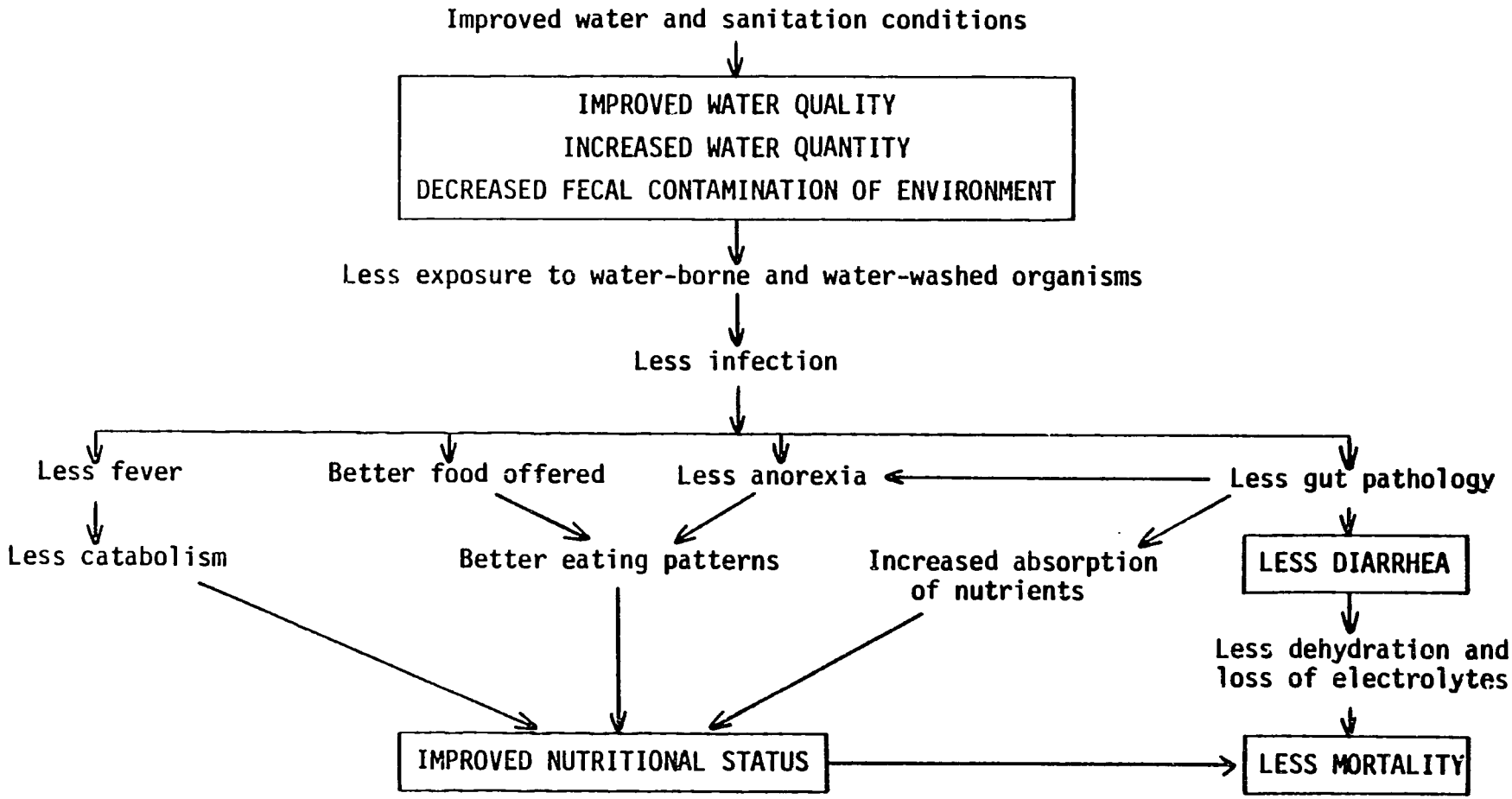


FIGURE 1. Schematic presentation of the sequence of expected benefits from water and sanitation improvements. The most easily measured outcomes are boxed.

FIGURE 2. A schematic diagram of different surveys and trials. Plausability of associations reported increase the further a study is from point A.

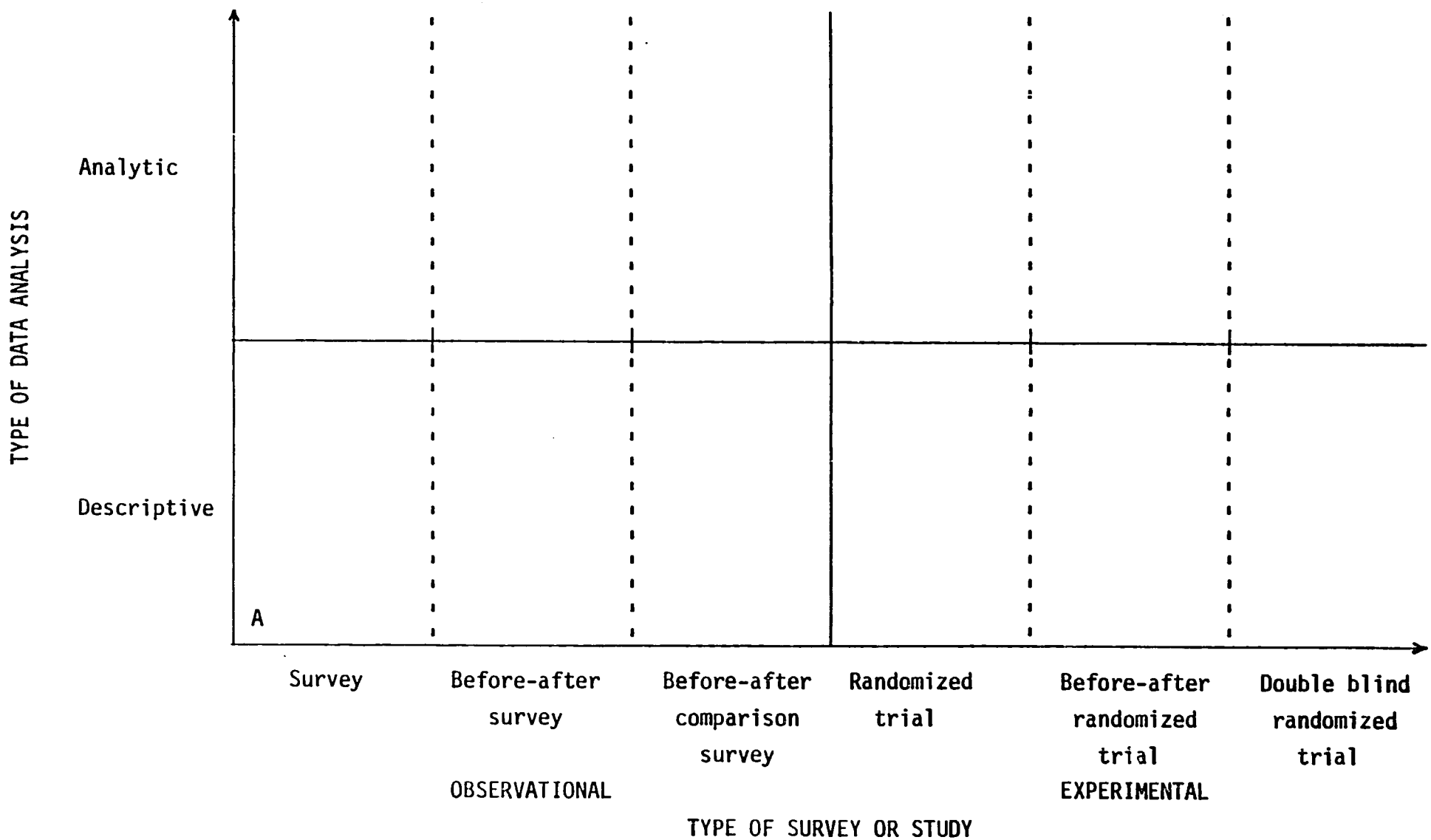

TYPE OF DATA ANALYSIS
Analytic
Descriptive
A
Survey
Before-after survey
Before-after comparison survey
Randomized trial
Before-after randomized trial
Double blind randomized trial
OBSERVATIONAL
EXPERIMENTAL
TYPE OF SURVEY OR STUDY

FIGURE 3. Potential confounding factors and their relationship to the flow of events in figure 1.

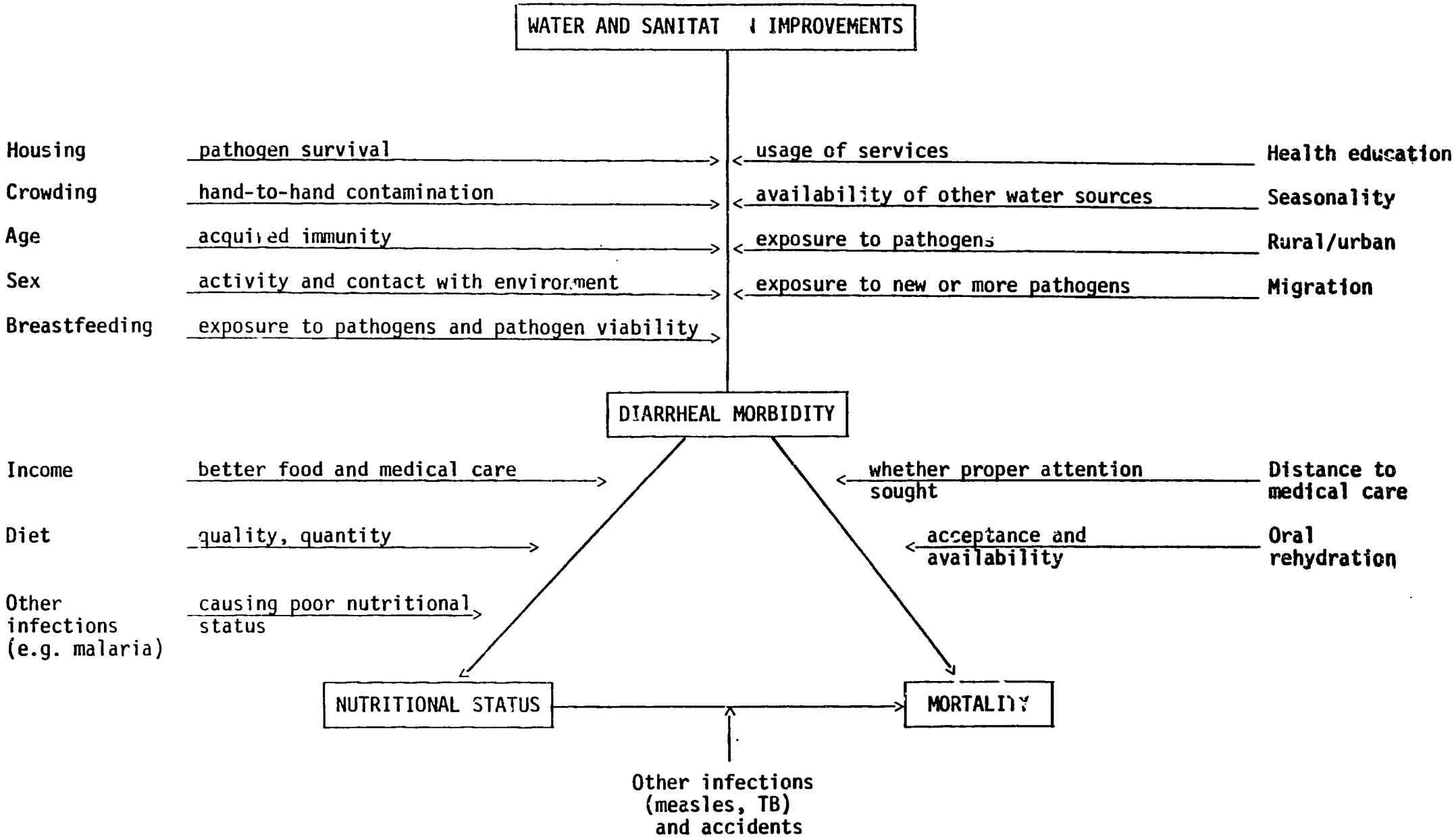
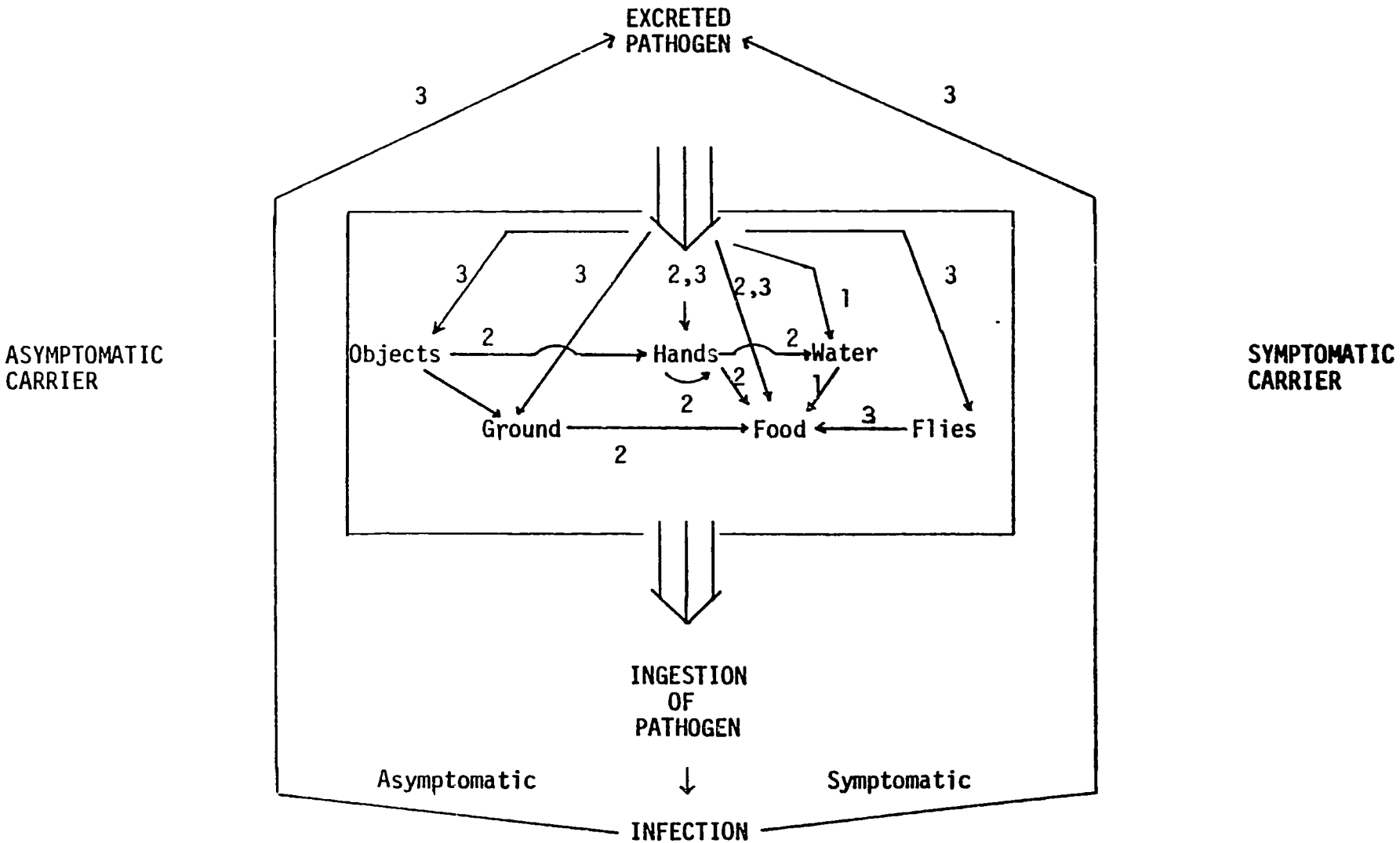

WATER AND SANITATI I IMPROVEMENTS
Housing
pathogen survival
Crowding
hand-to-hand contamination
Age
acquired immunity
Sex
activity and contact with environment
Breastfeeding
exposure to pathogens and pathogen viability
usage of services
availability of other water sources
exposure to pathogens
exposure to new or more pathogens
Health education
Seasonality
Rural/urban
Migration
DIARRHEAL MORBIDITY
Income
better food and medical care
Diet
quality, quantity
Other infections (e.g. malaria)
causing poor nutritional status
whether proper attention sought
acceptance and availability
Distance to medical care
Oral rehydration
NUTRITIONAL STATUS
MORTALITY
Other infections (measles, TB) and accidents

FIGURE 4. Schematic presentation of the fecal-oral transmission of pathogens. The points that water and sanitation interventions may break this transmission are numbered (1 = water quality; 2 = water availability; 3 = excreta disposal).


EXCRETED
PATHOGEN
3
3
ASYMPTOMATIC
CARRIER
SYMPTOMATIC
CARRIER
3
3
2,3
2,3
1
3
Objects
2
Hands
2
Water
2
2
1
Ground
Food
3
Flies
2
INGESTION
OF
PATHOGEN
Asymptomatic
Symptomatic
INFECTION