Effectiveness of an educational intervention delivered through the health services to improve nutrition in young children: a cluster-randomised controlled trial

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Summary

Background Malnutrition is the underlying cause of half of child mortality. Many programmes attempt to remedy this issue but there is a lack of evidence on effective ways to decrease child malnutrition.

Methods We did a cluster-randomised trial of an educational intervention in a poor periurban area (ie, shanty town) of Peru. Guided by formative research, the intervention aimed to enhance the quality and coverage of existing nutrition education and to introduce an accreditation system in six government health facilities compared with six control facilities. The primary outcome measure was growth that was measured by weight, length, and *Z* scores for weight-for-age and length-for-age at age 18 months. Main secondary outcomes were the percentage of children receiving recommended feeding practices and the 24-h dietary intake of energy, iron, and zinc from complementary food at ages 6, 9, 12, and 18 months. Analysis was by intention to treat.

Findings We enrolled a birth cohort of 187 infants from the catchment areas of intervention centres and 190 from control areas. Caregivers in intervention areas were more likely to report receiving nutrition advice from the health service than were caregivers in control health facilities (16 [52%] of 31 *vs* 9 [24%] of 37, p=0·02). At 6 months more babies in intervention areas were fed nutrient-dense thick foods at lunch (a recommended complementary feeding practice) than were controls (48 [31%] of 157 *vs* 29 [20%] of 147; difference between groups 19 [11%], p=0·03). Fewer children in intervention areas failed to meet dietary requirements for energy (8 months: 30 [18%] of 170 *vs* 45 [27%] of 167, p=0·04; 12 months: 64 [38%] of 168 *vs* 82 [49%] of 167, p=0·043), iron (8 months: 155 [91%] of 170 *vs* 161 [96%] of 167, 9 months: 152 [93%] of 163 *vs* 165 [99%] of 166, p=0·047), and zinc (9 months: 125 [77%] of 163 *vs* 145 [87%] of 166, p=0·012) than did controls. Children in control areas were more likely to have stunted growth (ie, length for age less than 2 SD below the reference population median) at 18 months than children in intervention groups (26 [16%] of 165 *vs* 8 [5%] of 171; adjusted odds ratio 3·04 [95% CI 1·21–7·64]). Adjusted mean changes in weight gain, length gain, and *Z* scores were all significantly better in the intervention area than in the control area.

Interpretation Improvement of nutrition education delivered through health services can decrease the prevalence of stunted growth in childhood in areas where access to food is not a limiting factor.

Introduction

Inadequate nutrition during the first 2 years of life can lead to morbidity in childhood, and is one of the most important preventable risk factors for mortality. Undernutrition in childhood has been estimated to cause half of all preventable deaths in infants worldwide.[1] In Peru, like in many developing and transitional societies, the most common nutritional problems are stunting[2,3] and iron-deficiency anaemia.[2,4,5] Inadequate nutrition might be caused by lack of access to sufficient food or by lack of variety and quality, especially for micronutrients of high bioavailability. However, caregivers might not make the best use of available resources because of cultural beliefs and practices, lack of knowledge of the best foods for young children even when available in the home, and inappropriate advice.[6,7] In these circumstances, interventions that provide additional complementary food can prevent growth retardation,[8] especially for children 6–12 months old.[9]

Large-scale educational interventions have also been effective in changing the way caregivers give food, increasing dietary intake, and in improving child growth,[8] although results have usually been based on preintervention versus postintervention assessments or on comparisons with children not included in the education programme. Two controlled trials[10,11] showed that community-based culturally appropriate nutrition education can improve infant-feeding practices, dietary intake, and growth. However, interventions that are heavily dependent on community-based strategies are limited in many countries because sustainability depends on political expediency or on the continued presence of non-government organisations. In countries where government health services provide wide coverage and are easily accessible, these services are a logical and more sustainable channel for educational interventions. However, few randomised trials have measured the effect of child-nutrition educational interventions implemented through health services. High turnover of staff and difficulty of training all health professionals involved in counselling hinders interventions that focus on training individuals.

To assess the potential benefit of health-service educational interventions, trials that measure the effect on the general population are needed. We did a cluster-randomised effectiveness trial of a nutrition education programme that focused on improving the service rather than improving individual service providers. The trial was done in poor periurban communities (shanty town areas) in Peru. We measured the effect of the 2-year study on feeding practices, dietary intakes, and growth in catchment populations of 12 participating health services, and present results from a cohort of children born during the intervention and followed up until age 18 months.

Methods

Participants

The study took place in Trujillo, a city located 400 km north of Lima with a population[12] of 600 000. Health facilities of the government of Peru serve populations that live in periurban shanty towns characterised by inhabitants with a low and insecure income, poor housing, and by a general lack of one or more essential services (ie, piped water, reliable electricity supply, and sewage disposal). However, various nutritious foods are available to almost all families. Acute malnutrition (ie, low weight for length) is unusual in children in these areas, but anaemia and growth faltering that leads to stunted growth are common. Patterns of growth in children are similar to those in periurban settlements in Lima:[2,13,14] average birthweight[2] is 3·2 kg, 5% of children have low birthweight, and weight and linear growth fall behind international reference growth curves from age 4 months. In children about 18 months or older, average growth is similar to reference data. Although there can be a catch-up period of weight gain, average length remains low for age.[13]

Procedures

The intervention was done as a programme of the regional health authority, which appointed a commission to assist and monitor the intervention. The design of the intervention, measurement of effect, and technical assistance during implementation were provided by the project. We took care to ensure that activities of the intervention enhanced existing activities and were sustainable. This cluster-randomised control trial was approved by the ethics committee of the Instituto de Investigación Nutricional, Lima, Peru. Families who participated were informed of the study protocol and signed consent was obtained.

Health facilities serving periurban communities in Trujillo were eligible unless they were unique in some way (ie, had a characteristic not found in any other centre). Facilities were divided into three types: community hospitals offering maternal and perinatal specialist services; health centres with medical staff always in attendance; and health centres with more limited services. Facilities were paired for similar socioeconomic conditions in catchment populations (on the basis of local knowledge of personnel in the health region).

Randomisation was done before formative research to avoid it acting as an intervention. Starting with the community hospitals, one of each pair (ie, two centres of the same type and broadly similar characteristics) was assigned to intervention or control by the investigator (MP) tossing a coin in the presence of the local health authorities. Because families might attend a close-by health facility rather than the intended catchment facility, we excluded a pair if the randomisation resulted in a control site being directly adjacent to an intervention site. Randomisation of pairs continued until we had completed the predetermined number of six pairs of facilities. The seventh pair was held in reserve.

We studied the intervention's implementation process and the effect of the intervention on child outcomes. The assessment of the process—which involved use of structured observations in the health facilities, interviews of caregivers, and a cross-sectional survey—will be published elsewhere. To assess the effect of the intervention, a cohort of children was followed up from birth to age 18 months, and a final survey was done at the end of the experimental phase. Data were collected by project field workers who were not involved in the delivery of the intervention.

The intervention aimed to raise the profile of nutrition in the health facilities and to integrate nutrition services into existing child-oriented national programmes such as immunisation, monitoring of growth and development, and management of acute respiratory infections and diarrhoea. We aimed to enhance the quality of nutrition counselling through training and provision of simple, standardised, age-appropriate messages to be used at all points of contact with young children in the facility. Materials available in health facilities were adapted for the study and provided as flip charts and single-page recipe fliers. Three key messages were designed and disseminated among all staff in the facilities that had any contact with caregivers of young children. These messages were: a thick puree satisfies and nourishes your baby, equivalent to three portions of soup: at each meal give puree or thick-food preparation first; add a special food to your baby's serving: (chicken) liver, egg, or fish; and teach your child to eat with love, patience, and good humour. Facilities were assisted in developing their own protocols for use of the educational materials, and clinical history forms were designed to help prompt physicians to include brief questions and advise on nutrition. The intervention included demonstrations of the preparation of complementary foods, and group sessions for caregivers of children of similar ages were added to enhance the coverage and the nutrition content of the growth-and-development-monitoring programme in well-baby clinics. The intervention also provided training to improve anthropometry skills in health-care workers.

We introduced an accreditation scheme as a mechanism for institutional change, two rounds of which were done during the course of the intervention. Accreditation was done by two local health professionals (who were selected by the health region and the project team) and by project workers (who were trained by the project team). Accreditation was based on the satisfaction of previously defined criteria that measured the health service compliance with the intervention and was done by a review of health-facility records, observation of contact with patients, interviews with caregivers of young children on leaving the facility, and by a few home visits to caregivers who had visited the facility in the preceding 2 weeks. Structured instruments with a defined format were used throughout, and results were given to the facilities immediately after the accreditation visit. Accredited facilities received public recognition and a commemorative plaque to place in the clinic.

We hypothesised that the intervention would lead to improved feeding practices, dietary intakes, and growth of children in the catchment areas of the intervention health facilities. The primary outcome was growth measured by weight, length, and *Z* scores for weight-for-age and length-for-age at age 18 months. Secondary outcomes were the proportion of children receiving recommended feeding practices and the 24-h dietary intake of energy, iron, and zinc from complementary foods at ages 6, 9, 12, and 18 months. We enrolled a cohort of newborns after a census of the catchment population of each health facility. In small catchment areas, the whole area was visited to identify all pregnant women; larger areas were divided into geographical districts defined by the health facilities and a subsample was enrolled from each district. In every district, a block was chosen at random and then a single house was randomly selected as a starting point for the census. When babies were born in these households their parents were invited to participate. Additional babies were identified by extending the census to contiguous areas.

The first training activities of the intervention started in May, 1999. Newborns who were found at home, who were aged 10 days or younger, who had no known congenital malformation or chronic condition that could affect growth, and whose parents gave written informed consent were enrolled between Aug 13, 1999, and Feb 10, 2000, and followed up until 18 months of age. Families were not told whether they were in the intervention or control group.

Fieldworkers visited the families as soon as possible after birth and when the baby was 3, 4, 6, 8, 9, 12, 15, and 18 months old—crucial ages at which to assess nutrition, feeding practices, and growth. At the first visit, fieldworkers collected information on family composition and socioeconomic conditions and, at each subsequent visit, questioned families about the use of health services and exposure to nutrition messages during the period between interviews; they also used a standardised, structured-observation technique to assess home hygiene. Reported signs of illness during the previous 24 h were used as an estimate of overall population morbidity.

At each visit, children were weighed on clock balances accurate to 100 g that were calibrated weekly, and recumbent length was measured with locally made, rigid stadiometers accurate to 0·1 cm. Data collectors were trained and standardised according to WHO guidelines[15] before the study started. Questions about age-specific feeding practices were asked at each visit. At 6, 8, 9, 12, 15, and 18 months, trained nutritionists did a second interview to estimate dietary intakes using quantitative 24-h recall. Weighing of food portions with digital balances (maximum weight 2 kg and precision 1 g), use of household utensils, and locally made food models (representing different sizes of food or of food portions) helped estimate portion sizes and the ingredients of mixed dishes consumed by the child.

Statistical analysis

Financial and practical factors limited the number of health services in which we could work, and six health facilities were thus thought to be the minimum acceptable number of health services per group. We did not have information from the population to calculate intracluster correlation, but did expect the design effect to be quite low. The sample size for the number of individuals in the cohort was calculated for 80% power and 95% CI for the primary outcomes. Based on the frequency of reported exclusive breastfeeding in coastal cities excluding Lima,[2] we anticipated differences of 15% in exclusive breastfeeding (increase from 15% to 30% at 2 months [n=133 per randomised group], and from 10% to 25% at 4 months [112 per group]). Furthermore, we expected a 15% difference in the frequency of recommended complementary feeding (ie, breastfeeding plus two or more energy-dense preparations, from 15% to 30% [133 per group]), and a minimum difference of 112 kcals per day, 2 mg iron per day, and 1 mg zinc per day in children age 6–12 months. These estimates were based on observed feeding practices in similar periurban communities in Lima.[16] A difference of 0·5 cm at 1 year was predicted for linear growth.[13]

To account for the cluster design and the anticipated (low) loss to follow-up, we increased the sample size by 25% and aimed to enrol 180 children per group (30 per cluster). We did not make an allowance for the different sizes of cluster catchment areas.

Data were collected on forms that were previously piloted, checked by field supervisors, entered in purpose-designed programmes (which included range and consistency checks), and analysed with SPSS version 10. Socioeconomic information was analysed by use of principal-component analysis, and three clusters of associated variables were identified: housing, possessions, and educational level of the parents. A hygiene score was calculated from the observations of the

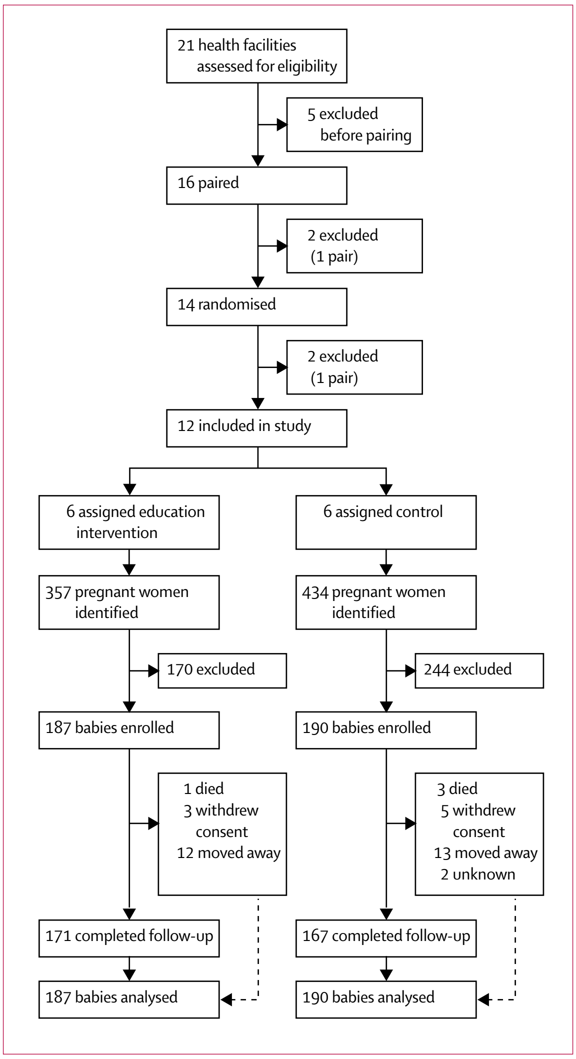

Figure 1: Trial profile

	Intervention (n=187)	Control (n=190)
Number of males	87 (54%)	98 (48%)
Housing score*	3·42 (1·05)	2·93 (1·35)
Mother's education level (number completing secondary school)	97 (52%)	68 (36%)
Father's education level (number completing secondary school)	109 of 167 (65%)	103 of 163 (63%)
Possessions†	2·75 (1·13)	2·34 (1·25)
Household spending on food/person/day‡	2·97 (1·51)	2·70 (1·21)
Crowding§	0·72 (0·28)	0·64 (0·28)
Hygiene score¶	2·52 (0·74)	1·93 (0·91)
Birth order of child:		
First	94 (50%)	83 (44%)
Second	39 (21%)	49 (26%)
Third or over	54 (29%)	58 (31%)
Mean age (days)	0·15 (0·06)	0·15 (0·05)
Birthweight (kg)	3·41 (0·45)	3·35 (0·46)
Weight (kg)	3·24 (0·45)	3·22 (0·43)
Length (cm)	49·6 (1·91)	49·8 (2·19)
Number participating in community organisation‖	59 (32%)	62 (33%)

Data are number of people (%) or mean (SD). *Source of drinking water, storage of drinking water, sewage disposal, floor, and wall material of house. †TV, radio, refrigerator, and food blender. ‡Amount spent on food in soles (local currency) per person per day. §Number of residents in house/number of rooms excluding bathroom. ¶Based on observations of the cleanliness of the home, specifically the state of the floor and whether human faeces were visible around the house. ‖For example, a community kitchen or mothers' club.

Table 1: **Characteristics of children and their household at enrolment**

household. Food intake was converted to nutrients by use of Peruvian food-composition tables,[17] which include data for food composition from other Latin American and US tables where necessary. We also compared data for dietary intake with recommended daily intakes for complementary foods.[6,18,19] Anthropometric data were converted to *Z* scores by use of reference values.[20]

Analysis was by intention to treat, and children who moved away were analysed as part of the community in which they were enrolled unless they were lost to follow-up because they moved out of the study area completely. Outcomes reported here all pertain to the individual level rather than at the cluster level. Outcomes were predefined as: knowledge of key feeding practices and messages; actual desirable feeding practices (defined as an age-dependent composite—eg, exclusive breastfeeding for children younger than 6 months, breast milk plus two thick foods and one animal product per day for children older than 6 months); and dietary intakes from complementary foods for energy, macronutrients, micronutrients, and foods from an animal source for children older than 6 months. For morbidity outcomes: diarrhoea was defined as three or more liquid or semiliquid stools in 24 h; fever as reported by parents; and anorexia as a reduction in appetite that was a change from the child's usual state. Children's visits to the health facilities were classified as mainly illness-related or as preventive (ie, attendance at well-baby clinic, vaccination, or nutrition consultation). Assessment of growth was by comparison of final attained weight, height, and associated *Z* scores at 18 months of age.

Results were compared by use of ANOVA with transformation if needed; when not possible, nonparametric statistical tests were applied. Significance was accepted at $p<0.05$. Differences between groups were assessed after adjustment for biological (ie, birthweight and sex) and socioeconomic covariates. Random-effects models were applied to account for cluster randomisation. We did a permutation test to test the likelihood of type I error (ie, the chance of an equal or greater difference in outcome if the data had been generated by a different randomisation process)[21] by reanalysing of the outcomes of all possible randomisations by reassignment of health centres to intervention and control.

Role of the funding source

The sponsors of the study had no role in the study design, data analysis, data interpretation, or writing of the report. The corresponding author had full access to all the data and full responsibility for the decision to submit for publication.

Results

21 health facilities were identified: three community hospitals, one of which was excluded because it had just participated in a health-services intervention with a nutrition component; five health centres, one of which was excluded because it shared administration with a religious organisation; and 13 health posts, two of which were excluded because they were thought too dangerous for fieldwork and one because it was established for a displaced population and was therefore regarded as unique. Two were excluded after pairing because the required number had been reached and two more were excluded because they were directly adjacent to each other (figure 1). There was one community hospital, two health centres, and three health posts in each study group.

791 pregnant women were identified by house-to-house census in the catchment areas, 377 (48%) of whom were enrolled (figure 1). The main reasons for not being enrolled were that the needed sample size had been achieved or that the baby had been born before predicted and was outside the age criterion; 12 mothers refused to participate and two had babies with congenital malformations. During the 18 months, 75 families left the study for varying lengths of time because they moved out of the area or decided not to continue. Some of these families later rejoined the study and at 18 months all but 39 children and their caregivers were available to participate (figure 1). All clusters were included in the analysis. The health facilities had been paired with respect to the services offered. Table 1 shows the characteristics of children and families at enrolment

Children in the intervention group made significantly more preventive health-care visits than did those in the control group (9·73 visits *vs* 8·17 visits, $p<0·0001$) over 18 months. This finding was significant in the months preceding the visits when children were aged 3, 6, 12, 15, and 18 months. The number of visits related to illness and the frequency of signs of morbidity 24 h before every interview did not differ between intervention and control groups (data not shown).

Twice as many mothers in the intervention group reported receiving nutritional advice shortly after birth than did those in the control group (16 [52%] of 31 *vs* 9 [24%] of 37, $p=0·02$). These numbers increased in both study groups with the age of the child, but reports were significantly greater in the intervention group than in controls only at 4 months and 18 months ($p=0·002$). Twice as many mothers in the intervention group as in the control group who reported taking their child to a health centre had stated that they had received nutritional advice.

The results with respect to caregivers' knowledge of age-specific feeding practices are shown in table 2. In

	Intervention	Control	p (unadjusted)	p (adjusted)
What foods or liquids does a baby need for the first months?				
Birth	138 of 187 (74%)	140 of 190 (74%)	0·980	..
4 months	141 of 171 (82·5%)	130 of 170 (77%)	0·171	..
How can a mother have more breastmilk?				
Birth	16 of 187 (9%)	14 of 190 (7%)	0·670	No significant
4 months	27 of 171 (16%)	11 of 170 (7%)	0·006	covariables
What do you do if the child has colic or gases?				
Birth	17 of 187 (9%)	17 of 190 (9%)	0·961	No significant
4 months	24 of 171 (14%)	5 of 170 (3%)	0·00024	covariables
Name three foods that you think are the best for an infant 7–8 months old				
8 months	108 of 170 (64%)	81 of 169 (48%)	0·004	0·011*
12 months	113 of 170 (67%)	78 of 169 (46%)	0·00016	No significant
15 months	109 of 167 (65%)	79 of 169 (47%)	0·001	covariables
18 months	112 of 171 (66%)	91 of 167 (55%)	0·039	0·107†
When you give the main meal to a child, what is the food you give first?				
8 months	95 of 170 (56%)	50 of 169 (30%)	<0·0001	No significant
12 months	71 of 170 (42%)	45 of 169 (27%)	0·003	covariables
15 months	84 of 167 (50%)	56 of 169 (33%)	0·001	
18 months	87 of 171 (51%)	54 of 167 (32%)	0·001	0·01‡ No significant covariables
What would you do if a child does not want to eat?				
8 months	66 of 170 (39%)	33 of 169 (20%)	<0·0001	0·0001
12 months	80 of 170 (47%)	38 of 169 (23%)	<0·0001	No significant
15 months	73 of 167 (43%)	49 of 169 (29%)	0·005	Covariables
18 months	75 of 171 (44%)	45 of 167 (27%)	0·001	0.006§

Data are number (%). *=Adjusted for birthweight. †=adjusting for housing. ‡=adjusted for sex. §=adjusted for parent's education.

Table 2: **Number of carers who correctly answered questions on age-specific feeding practices**

	Intervention	Control	p (unadjusted)	p (adjusted)
Currently breast feeding*				
3 months	171 of 174 (99%)	172 of 173 (99%)	0·317	..
6 months	163 of 172 (95%)	157 of 165 (95%)	0·872	..
8 months	157 of 169 (93%)	158 of 168 (94%)	0·670	..
9 months	154 of 164 (94%)	150 of 165 (91%)	0·306	..
12 months	144 of 159 (91%)	150 of 162 (93%)	0·514	..
15 months	117 of 157 (75%)	131 of 155 (85%)	0·029	..
18 months	100 of 145 (69%)	113 of 150 (75%)	0·223	No significant covariables
Exclusively breastfeeding*				
Birth	143 of 187 (77%)	144 of 190 (76%)	0·877	..
3 months	94 of 174 (54%)	105 of 174 (60%)	0·233	..
4 months	76 of 171 (44%)	81 of 170 (48%)	0·553	..
Eating thick food first at main meal†				
6 months	48 of 157 (31%)	29 of 147 (20%)	0·03	No significant covariables
8 months	44 of 166 (21%)	34 of 163 (27%)	0·229	..
9 months	57 of 161 (35%)	27 of 158 (17%)	0·000	No significant covariables
12 months	69 of 166 (42%)	43 of 165 (26%)	0·003	..
15 months	70 of 166 (42%)	56 of 166 (34%)	0·114	..
18 months	75 of 171 (44%)	63 of 167 (38%)	0·252	..
Consuming egg, chicken liver or fish†				
6 months	111 of 171 (65%)	84 of 165 (51%)	0·009	0·009‡
8 months	104 of 170 (61%)	81 of 167 (49%)	0·019	0·027§
9 months	93 of 163 (57%)	80 of 166 (48%)	0·108	..
12 months	94 of 168 (56%)	90 of 167 (54%)	0·705	..
15 months	84 of 168 (50%)	88 of 167 (53%)	0·622	..
18 months	109 of 171 (64%)	96 of 168 (57%)	0·215	..
Breast milk+two thick preparations+animal source food				
6 months	21 of 171 (12%)	15 of 165 (9%)	0·345	No significant covariables
8 months	40 of 170 (24%)	31 of 167 (19%)	0·264	..
9 months	55 of 163 (34%)	38 of 166 (23%)	0·029	..
12 months	61 of 168 (36%)	59 of 167 (35%)	0·852	..
15 months	60 of 168 (36%)	68 of 167 (41%)	0·347	..
18 months	72 of 171 (42%)	71 of 168 (42%)	0·977	..

*Questionnaire data. †24-h dietary recall data. ‡Adjusted for mother's age. §Adjusted for birthweight.

Table 3: **Feeding practices (24-h recall)**

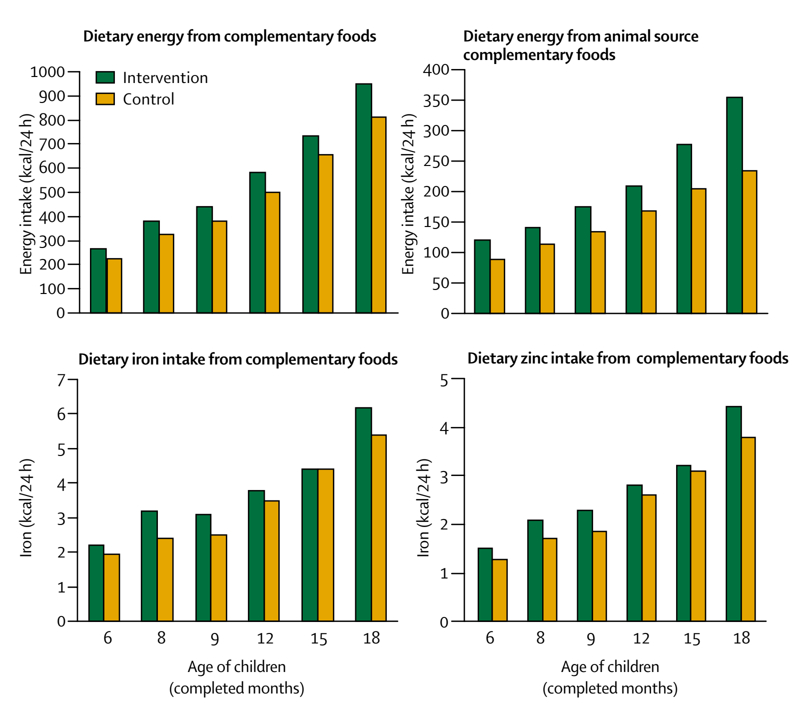

Figure 2: **Unadjusted mean energy and nutrient intakes from complementary foods (24-h recall)**

both groups, mothers knew the importance of breastfeeding from birth at time of delivery. At 4 months, when asked what could be done to produce more breastmilk, significantly more mothers in the intervention group answered more frequent suckling. At the 4-month visit, more women in the intervention group than the control group reported that if her baby had colic, she could drink the digestive teas rather than giving them directly to her baby (table 2). Moreover, more mothers in the intervention group responded correctly to questions asking them to name three important foods (ie, chicken liver, eggs, and fish) for a child aged 7–8 months than did mothers in the control group. More caregivers in the intervention group responded that they would give thick foods first at main meals than did those in the control group. More caregivers in the intervention group said they would encourage their child to eat if they had a reduced appetite than did those in the control group (table 2).

Nearly all women reported breastfeeding their baby, and there were no significant differences between groups (table 3). Using the WHO definition of the previous 24 h, 76·0% of infants in each study group were exclusively breastfeeding at enrolment. The frequency of exclusive breastfeeding dropped to 54·0% (intervention) and 60·3% (control) at 3 months, and to 44·4% and 47·6%, respectively, at 4 months (table 3). The reported number of breastfeeds in 24 h only differed between groups at 15 months, when intervention babies were breastfed slightly more often than were controls (11·7 feeds *vs* 10·0 feeds, p=0·036 after log transformation).

Complementary feeding practices differed between groups. One key message of the intervention was to offer a thick food (eg, purée or main course) to a child at the main meal before the commonly offered soup (which has low energy and nutrient densities compared with thick food). From the 24-h dietary recall, consumption of thick food first was significantly greater in the intervention group than in the control group at 6 months, 9 months, and 12 months. Another important message of the intervention was to add a source of animal protein (ie, egg, chicken liver, or fish) to the child's meals everyday. A significantly higher proportion of children in the intervention group received chicken liver, fish, or egg than did controls at age 6 months and 8 months. Enhanced intake of animal protein was caused mainly by increased frequency of chicken-liver consumption. The message to motivate caregivers to teach their children to eat with love, patience, and good humour could not be assessed by interview.

One of the main study objectives was to increase the number of children older than 6 months who received breast milk plus two thick preparations and one food product from an animal source. However, the difference between groups was significant only at 9 months (table 3).

Mean energy intake from complementary foods was significantly higher in the intervention group than in the control group at 18 months (figure 2). The differences remained significant after adjustment for household socioeconomic characteristics. For all ages, mean energy intake in intervention and control groups exceeded the recommended intakes for complementary foods, assuming average breastmilk intake. However, more children in the control group did not meet 80% of recommended energy intake per day compared with those in the intervention group at 8 months (45 [27%] of 167 *vs* 30 [18%] of 170, p=0·040), 12 months (82 [49%] of 167 *vs* 64 [38%] of 168, p=0·043), and at 15 months (51 [31%] of 167 *vs* 35 [21%] of 168, p=0·042). Adjustment for baseline characteristics eliminated the significance of the difference at 8 months (p=0·176), 12 months (p=0·176 after adjustment for housing), and at 15 months (p=0·090 after adjusting for parental age).

Energy intake from animal sources was used as an indicator of food intake from an animal source and dietary quality. Children assigned to the intervention group had significantly higher intakes of energy from animal sources than did those assigned to the control groups at age 15 months and 18 months (figure 2). Adjustment for household characteristics in the multivariate model reduced significance at 15 months to p=0·082, but the difference in the adjusted means remained significant at 18 months (p=0·001). The increase in energy from animal sources was the result of an increase in the use of animal-source foods rather than an increase in the overall volume of food given.

Mean total iron intake from complementary food was well below recommended intakes for every age group, but was higher in the intervention than in the control group. Unadjusted differences were significant at 8, 9, and 18 months (figure 2), but the significance was no longer apparent after adjustment for household socioeconomic variables (p=0·31 at 8 months, 0·15 at 9 months, and 0·10 at 18 months). More than 90% of all children younger than 12 months did not have 80% of daily-recommended intakes for iron, diminishing to about 40% of children at 18 months old. Differences between intervention group and control group were significant at 8 months (155 [91%] of 170 *vs* 161 [96%] of 168, p=0·047) and at 9 months (152 [93%] of 163 *vs* 165 [99%] of 167, p=0·003 and at 18 months (63 [37%] of 171 *vs* 80 [48%] of 168, p=0·045).

Iron intake from good sources of haem (ie, meat, poultry, and fish), although low, were non-significantly higher in the intervention group than in controls at 6 months (mean 0·75 mg per day [SD 0·88] *vs* 0·64 mg per day [0·97]; p=0·275 [ANOVA]) and at 8 months (0·82 [1·36] mg per day *vs* 0·66 mg per day [1·18]; p=0·257 [ANOVA]), when adjusted for socioeconomic variables. However, unadjusted values by use of Mann-Whitney test showed significant differences. The nutrient densities for iron and zinc were consistently lower than recommended values[6] and there were no differences in nutrient density for iron or zinc at any age (data not shown).

Mean zinc intake was also lower in participants than that recommended, but was significantly higher in the intervention group at 9 months than in the control group (2·33 mg per day [SD 1·89] *vs* 1·85 mg per day [1·60], p=0·014, figure 2). This difference remained significant when mean intakes were adjusted for parents' education levels in the model (p=0·035). The proportion of children not meeting 80% of recommended intakes was significantly lower in the intervention group than in the control group at 9 months (125 [77%] of 163 *vs* 145 [87%] of 167, p=0·012). For other micronutrients, calcium intake from complementary foods was also significantly higher at 18 months in the intervention group than in controls (adjusted mean 526 mg per day *vs* 393 mg per day, p=0·007 after adjustment for parental age).

Growth was similar to that described for the Peruvian population[2,13]—ie, parallel to or above the reference[18] for the first 4–6 months followed by a reduction in mean length and weight increments. Figure 3 shows unadjusted, main-effect growth outcomes over 18 months. Linear growth was much the same in both intervention and control children for the first 6 months, followed by a divergence between groups and less stunting in the intervention group. The intervention reduced stunting in linear growth (figure 4 and table 4). Linear-growth velocity, measured as the group mean gradient of the regression line of the *Z* scores for length-for-age×age for each individual, was significantly greater in the intervention group than in controls, even after adjustment for socioeconomic status, hygiene score, and birthweight variables and after application of the random-effects model in recognition of the cluster design.

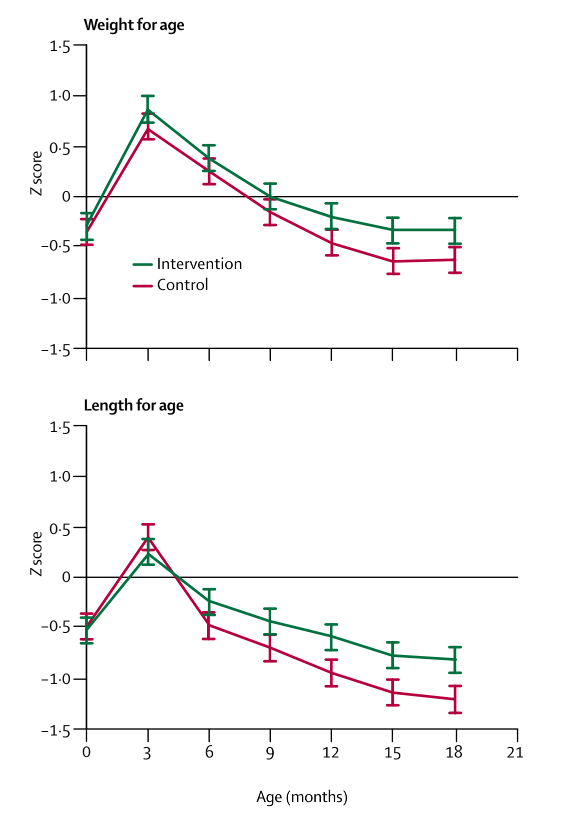

***Figure 3:* Changes in weight-for-age and length-for-age *Z* scores in children in the cohort**
Error bars=SE.

At 18 months, children in the intervention group were 1 cm taller (p=0·0003) and three-times less likely (p=0·018) to be stunted than were children in the control

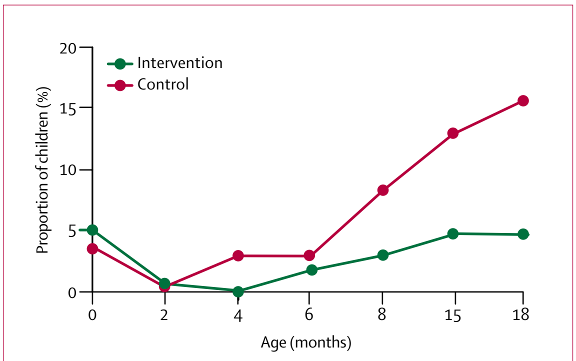

***Figure 4:* Cumulative rate of stunting from 0 to 18 months**

	Intervention	Control	Undajusted difference	p	Adjusted difference	p
Mean weight at 18 months (kg)	10·77 (1·16)	10·48 (1·02	0·295 (0·061 to 0·529)	0·014	0·199 (–0·033 to 0·431)	0·093
Mean length at 18 months (cm)	79·36 (2·74)	78·29 (2·66)	1·068 (0·488 to 1·648)	<0·0003	0·714 (0·146 to 1·282)	0·014
Mean z-score weight for age at 18 months	–0·33 (0·90)	–0·62 (0·83)	0·285 (0·099 to 0·471)	0·003	0·194 (0·008 to 0·38)	0·041
Mean z-score length for age at 18 months	–0·81 (0·80)	–1·19 (0·83)	0·386 (0·209 to 0·562)	<0·0001	0·272 (0·099 to 0·445)	0·002
Mean z-score weight for length at 18 months	0·15 (0·87)	0·05 (0·79)	0·091 (–0·089 to 0·271)	0·319	0·048 (–0·139 to 0·237)	0·609
Linear growth velocity*	–0·031 (0·043)	–0·057 (0·05)	0·026 (0·016 to 0·036)	<0·0001	0·021 (0·012 to 0·031)	<0·0001
Number of children with stunting at 18 months	8 of 171 (4·7%)	26 of 165 (15·8%)	3·811 (1·672 to 8·689)†	0·001	3·035 (1·207 to 7·636)†	0·018

Data are mean (SD) or mean (95% CI). *Mean of the regression gradient for length for age *vs* age. †Odds ratio (95% CI).

Table 4: **Child growth at 18 months by intervention group**

group, even after correcting for the effect of birthweight (figure 4). Mean weight gains also differed between groups: children in the intervention group at 18 months weighed 295 g more than did controls (p=0·014) before adjustment and 199 g more (p=0·093) after adjustment for birthweight and socioeconomic factors.

Discussion

We have shown improved knowledge, preventive health-care-seeking behaviour, feeding practices, dietary intake, ponderal growth, and linear growth in a cohort of children living in the catchment area of health facilities that participated in an educational intervention to improve nutrition in young children. The intervention reduced the rate of stunting by more than two thirds. These results add to evidence that nutrition education without the provision of food supplements can improve the dietary intake of young children and improve growth.

Several randomised control trials of interventions that have included nutrition education combined with other strategies found improvements in growth and prevention of malnutrition, but the effect of the nutrition-education component cannot be distinguished.[22–24] A non-randomised community-based education programme in China[10] increased the frequency of consumption of foods from an animal source and improved infant growth. A cluster-randomised trial in India[11] reported significantly increased exclusive-breastfeeding rates, a low frequency of diarrhoea, and a growth benefit in boys but not girls compared with a control group.[25] The Indian study had a similar design to our study, in that the intervention was developed after formative research and implemented through existing services. The positive effects recorded in two different circumstances suggest that this intervention might be an effective programme of nutrition education, which could have positive benefits that are measurable in the population served by the targeted health programme.

The strengths of this study are the randomised design, comparison of six intervention health facilities with six control facilities, and that the number of children enrolled exceeded that estimated to ensure sufficient power to test the hypotheses. Refusal to participate and loss to follow-up were low. Randomisation ensured that contiguous communities were not assigned to different study groups, thus reducing intervention contamination. Moreover, the assessment team was completely independent of the intervention. The intervention communities had a further round of surveys in the midterm as part of the process assessment, but care was taken to avoid giving any nutrition or health advice during the interviews and only nine children in the midterm survey were also part of the cohort.

There were some inevitable weaknesses in the study design. The study could not be blinded, which could have led to bias. However, data collection was standardised, interviews were structured, and interviewers rotated between intervention and control areas to limit any bias that might result from the same team always interviewing intervention or control families. Nevertheless, knowledge of the group could have influenced data collectors' interpretation of responses or the recording of dietary-recall data, but this knowledge is unlikely to have affected weight or height measurements.

A further limitation was the socioeconomic differences between families in the intervention and control groups. We had found that some socioeconomic characteristics—housing score, educational level of parents, hygiene score, and birthweight—affected variation in growth outcomes in the population. This information allowed us to develop a predefined analysis plan using the appropriate variables in the multivariate analysis of differences in the growth of the cohort. In the final model, differences in growth between the groups remained significant after adjustment. After adjustment in the multivariate analysis, differences between groups for dietary intake remained significant, providing clear evidence of the effect of the intervention on dietary intakes.

The magnitude of the difference in growth between the groups is greater than might be expected from the significant, but modest, differences in feeding practices and dietary intakes. Emphasis on preventive nutrition (which led to differences in the groups at an early age before adverse nutritional effects occurred) might have been crucial, or a small but sustained difference in nutrition throughout early life could have resulted in fairly large effects in the prevention of growth retardation. Errors in the estimation of outcomes such as behaviours and dietary intake make the detection of intervention effects more problematic than for outcomes such as nutritional status or growth.

Issues of sustainability and generalisability need to be addressed in all public-health interventions. Our study took place in a periurban population in a coastal Peruvian city. The population has ready access to a generally well-organised and professional health service—a likely prerequisite for an educational intervention. Furthermore, food availability alone is not a limiting factor because almost all families are able to meet recommended intakes for dietary energy.[15] However, cultural preferences favour foods with a low-energy density such as soups and broths,[6] which reduce the total energy intake of infants.[16] Foods from animal sources that are high in bioavailable micronutrients are fairly expensive, but sources such as those selected for promotion in this intervention (ie, chicken liver, eggs, and fish) are affordable. In some areas, including the Peruvian highlands, food availability may be limited and education alone might have no effect if not complemented by food assistance. However, even if limited to similar periurban populations described here, this intervention could benefit large populations because the greatest number of poor people live in these urban areas in Peru.[12] Similar situations are found in other cities in Latin America and other developing countries.

Our study selected government health services as the most widely used and sustainable vehicle for nutrition education because even in a context of changeable government policies and international donor priorities, local government health services continue to provide health care (including immunisations and monitoring of growth and development) to poor areas. Despite the importance and potential of interventions through the health services, few randomised controlled trials of educational interventions in developing countries have been done. The Baby Friendly Hospital Initiative increased exclusive breastfeeding in Belarus,[24] and the Integrated Management of Childhood Illnesses (IMCI)[26] is another approach that uses health services to improve nutrition. A randomised trial[27] of the nutrition component of IMCI in Brazil showed limited benefits: the intervention improved weight gain in children older than 12 months whose caregivers had been counselled by trained doctors, but there was no effect on the linear growth of children younger than 12 months. Because assessment was limited to the children of caregivers who had received counselling from trained doctors, the extent of benefit in the overall population is unknown.[27] The intervention in the Brazilian study focused on training medical staff; by contrast, our intervention looked at the service offered by the health facility overall and the effect was measured in the whole infant population served by the facility.

We were careful to limit the intervention to sustainable enhancement of existing services. No new personnel were added, and material benefits given to the centres were kept to a minimum, mainly in the form of educational materials. The programme aimed to improve services rather than train individuals who may move on and be replaced. However, participation of the health facility in a project probably generated some enthusiasm, and even if the project did not depend on investing in individuals, the role of highly motivated individuals is difficult to avoid.

Malnutrition is a major cause of child morbidity and mortality, and effective interventions are urgently needed to prevent growth faltering in young children. Our educational intervention prevented stunting, a form of chronic malnutrition that occurs in more than 15% of infants in this population. Research is needed to determine the sustainability of the intervention in Trujillo, and the generalisability of the intervention strategy to similar settings in Peru and elsewhere.

Contributors

M E Penny and H M Creed-Kanashiro conceived and participated in the project, interpretated the data, and the wrote the article. L E Caulfield and R E Black advised on study design and implementation, and participated in the interpretation of the data and the writing of the article. M R Narro and R C Robert participated in the design of the intervention, assessment, and revision of the article.

Conflict of interest statement

We declare that we have no conflict of interest.

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