International Journal of Epidemiology
© International Epidemiological Association 1988

Vol. 17, No. 2
Printed in Great Britain

Case-Control Studies of the Effect of Environmental Sanitation on Diarrhoea Morbidity: Methodological Implications of Field Studies in Africa and Asia

JOHN BRISCOE,*‡ JANE BALTAZAR† AND BEVERLY YOUNG*

Briscoe J (Department of Environmental Sciences and Engineering, School of Public Health, University of North Carolina, Chapel Hill, NC, 27514, USA), Baltazar J and Young B. Case-control studies of the effect of environmental sanitation on diarrhoea morbidity: methodological implications of field studies in Africa and Asia. *International Journal of Epidemiology* 1988, **17**: 441–447.
The problems and prospects in the use of case-control studies to assess the effects of improvements in environmental sanitation on diarrhoea morbidity are discussed on the basis of two field studies. It is concluded that an adequate design is available for assessing the effects of a single improvement on diarrhoeal disease. The estimates of effect appear to be valid and sufficiently precise. For addressing more complex questions of interactions, sample sizes would have to be increased substantially. The experience with two field studies suggests that there is hope that a simpler protocol may be feasible, in which only limited information is collected, in which few home visits are made, and in which analytical techniques are simple. Until more field studies have been conducted definitive conclusions cannot be reached on the applicability of such a simple, rapid and inexpensive approach.

In recent years much attention has been paid to methodological issues involved in assessing the impact of improved water supply and sanitation facilities on health. It has been shown that existing studies are plagued by a series of methodological problems,[1] and that useful studies will be those which give rapid results at relatively low cost.[2] An extensive theoretical assessment of the problems and prospects of the case-control method[3] dealt with some major design issues (including sample sizes and methods for limiting potential sources of bias), suggested that the case-control method offers promise as a rapid, inexpensive and yet valid procedure, and concluded that field trials were needed.

This paper summarizes the methods and results from case-control studies on the effect of environmental conditions on diarrhoeal disease in a rural African setting (Zomba in Malawi)[4] and a peri-urban Asian setting (Cebu in the Philippines).[5] It will require more than two field studies before definitive conclusions can be drawn on the prospects for employing the case-control design as a rapid, inexpensive and relatively simple tool for assessing the impacts of environmental sanitation improvements. It is nevertheless appropriate to outline, in a tentative way, the conditions under which such simple studies might be possible, and to indicate what elaborations will be necessary when more complex questions are to be addressed and when conditions preclude the application of the simplest method.

The field settings

The rural African study was conducted as an evaluation of the impact of a Government of Malawi Rural Piped Water Supply and Hygiene Education Program. Under this programme self-help gravity water supply systems are built, supplying water through public faucets within 400 metres of the homes of the villagers. The quality of water provided through the improved system is vastly superior to that of the traditional supplies.[4] The geometric mean fecal coliform levels (per 100 ml) for piped water and water from unprotected rivers and wells are 12 and 540 respectively at the source and 16 and 760 in the house.

* Department of Environmental Sciences and Engineering, School of Public Health, University of North Carolina, Chapel Hill, NC, 27514, USA.
† Department of Epidemiology and Biostatistics, Institute of Public Health, University of the Philippines, 625 Pedro Gil, Ermita, Manila, Philippines.
‡ Current address: Room I 7025 World Bank, Washington DC 20433, USA.

The Asian study was associated with a large prospective study of child morbidity, growth and mortality in a peri-urban area in the Philippines, making it possible (when analysis of the prospective study is complete) to compare the results of the rapid, inexpensive method (the case-control study) with the time-consuming, expensive standard method (the prospective study). In the Philippines, too, there were substantial differences in the quality of water from different sources. The geometric mean fecal coliform levels for piped and borehole water were close to zero, while the mean levels for springs and dug wells were over 50 fecal coliforms per 100 ml.

METHODOLOGY

With minor variations, the field studies (described in detail elsewhere)[4,5] followed the procedures outlined in the earlier theoretical study.[2] In summary this procedure involved identifying cases as children brought to a clinic for diarrhoea and controls as children *brought to the same clinic for one of several diseases* considered to be of similar severity to diarrhoeal disease. Clinics were chosen so that they were used by families who were exposed to improved environmental conditions and families who were not so exposed. In choosing sample sizes it was assumed that: between 40% and 60% of the population used adequate water supply and sanitation facilities; it was of public health interest to detect a reduction of diarrhoeal diseases of 33% (ie an odds ratio of 0.67); there should be a 90% chance of detecting this reduction at the 5% significance level; and one control would be chosen for each case. The minimum sample size was calculated to be 460 cases and 460 controls.

The major features of the studies are as follows:

—data were collected during four-month periods corresponding to the warm-weather diarrhoea peaks;

—recruitment took place at three clinics in Malawi and 16 clinics in the Philippines;

—cases were children (under five years old in Malawi and under two in the Philippines) who were brought to a clinic because of diarrhoea;

—controls were children who were brought to a clinic because of specific diseases (essentially malaria and respiratory infections in Malawi and acute respiratory infections in the Philippines) and who did not have diarrhoea in the past 24 hours;

—390 cases and 440 controls were recruited in Malawi and 281 cases and 384 controls in the Philippines (in neither case, therefore, quite meeting the design sample size of 460 cases and 460 controls);

—data were collected both at the clinic (clinical, anthropometric and identification information) and the home (water sources, uses and quantities of water, excreta disposal practices, and socio-economic information);

—samples of drinking water were collected from both the source and the home and examined for fecal coliforms;

—children whose families used both an improved water supply and had adequate excreta disposal practices were categorized as 'exposed to good environmental sanitation' while all other children were considered 'not exposed';

—the effects of exposure were assessed by calculating both the crude odds ratios and the adjusted odds ratio by logistic regression analyses which accounted for the effects of both potential confounders (including family social, economic and demographic characteristics and feeding practices) and selection confounders (clinic of recruitment and house-to-clinic distance in both settings, and time of recruitment in Malawi).

The principal results—the adjusted and crude odds ratios—are presented in Table 1.

These results (which are discussed in more detail elsewhere[4,5]) suggest that exposure to improved environmental sanitation (both water supply and excreta disposal) is associated with a reduction in diarrhoeal disease of about 20% in both settings. In the Philippines study, where rectal swabs from cases and controls were analysed for all major diarrhoea pathogens, it was shown that when cases and controls were restricted on the basis of fecal microbiology the reduction in diarrhoea was about twice as great (ie about 40% rather than 20%).

IMPLICATIONS OF THE FIELD STUDIES

Conduct of Field Studies

From the experience gained in the two completed studies it is evident that particular attention should be given to the selection of the study site and of the

TABLE 1 *Adjusted and crude odds ratios.*

	Adjusted odds ratio and 95% CI	Crude odds ratio and 95% CI
Malawi	0.80 (0.54–1.17)	0.77 (0.54–1.09)
Philippines	0.79 (0.56–1.13)	0.80 (0.58–1.10)

NB Confounders controlled for in the adjusted analysis included family social, economic, and demographic characteristics, feeding practices, clinic of recruitment, house-to-clinic distance, and time of recruitment.

recruitment clinics. An 'ideal' study site would have a fairly even distribution of population among those considered to have 'adequate' water supply and excreta disposal facilities and practices and those considered to have inadequate facilities and practices. In Malawi, the site was selected so that about half of the population used improved water sources. Because the exposure definition ultimately used was 'good water and excreta disposal', the proportion exposed dropped to just 20%. In the Philippines the site selection was determined by the location of the associated prospective study. By good fortune it transpired that there was a fairly balanced distribution between the two exposure levels.

Recruitment clinics are chosen both to give the required balance between exposed and unexposed, and to ensure that sufficient cases (and controls) are recruited. In both settings recruitment proceeded more slowly than had been projected on the basis of records. In both settings it was also necessary to use multiple clinics in order to ensure adequate recruitment. Since exposure levels varied by clinic this required special attention in the analysis. A larger study might necessitate selecting clinics in geographically distinct locations, further increasing logistic demands.

Validity

As in any epidemiological study, the validity of the results obtained from these two studies may be compromised because of selection biases, misclassification biases and biases arising from the omission of confounding variables.

With regard to selection bias, detailed consideration was given to this problem in the study design[2] and the recommended procedures regarding definition of cases and controls closely adhered to. Differences between cases and controls in, for instance, distance from home to clinic and socioeconomic status, were found to be small. In addition, where there were differences in exposure among different groups in the study population (by clinic in both studies and by time in Malawi) these were controlled for by matching and treating these as 'selection confounders'[6] in the analysis. The procedures for dealing with selection biases appear to have been appropriate and can be recommended for future studies of this sort.

With regard to biases arising from misclassification with respect to disease status, in both studies careful attention was paid at the clinic to obtaining a detailed history of the disease from which the child was suffering, so that misclassification on the basis of disease status was minimized.

In both studies care was also taken to obtain valid information on exposure status. Information on water source used, storage and treatment practices and excreta disposal practices was collected through home visits which included inspections of facilities and, in some cases, observations of conditions. Because observations of actual water use and sanitation practices were not possible, there is a possibility that there would be some misreporting and thus some misclassification.

Finally, after taking care not to include any intervening variables (such as nutritional status and the diarrhoea disease status of other family members), the full set of potential confounders were included in each analysis. In fact, probably because many of the factors which might be expected to affect diarrhoeal disease (such as income and education) might also affect the control diseases, there was relatively little actual confounding, as shown in Table 1.

Overall, it appears that the estimates of effect of improved environmental sanitation on diarrhoea reporting to the clinic are unlikely to be seriously biased.

Precision

When the data were analysed in accordance with the study design (ie the population was grouped into two exposure categories) then the precision of the estimates of effect was satisfactory. (Under the null hypothesis, the probability of obtaining by chance as estimated odds ratio of 0.8 or less is about 15% in both settings.)

The great temptation in such studies is to try to use the data to address questions for which the study was not designed. In the particular case of water supply and excreta disposal, in many instances the policy implications of interactions are important. For instance, there is some evidence that single improvements in environmental conditions (for instance, improving water supplies) may not have a direct impact on diarrhoea, but may be necessary if subsequent improvements in excreta disposal are to be effective.

Purists would argue that such explorations should simply not be undertaken because there is no chance that statistically significant results could be demonstrated.[7] Others, however, noting that 'confidence intervals are almost always wider than one would wish'[8] argue that 'insistence on significance tests of heterogeneity of effect over subgroups as a prerequisite for subgroup analysis . . . (comes) . . . at a cost of almost total inability to recognise variability that has its roots in the population under study'.[8] Even though interactions may not be statistically significant, when supported by clinical impressions and biological plausibility, point estimates may add to our understanding.[9]

In both studies interactions between water supply and sanitation were examined. In trying to compare, say, the effect of excreta disposal with water supply versus excreta disposal without water supply, we are dealing with point estimates which are quite similar and both of which have substantial standard errors associated with them. The great imprecision of the estimate of the difference between these two imprecise estimates makes it impossible to draw any but very, very tentative suggestions from such comparisons.

We can conclude, then, that the precision of the estimates were, in both cases, sufficient for the comparisons for which the studies were designed. For examining interactions the sample sizes are much too small, and thus these studies cannot be used to assess such interactions.

PROSPECTS FOR FUTURE CASE-CONTROL STUDIES OF THE EFFECT OF ENVIRONMENTAL SANITATION ON DIARRHOEAL DISEASES

The Malawi and Philippines studies demonstrate that the substantial potential advantages (in terms of resources and time) of the case-control method relative to other approaches for assessing the impact of water supply and sanitation conditions on diarrhoeal disease can, in fact, be realized in practice. In both settings large prospective studies addressing these (and other, related, issues) were initiated four years before the case-control studies;[10,11] in neither case are definitive results from these longitudinal studies yet available.

While the Malawi and Cebu studies were far less time-consuming and costly than counterpart longitudinal studies being conducted in each setting, they nevertheless still required substantial inputs of specialized manpower, trained field staffs, substantial logistic support and good computer capacity. Health impact evaluations of water supply and sanitation programmes have been, and will continue to be, conducted by scientists who have some training in research methods but who do not have graduate degrees in epidemiology or biostatistics, and who are required to operate on modest budgets without sophisticated computer support. In such circumstances the design used in the Malawi and Philippines case-control studies is still too expensive and complex to be conducted in many of the settings in which these relationships will be investigated. On the other hand, even the present study designs are inadequate if it is essential to examine the interactions between water supply and sanitation inputs (which may be of crucial policy or research concern in some settings).

The exploration of the study designs which might be used to address these diverse problems deals with just two problems, those of validity and precision. As data collection procedures and analytical techniques are

TABLE 2 *Case-control studies of water supply, sanitation and diarrhoea—study options.*

	Data collected at:	Clinic only	Clinic and home
Sample size	Information on confounders collected?	No	Yes
		Type A	Type B
Moderate (about 500 cases and 500 controls)	(a) Cost and duration	X dollars, 6 months	2X dollars, 12 months
	(b) Complexity of logistics and analysis	Simple	Moderately complex
	(c) Validity	Currently unknown; potentially moderate	High
	(d) Questions which can be addressed	Comparisons of 1 exposed and 1 unexposed group only	Comparisons of 1 exposed and 1 unexposed group only
		Type C	Type D
Large (about 1500 cases and 1500 controls)	(a) Cost and duration	2X dollars, 12 months	8X dollars, 24 months
	(b) Complexity of logistics and analysis	Simple	Very complex
	(c) Validity	Currently unknown; potentially moderate	High
	(d) Questions which can be addressed	Interactions of water supply and sanitation	Interactions of water supply and sanitation

NB 1. All figures (on sample sizes, costs and duration) are indicative only.

2. 'X' would appear to vary between $15 000 and $30 000.

3. In some circumstances within the overall structure of Study D it may be possible to show, for a sub-sample, that confounding is not an actual source of bias. After this has been shown, for the remaining cases and controls protocol C rather than D may be followed, simplifying the study and reducing the costs.

simplified, attention has to be given to potential sources of bias; where the studies are designed to address more complex questions attention has to be given to the precision of the estimates of the added hypotheses, with accompanying concerns about sample sizes. The interplay of these forces is represented on Table 2, in which four study 'types' are presented and the characteristics (cost, duration, complexity of logistics and analysis, validity of results and type of questions which can be addressed by the study) described. The remainder of this paper deals with the prospects for the simpler study designs.

Issues in Considering a Simpler Protocol

From the Malawi and Philippines studies it is apparent that the precision in these studies is about the minimum that is acceptable. 'Simplification', therefore, cannot mean smaller sample sizes. The key question on which the hope of simplification hinges, then, is whether it is possible to reduce substantially the logistic and analytical complexity of the current design. From a logistic point of view a major demand is that of making the home visits. The home visits were made for two reasons. First, mothers who consented to participate in the study already had to stay longer at the clinic than was otherwise necessary. It was (and is) considered inappropriate to try to delay the mother still further in order to answer a variety of questions regarding potential confounding variables. Second, the home visits were undertaken in order to get the highest possible quality of information on exposure (water use and sanitation practices). The hope of simplification, then, revolves around the possibility of not conducting home visits, yet avoiding biases due to misclassification of exposure status and confounding.

Question 1: Can personnel at the clinics be relied on for information on disease status?

In both Malawi and the Philippines, after a preliminary screening by clinic personnel, the disease status of potentially eligible children was ascertained by clinically-trained project staff (nurses in all cases). For a simpler protocol it would be advantageous if the attending clinician could select cases and controls, thus obviating the need for a study staff member at the clinic. The danger is that the validity of the disease information (vital to obtaining unbiased estimates of effect) would be compromised. Subjective judgements suggest that where the level of training of the clinic personnel was relatively high (the Philippines), selecting cases and controls on the basis of reported diagnosis by the examining clinician would not introduce serious misclassification errors. However, where the personnel providing clinical services were less trained (Malawi), selection of cases and controls could not be done on the basis of the reported diagnosis, but would require additional project staff with more advanced medical training to supervise recruitment.

Question 2: Can exposure information be collected at the clinics?

Because of concern over this, questions on type of water source used were asked both at the clinic and at home (where the actual source was inspected). Assuming that the inspected source was indeed the true source, then it is possible to assess the misclassification biases which would arise if the study relied on questions at the clinic rather than the home observations. In Malawi there were substantial differences between the answers at the clinic and the answers at the home. It is suspected that this is attributable in part to the large number of water outages due to pipe breakages in the study period. In the Philippines, on the other hand, there was almost complete agreement between the home and clinic data.

The situation with excreta disposal is more problematic. In Malawi families were characterized as having 'good sanitation' if the mother said they used a latrine and an inspection showed a latrine in good order which seemed to be used; in the Philippines the child was considered to live in a sanitary environment only if the mother said they used an adequate excreta disposal facility, and if the interviewer judged there to be no fecal materal in the immediate vicinity of the household. With respect to the 'simplified protocol', there are three problems. First, even with home visits and inspections we are not quite sure how to measure whether personal sanitation practices are good or not. Second, we suspect that observation is necessary, and this would obviously not be possible if information were collected at the clinic. And third, this is a sensitive area in which the likelihood of correct answers at the clinic would always be open to question.

In any study, irrespective of the 'simplicity' or 'complexity' of the study, there should be a pre-study assessment of hygiene practices and good measures of these. This work would necessarily involve an anthropologist. In all cases the anthropologist's brief would be to develop a home interview/observation protocol for getting a measure of hygiene practice questions. Where a 'simplified' protocol is being considered, the anthropologist would also need to develop a short questionnaire which could be administered at the clinic for categorizing the sanitation practices of the family, and a protocol for validating this questionnaire through home visits and observations for a sub-sample.

Evidently no definitive conclusion can be reached concerning the validity of exposure data collected at the clinic versus data collected at the home. As with most other variables of interest to health planners,[12] there has been little attempt to assess the sensitivity and specificity of responses to questions regarding water supply and sanitation. An important need in such studies is greater attention to the development and validation of instruments for measuring hygienic behaviour.

Question 3: Can confounders be excluded?

In the Malawi and Philippines studies the standard analytical procedure (controlling for potential confounders by logistic regression analysis) was followed. To collect the necessary information on confounders entailed not only additional field work but also meant: losing some of the sample (6% and 3% in Malawi and the Philippines, respectively) because of inadequate information on potential confounders; diverting attention from the variables of greatest importance (the disease and exposure variables); and spending considerable time and effort on controlling for confounding in the analysis. To assess the possibilities of a simplified protocol for future use in rapid assessments of the impact of water supply and sanitation conditions on diarrhoeal disease, it is of interest to assess how much the estimates of the odds ratios would have changed if the confounders had simply been ignored. Table 1 shows that, comparing the crude and adjusted odds ratios for each of these studies, there was little actual confounding. This was not surprising because many of the confounders would be expected to affect the control diseases as well as diarrhoea. We can then conclude that a much simpler questionnaire, in which information was not collected on confounders but only on disease and exposure (water and excreta disposal) factors would have sufficed for these two studies. These findings suggest that there might indeed be a possibility (after other pilot studies have been performed) of developing an operational protocol in which information on confounders is not collected and analyses are based on simple tabular analyses.

Question 4: Are currently recommended sample sizes adequate?

The second set of issues to be dealt with in considering modified designs affect not the validity of the estimates of effects, but the precision of these estimates.

The Malawi and Philippines studies demonstrate that, where just two exposure categories are considered, the current recommendations[2] on sample sizes (about 500 cases and 500 controls) are adequate to get estimates of sufficient precision. In many settings the demands of the policy-makers may be more difficult to meet. For instance, there may be interest not only in a simple dichotomy, but whether there are differences between levels and combinations of service. In such cases, and in cases in which information on interactions is required, the sample size demands increase rapidly. Enormous sample sizes would be required[13] if a study were designed to detect whether, say, the reduction in diarrhoea as a result of improving the level of service from a standpipe to a yard tap is greater than the reduction as a result of moving from an unimproved source to a standpipe. The only alternative which is within the bounds of practicality would be to design a study to estimate the reductions involved first, in moving from an unprotected source to a standpipe and, second, in moving from either of these to a yard tap. The sample sizes required to do this would be higher (about 30% in one plausible case)[2] than those used in Malawi and the Philippines.

Policymakers, however, will not be interested only in the magnitude of these effects, but also the magnitude of differences between them. For a not uncommon mix of traditional sources, standpipes and yard taps,[2] to show that yard taps reduce diarrhoea 33% more than standpipes requires about 2500 in each of the case and control groups.*

We would argue that the sample sizes should be increased so that it becomes meaningful to obtain estimates of this difference (to be considered in conjunction with other evidence of such differences) and, therefore, that the sample size should reflect a balance between the demands of statisticians and the demands of practicality. For argument's sake in Table 2 we have assumed that the sample sizes should be of the order of about 1500 cases and 1500 controls if there is to be any chance of obtaining meaningful estimates of interaction (or differences between levels of service).†

* Throughout this analysis sample sizes have been calculated using conventional procedures,[14] in which the objective of the study is a hypothesis test. Since interval estimation is in fact often the goal of analysis in applied epidemiological studies, there is a growing feeling[15,16] that sample sizes should be calculated, not on the basis of a hypothesis test, but on the basis of the desired precision of the confidence interval. The effects on required sample sizes of this changed perspective can be substantial. Consider, for example, that sample sizes were calculated so that the upper limit of the confidence interval is 1.0. For odds ratios and exposure frequencies of interest in the present context, sample sizes would be less than a half of those required in a hypothesis-testing framework.[17]

† It should be noted that if a series of such studies should be done, each yielding an estimate not statistically different from zero then it would be possible to obtain an overall estimate of the 'average' magnitude of such effects with substantially greater precision.

TENTATIVE CONCLUSIONS ON THE PROSPECTS FOR A SIMPLIFIED PROTOCOL

Obviously definitive conclusions on the prospects of developing a simplified protocol for conducting case-control studies of the effect of water supply and sanitation interventions on diarrhoeal disease need to be based on a substantial number of field studies. At present only two such field studies have been conducted. The dangers of generalizing from so limited an experience notwithstanding, we believe it useful to outline some tentative conclusions which seem to emerge from these two examples.

The results of the two studies are not inconsistent with the hope for the evolution of a simplified protocol which could be implemented and analysed relatively rapidly and inexpensively where policymakers wish to compare diarrhoeal disease amongst those who do have adequate water supply and sanitation with those who do not. It would appear that some elements of such a simple approach might be:

—where there was interest in just two exposure categories, sample sizes could be about 500 cases and 500 controls;
—where there was interest in interactions between water supply and sanitation or in differentiating between the effects of different levels of service, then sample sizes will usually need to be increased to at least 1500 cases and 1500 controls;
—a modest anthropological assessment would be done before the epidemiological study to develop culture-specific questions relevant to hygiene;
—information need be collected only on disease and water use and sanitation practices;
—information can all be collected through questions administered to the mother at the clinic by the attending clinician;
—estimates of the sensitivity and specificity of the exposure data would be obtained for cases and controls by conducting follow-up home visits for a sample of all cases and controls and, on the basis of these estimates, adjustments made to the estimated odds ratios.

ACKNOWLEDGEMENTS

The Malawi and Philippines projects were funded by the US Agency for International Development and the National Academy of Sciences. Institutions which collaborated in this work include: The Ministry of Health and the Ministry of Works of the Government of Malawi, the Cebu Institute of Medicine, the Nutrition Center of the Philippines, the University of San Carlos and the University of North Carolina.

REFERENCES

[1] Blum D, Feachem R G. Measuring the impact of water supply and sanitation investments on diarrhoeal diseases: problems of methodology. *Int J Epidemiol* 1985; **12:** 357–65.

[2] Briscoe J, Feachem R G, Rahaman M M. Measuring the impact of water supply and sanitation facilities on diarrhoea morbidity: prospects for case-control methods. WHO/CWS/85.3 CDD/OPR/85.1, Geneva, World Health Organization, 1985.

[3] Briscoe J, Feachem R G, Rahaman M M. *Evaluating Health Impact: Water Supply, Sanitation and Hygiene Education.* Ottawa, IDRC Press, 1986; 80 pages.

[4] Young G, Briscoe J. A case-control study of the effect of environmental sanitation on diarrhea morbidity in Malawi. *J Epidemiol and Comm Health,* in press, 1988.

[5] Baltazar J, Briscoe J, Mesola V, Moe C, Solon F S, VanDerslice J, Young B. Can the case-control method be used to assess the impact of water supply and sanitation on diarrhea? A study in the Philippines. *Bull WHO,* in press, 1988.

[6] Miettinen O S, Cook E F. Confounding: essence and detection. *Am J Epidemiol* 1981; **114:** 593–603.

[7] Fleiss J L. Significance tests have a role in epidemiologic research: reactions to A. M. Walker. *Am J Publ Health* 1986; **76:** 559–60.

[8] Walker A M. Reporting the results of epidemiologic studies. *Am J Publ Health* 1986; **76:** 556–58.

[9] Foxman B, Frerichs R R. Response from Drs Foxman and Frerichs. Letters to the editor. *Am J Publ Health* 1986; **76:** 587.

[10] Linskog P A, Linskog, R U M. The importance of hygiene education in obtaining a health impact through improved water supply and sanitation, with examples from Malawi. Paper presented at the International Council of Scientific Unions' Committee on Teaching of Science Conference, Bangalore, India 1985.

[11] Popkin B M, Akin J, Briscoe J, Guilkey D, Black R E, Flieger W F. Infant mortality: underlying and intermediate determinants. National Institutes of Health Research Grant 1 RO1 HD19983-01A1, 1985.

[12] Habicht J P, Butz W P. Measurement of health and nutrition effects of large-scale intervention projects. In R E Klein, (ed) *Evaluating the Impact of Nutrition and Health Projects.* Plenum Press, New York 1979; pp 133–69.

[13] Smith P, Day N E. The design of case-control studies: the influence of confounding and interaction effects. *Int J Epidemiol* 1984; **13:** 356–65.

[14] Schlesselman J J. Sample size requirements in cohort and case-control studies of disease. *Am J Epidemiol* 1974; **99:** 381–84.

[15] Gardner M J, Altman D G. Confidence intervals rather than P values: estimation rather than hypothesis testing. *Br Med J* 1986; **292:** 746–50.

[16] Gordon I. Sample size estimation in occupational mortality studies with use of confidence interval theory. *Am J Epidemiol* 1987; **125:** 158–62.

[17] O'Neill R T. Sample sizes for estimation of the odds ratio in unmatched case-control studies. *Am J Epidemiol* 1984; **120:** 145–53.

(Revised version received April 1987)