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AN EDUCATIONAL INTERVENTION FOR ALTERING WATER-SANITATION BEHAVIORS TO REDUCE CHILDHOOD DIARRHEA IN URBAN BANGLADESH

II. A RANDOMIZED TRIAL TO ASSESS THE IMPACT OF THE INTERVENTION ON HYGIENIC BEHAVIORS AND RATES OF DIARRHEA

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An educational intervention was designed to improve three water-sanitation behaviors empirically shown to be associated with high rates of childhood diarrhea in Dhaka, Bangladesh: lack of handwashing before preparing food, open defecation by children in the family compound, and inattention to proper disposal of garbage and feces, increasing the opportunity for young children to place waste products in their mouth. Fifty-one communities, each comprising 38 families, were randomized either to receive ($n = 25$) or not to receive ($n = 26$) the intervention. During the six months after the intervention, the rate of diarrhea (per 100 person-weeks) in children under six years of age was 4.3 in the intervention communities and 5.8 in the control communities (26% protective efficacy; $p < 0.0001$). A corresponding improvement in handwashing practices before preparing food was noted, although no improvement was observed for defecation and waste disposal practices. These data suggest that educational interventions for water-sanitation practices can have an important beneficial effect upon childhood diarrhea in developing countries, particularly when the interventions are designed in a simple way to promote naturally occurring salutory behaviors that are empirically associated with lower rates of childhood diarrhea.

diarrhea; random allocation; sanitation; water

Although numerous studies have addressed whether interventions can reduce the occurrence of diarrheal diseases, the results of these investigations have been unsatisfactory or conflicting (1–3). While international public health recommenda-

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tions declare the importance of education in decreasing diarrheal disease (4), Feachem (5) has noted that most studies do not document this assertion. Even the few studies that have demonstrated changes in behavior after an educational intervention have found no reduction (6) or an insignificant reduction (7) in diarrheal disease. Several reviews have outlined many of the methodological problems affecting studies in this field (2, 8).

In a companion paper (9), we report on the use of case-control methodology to detect practices that are empirically associated with childhood diarrhea and that can become a logical focus for interventions. In that study, which included families from 51 slums in Dhaka, Bangladesh, practices associated with high rates of childhood diarrhea included open defecation in the living areas by children, lack of maternal handwashing before preparing food, and inattention to proper disposal of garbage and feces, with the result that young children had an increased tendency to place waste products in their mouth.

In the present study, we report on the results of a randomized trial conducted in the same 51 communities and designed to modify these three targeted behaviors. The results six months after implementation of the intervention indicate a substantial reduction in the incidence of childhood diarrhea.

Materials and Methods

General Plan

Between October 1984 and January 1985, we obtained information about socioeconomic and demographic characteristics, behaviors related to water use and sanitation, and rates of diarrhea in children under six years of age in 51 communities in urban Dhaka. From these observations, we developed an educational intervention intended to improve three behaviors that appeared to influence the incidence of diarrhea. The same 51 communities were then randomized either to receive (n = 25) or not to receive the intervention (n = 26). The intervention was implemented between March 6 and May 1, 1985, with periodic revisits to reinforce its messages thereafter. After the intervention, we obtained histories of childhood diarrhea and observed water-sanitation behaviors. Rates of diarrhea and water-sanitation behaviors were then compared in the intervention and nonintervention communities for the six months following implementation of the intervention.

Selection and eligibility of communities and families

We selected 51 sites from geographically disparate impoverished areas of urban Dhaka. Persons living in each site received basic primary health care services from a resident community health worker from the Urban Volunteer Programme of the International Centre for Diarrhoeal Disease Research, Bangladesh (10). From each of the 51 sites, we selected a "community," which was defined as a cluster of 38 families occupying contiguous households beginning five houses away from the home of the community health worker. A family consisted of persons residing together and sharing the same cooking pot. The perimeters of seven of the communities lay within one-half mile of the perimeter of the next closest community, and three of the communities were within sight of each other. The remaining 41 communities were separated by a distance of one to 12 miles. From the 1,938 original families, 15 emigrated before completion of the census at the beginning of the study, so that the initial population consisted of 1,923 families, 1,350 with children under six years of age.

In each community, five families having at least one child aged under six years were randomly chosen as "sentinel" families for on-site observations of behaviors related to water use and sanitary practices. Eight of the 255 selected families refused observation or emigrated before the census, leaving 247 sentinel families at the outset of the study. If a family emigrated from a community and another family subsequently occupied the vacated residence, the new family was included in the study regardless of the date of in-migration. Although this eligibility procedure might have reduced the apparent efficacy of the intervention by including families that were absent for part or all of the major educational program, we included these in-migrant families because we were interested in assessing the practical effectiveness of the intervention upon communities, which are inherently dynamic demographic entities. On the other hand, since behavioral observations before and after the intervention were used to assess whether the training had affected behaviors among those fully participating in the educational program, we did not include for behavioral observations any family that immigrated into the study area after the intervention had begun.

Baseline observations

A census enumeration, including socioeconomic data for each family, was obtained in September 1984. From October 1984 through February 1985, fortnightly histories of diarrhea were obtained for children of all families, and a single prolonged on-site visit was made to the home of each sentinel family for observation of water-sanitation behaviors.

To obtain histories of diarrhea during the baseline period, 13 trained interviewers conducted fortnightly interviews of mothers or caretakers to determine whether their children aged under six years had had an episode of diarrhea (defined as three or more loose stools in any 24-hour period; see comparison paper for full definition (9)) during the past two weeks. To improve the accuracy of these histories, we encouraged mothers to keep "calendars" to record episodes of diarrhea for each child. The calendars were specially designed for use by illiterate adults. Supervisors made frequent unannounced spot checks to verify the accuracy of the histories.

For the observation of water-sanitation behaviors, a trained field research officer made an unannounced visit once during the baseline period to each sentinel family. Each visit began in the early morning and lasted from three to five hours. A highly structured data instrument was used to ensure systematic notation of observations. The five field officers responsible for the visits were assigned to families in a randomized fashion to safeguard against bias that might have occurred if the temporal order of visits for different clusters varied in a systematic fashion. Moreover, two supervisors made unannounced, random spot checks of the observations to verify completeness and accuracy of the observations.

Intervention

Formulation of the intervention. As described in detail elsewhere (9), a comparison of the practices of sentinel families with high rates of childhood diarrhea versus the practices of sentinel families with low rates of childhood diarrhea during the baseline period revealed only three behaviors to be associated with childhood diarrhea: failure of mothers to wash their hands before preparing food; defecation by young children in the living area of the family; and inattention to proper disposal of garbage and feces from the living area. From these observations, we developed three educational messages emphasizing proper handwashing before food preparation, defecation away from the house and in a proper site, and suitable disposal of waste and feces, thus preventing access to waste products by young children.

Allocation of the intervention. Because of the small number of communities allocated to the intervention and control groups, and the consequent possibility that randomization per se might fail to equalize baseline risks of childhood diarrhea in the compared groups, we employed stratified randomization. We first ranked the 51 communities from highest to lowest according to their age-adjusted rates of childhood diarrhea. We then grouped the ordered communities into 25 adjacent pairs and one remaining community (having the lowest rate). The pairs each comprised a stratum. Within each stratum, one community was assigned to the intervention group and one to the control group with use of a random number table. For the one unpaired community, allocation was also random.

Implementation of educational intervention. The three messages formed the basis of an intensive training program conducted for eight weeks in 25 communities. Seven experienced trainers working with the authors and a local nontraditional educational facility, Village Education Resource Center, developed an educational program. The intervention approach included small-group discussions including only women or only children, larger demonstrations to mixed audiences, and community-wide planning and action meetings which included husbands. The training was conducted by the trainers and the 25 Urban Volunteer Programme community health workers living next to the 25 intervention communities. Posters made by the trainers and the community members, games, pictorial stories, and "flexiflans" (a flannel board with movable characters) (11) were developed to illustrate the messages.

Following the eight weeks of intensive training, one trainer and the community health workers continued to reinforce the educational message through new stories, games, and community organization in all 25 communities. The 26 community health workers in the nonintervention communities continued to provide the primary health care services cited earlier (10).

Assessment of outcomes

Beginning two weeks after initiation of the intervention, we evaluated the impact of the intervention upon observed hygienic practices and the incidence of childhood diarrhea for the next six months (March 20 through September 20, 1985). Utilizing the format described for the baseline period, we made one on-site behavioral observation for each sentinel family and obtained fortnightly histories of diarrhea for all children.

Analysis of data

Statistical comparisons of categorical baseline variables were made with use of the chi-square test or Fisher's exact test where appropriate. Comparisons of dimensional baseline variables employed the Mann-Whitney U test. To express the association between the intervention and the incidence of childhood diarrhea, we calculated incidence density ratios. A ratio of one corresponded to no association; values less than one corresponded to reduction in the incidence of diarrhea in the intervention group (e.g., protection); and values greater than one denoted an excess of diarrhea cases in the intervention group. The value (one minus incidence density ratio) denoted the proportionate reduction in diarrhea attributable to the intervention (e.g., protective efficacy). Precision-weighted incidence density ratios were calculated to summarize values over several strata; levels of significance and 95 per cent confidence intervals for incidence density ratios were calculated by standard techniques (12). To assess the effect of the stratified randomized allocation of the 51 communites to the intervention and control groups, we performed separate analyses, preserving the paired-community strata and using the above techniques to derive summary incidence density ratios and associated levels of significance.

Because clusters of individuals rather than individuals per se were the units of randomization and hence the appropriate units of analysis, we also compared incidence rates of diarrhea of the intervention and control communities according to the matched-pair strata with the Wilcoxon matched-pairs test, treating the incidence rate of each community as a single observation rather than a group of observations (13). This tactic yielded a very conservative estimate of statistical significance, since it estimated the variance of incidence rates by treating communities as single observations and since, as a nonparametric test, it made no distributional assumptions. Finally, to examine the correlation of the magnitude of the incidence density ratio for each matched pair, on the one hand, with the incidence of diarrhea in the control community for the pair, on the other hand, we calculated Spearman's rho. All p values were interpreted in a two-tailed fashion.

Results

Comparability of intervention and control areas in the baseline period

Table 1 shows the comparative demographic and socioeconomic profiles of the 937 intervention families and 986 control families in the 51 communities. Family size, ages of children, levels of education, family income, ownership of property, type of house construction, and access to sanitation facilities were similar. Before the intervention, the rates of diarrhea in the two groups were nearly identical, as were the frequencies of behavioral practices that were to be the focus of the intervention.

Comparison of hygienic practices after the intervention

After the intervention, significantly more mothers washed their hands before preparing food in the intervention areas (39/79 (49 per cent)) than in the nonintervention areas (25/75 (33 per cent)) ($p < 0.05$). There was no decrease in open defecation in the living area by ambulatory children (intervention 18/27 (67 per cent), nonintervention 12/19 (63 per cent)). Although the number of households with uncovered garbage or feces in the compounds did not differ (intervention 46/81 (57 per cent), nonintervention 41/76 (54 per cent)) and the number of households in which children

TABLE 1

Baseline sociodemographic, diarrheal, and behavioral variables in 937 intervention and 986 control families residing in Dhaka, Bangladesh, from October 15, 1984 to February 28, 1985

Variable	Communities	
	Intervention	Control
Demographic*		
No. of children by age (years) (% of total population)		
<1	166 (3)	175 (3)
1	181 (4)	161 (3)
2	169 (3)	187 (4)
3	210 (4)	201 (4)
4	167 (3)	171 (3)
5	167 (3)	164 (3)
Median years of education (range)		
Maternal	0 (0–16)	0 (0–18)
Paternal	0 (0–17)	2 (0–19)
Median no. of persons in family (range)	5 (1–15)	5 (1–19)
Socioeconomic*		
Median monthly family income† (range)	1,000 (50–30,000)	1,000 (50–35,000)
No. (%) of houses constructed with thatch	537 (57)	531 (54)
No. (%) of families with ≥1 radio	256 (27)	308 (31)
No. (%) of families with ≥1 watch	383 (41)	425 (43)
No. (%) of houses with one room	276 (29)	309 (31)
No. (%) of families with tap or tubewell water	162 (17)	239 (24)
No. (%) of families with sanitary or pit latrine	394 (42)	394 (40)
No. of episodes of diarrhea in children (per 100 person-weeks)*	7.3	7.6
Behavioral‡		
No. of families in which mothers wash hands before serving food (% first episode observed)	57 (62)	65 (65)
No. of families in which children defecate in living area (% first episode observed)	20 (59)	22 (63)
No. of families in which garbage, feces are in living area (% sentinel households)	64 (63)	77 (67)
No. of families in which children put garbage in mouth (% sentinel households)	43 (42)	41 (36)

* All families: 937 intervention, 986 control.

† Expressed as takas per month. (One US dollar = 29 takas.)

‡ Sentinel families only (102 intervention, 115 control); 30 families migrated out before observations were made.

were observed to put garbage in their mouth remained similar between the intervention (27/81 (33 per cent)) and control areas (21/76 (28 per cent)), several field officers noted that the amount of garbage and feces had decreased in the households in the intervention areas.

Comparison of rates of diarrhea after the intervention

The incidence of diarrhea experienced by children in the intervention areas was 26 per cent lower than that for children in the nonintervention areas after the intervention (incidence density ratio = 0.74; $p < 0.0001$) (table 2). The difference was particularly striking for the two- and three-year-old children in whom the incidence was substantially lower in the intervention areas (incidence density ratio = 0.54 and 0.68, respectively; $p < 0.0001$ and $p < 0.001$) (see table 3). The comparative rates of diarrhea between the intervention and nonintervention communities remained relatively constant throughout the postintervention period.

Analysis of the communities according to the paired fashion in which they were randomized (table 4) shows that in 16 of the 25 pairs, the intervention community experienced a lower rate of diarrhea, and in seven pairs, a statistically significant reduction in diarrhea occurred in the intervention community. The summary incidence density ratio (0.75; $p < 0.0001$) for this matched analysis reflected a 25 per cent reduction in childhood diarrhea attributable to the intervention. Even when communities were treated as individuals, the comparison remained significant ($p < 0.05$)

TABLE 2

Total episodes of diarrhea and person-weeks of observation in children <6 years residing in Dhaka, Bangladesh, in postintervention period, March 20–September 20, 1985

	Communities		Incidence density ratio	95% confidence interval
	Intervention	Control		
Total episodes of diarrhea	653	914		
Person-weeks of observation	15,212	15,810		
Episodes/100 person-weeks of observation	4.29	5.78	0.74*	0.67–0.82

* $p < 0.0001$.

TABLE 3

Episodes of diarrhea per 100 person-weeks of observation in Dhaka, Bangladesh, in children in intervention and control communities for the postintervention period March 20–September 20, 1985

Age (years)	Communities		Incidence density ratio	95% confidence interval
	Intervention	Control		
0	5.4‡	7.3	0.76	0.55–1.05
1	6.1	6.7	0.92	0.75–1.13
2	4.1	7.6	0.54**	0.43–0.66
3	4.3	6.2	0.68*	0.54–0.85
4	3.1	3.3	0.93	0.69–1.25
5	3.5	3.9	0.92	0.68–1.21
Overall			0.75**,†	0.68–0.83†

* $p < 0.001$.

** $p < 0.0001$.

† Mantel-Haenszel summary estimates of the incidence density ratio and the associated level of significance over the age strata.

‡ Episodes of diarrhea per 100 person-weeks of observation.

TABLE 4

Comparison of rates of diarrhea of children <6 years of age after educational intervention (March 20–September 20, 1985) between intervention and control communities in Dhaka, Bangladesh, in 25 matched pairs

Episodes of diarrhea per 100 person-weeks of observation		Incidence density ratio	95% confidence interval
Intervention communities	Control communities		
2.0	1.2	1.72	0.63–4.76
1.2	1.2	1.01	0.36–2.91
3.5	1.9	1.82	0.41–3.70
0.5	2.3	0.21**	0.08–0.59
2.3	2.4	0.95	0.49–1.89
1.4	2.6	0.54	0.22–1.30
3.6	2.6	1.41	0.77–2.63
2.9	3.1	0.94	0.51–1.75
11.2	3.2	3.50***	2.23–5.49
3.3	3.7	0.91	0.56–1.49
0.9	3.7	0.27***	0.12–0.58
2.9	3.9	0.75	0.40–1.43
6.1	4.1	1.47	0.90–2.42
3.6	4.1	0.87	0.50–1.52
6.4	4.3	1.49	0.94–2.32
3.3	4.3	0.78	0.42–1.52
5.2	4.4	1.18	0.64–2.17
1.2	7.3	0.17***	0.06–0.43
0.6	8.8	0.07***	0.03–0.14
2.9	9.3	0.32***	0.20–0.51
12.7	9.4	1.35	0.94–1.92
4.2	10.7	0.41**	0.22–0.72
7.6	11.0	0.69	0.47–1.02
2.2	11.6	0.19***	0.12–0.31
13.6	17.0	0.78	0.54–1.12
		0.75****,†	0.66–0.84†

** $p < 0.01$.
*** $p < 0.001$.
**** $p < 0.0001$.
† Mantel-Haenszel summary estimate of incidence density ratio and associated level of significance over the matched-pair strata.

by the conservative Wilcoxon matched-pairs test.

Examination of the data in table 4 revealed a significant inverse correlation between the incidence density ratio for each matched pair and the incidence of diarrhea for the control community of the pair (Spearman's rho = −0.45; $p < 0.05$). If the incidence of diarrhea for each control community can be taken as an estimate of the incidence for its matched intervention community in lieu of the intervention, this correlation suggests that the magnitude of the intervention's protective effect varies directly with the incidence of diarrhea in the community in which the intervention is imposed.

DISCUSSION

Our results suggest that a simple educational message designed to alter behaviors empirically associated with childhood diarrhea can both modify behavioral practices and lower rates of diarrheal disease. The fact that the reduction in diarrhea, while present for all age groups, was most marked in children aged two and three years perhaps reflects the diminished importance of maternal handwashing and clean immediate surroundings for infants and very young children, most of whom are breast-fed and are nonambulatory, and for highly ambulatory older children who have extensive contact with environments outside their family compounds. Chance occurrence, inadequate delivery of the educational message, and poor community compliance are all plausible explanations for the lack of success in those community pairs in which rates of diarrhea were not improved by the intervention.

Blum and Feachem (8) have outlined many of the weaknesses of the current water and sanitation literature, including lack of controls, inadequate account of confounding variables, simple one-to-one community comparisons, nonconcurrent comparisons, inadequate monitoring of behavioral change, ambiguous definitions of diarrhea, and inadequate indicator recall. Because the present study randomly assigned the intervention and control regimens to numerous distinctive communities, with demonstrable comparability of the intervention and control groups at baseline, and because the intervention and control groups were followed concurrently after randomization, we believe that the study employed adequate safeguards against biases that may have weakened past studies (14). Moreover, we also monitored behavioral outcomes, provided explicit definitions of episodes of diarrhea, and attempted to enhance the validity of histories of diarrhea by use of home-maintained health calendars (15).

Potential limitations of the study

Certain other problems, however, warrant consideration. This study was not performed in a double-blinded fashion. Nevertheless, we attempted to avoid detection bias by using separate personnel for implementing the intervention and for observing the effects of the intervention, and by providing no incentive to the research personnel or study population to report rates of diarrhea that would reflect success of the intervention. Also arguing against bias was the lack of effect of the intervention on rates of diarrhea in certain ages (four and five years), as well as on scabies, which was simultaneously monitored after the intervention and whose occurrence bears no logical relationship to the intervention (B. Stanton, unpublished observations). That only one of the three behavioral practices differed after the intervention also suggests that reporter bias by the research team was unlikely, since such selective misreporting would be most unlikely. Finally, the results obtained for routine two-week diarrhea recalls by the interviewers and by unannounced, independent, verification visits by supervisors showed a high level of concordance, which was similar for the intervention and control communities.

Biases could also have resulted from the high rate of migration observed in the urban study areas, with the potential for differential migration patterns in the intervention and control communities. After the initial census enumeration, equivalent percentages of intervention and control communities immigrated (19 per cent in intervention communities vs. 23 per cent in control communities) or emigrated (38 per cent in intervention communities vs. 37 per cent in control communites). The demographic characteristics and rates of childhood diarrhea during the baseline period were similar in the compared groups for both emigrant and immigrant families. Moreover, restricting the analysis to only those persons originally enrolled in the study produced similar postintervention rates of diarrhea (intervention communities: 4.16 episodes per 100 weeks; nonintervention communities: 5.65 episodes per 100 weeks, $p < 0.0001$). It is therefore difficult for us to attribute the apparent impact of the intervention to migration bias.

Because the control and intervention groups were followed concurrently and because water-sanitation behaviors and rates of diarrhea were comparable at baseline for the two groups, we assessed the impact of the intervention upon behavioral patterns and rates of diarrhea by focusing exclusively on events occurring after onset of the intervention. At the same time, it might seem anomalous that the improvement in handwashing behavior after the intervention resulted from a lesser deterioration of handwashing practices relative to baseline in the intervention communities than in the control communities. Similarly, it could also be pointed out that relative to baseline, rates of diarrhea decreased in both groups, but to a greater extent in the intervention group. These observations illustrate the fallacy of nonconcurrent comparisons in countries such as Bangladesh, where marked seasonal changes occur for rates of diarrhea and associated behavioral practices. The transition from the baseline period to the postintervention period in this study coincided with the onset of the monsoon season, when flooding covers available taps and tubewells in many parts of the city and generally impedes access to sources of putatively clean water. This seasonal change may be responsible for the general decrease in handwashing for both the intervention and control communities. By contrast, heavy rains, which wash away debris, may have resulted in the overall decrease in environmental garbage seen during this period compared with the dry season.

Next, although unannounced spot checks were made to verify the accuracy of the recording of observations, no attempt was made to determine the consistency of behaviors in a given household from one day to the next. Such fluctuations in behavior undoubtedly occurred, thereby diminishing the sensitivity of single observations for detecting postintervention alterations in behavior. Thus, while it is possible that the decrease in diarrhea observed in the intervention communities was due solely to changes in handwashing practices, it may be that our observational tool was too insensitive to detect corresponding changes in other behaviors.

Finally, while no specific attempt was made to monitor for "spillover" of the educational message from intervention to control communities, the distance and large numbers of persons separating communities made such an occurrence unlikely. Moreover, such contamination would have lessened the measured impact of the intervention and would have constituted a major concern only if the study had failed to find an effect.

Implications of the findings

Although consistent with logical expectation, our results are more striking than results from previously reported educational interventions. In addition to differences in experimental design, there are several possible explanations for the magnitude of protection observed in this study.

With some obvious exceptions (7), the training approach utilized has rarely been described in detail in published reports of water and sanitation interventions (16). Whether this absence reflects simple omission or a more fundamental lack of attention to the educational component of the interventions cannot be determined. In the present study, we devoted considerable attention to the delivery of our educational program. We attempted to include community members in all stages of the program, and we addressed the recognition of the importance of different audiences and the necessity of being able to respond to organized community requests (17, 18). We attempted to use materials that were developed for a largely uneducated audience and that involved audience participation (11) and to involve the community in identifying both the needs of and solutions to their health problems (17).

Nevertheless, some studies have described an elaborate and appropriate training approach (7), and others have described behavioral changes that suggest that the training used was appropriate (6). Yet, these studies did not demonstrate significant decreases in diarrheal disease. We suggest that this may be because the educational programs offered too many prohibitions and recommendations and were developed without specific attention to empirical associations with disease or to the acceptability of the recommendations. The results from this study both validate the usefulness of empiric behavior-diarrhea associations detected in a case-control study (9) and illustrate the substantial impact of an appropriately conceived educational intervention upon childhood diarrhea in impoverished settings.

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