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2 Gilbert RE, Fleming PJ, Azaz Y, Rudd PT. Signs of illness preceding sudden unexpected deaths in infants. *BMJ* 1990;300:1237–9.
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Drs Taylor and Emery comment:
The main differences between the Bristol and Sheffield views on unexpected infant death are firstly, that the Bristol group do not accept the concept that several contributory factors may result in a lethal situation, and secondly that they are unable to accept that filicide is the probable cause of a small percentage of cot deaths.

Our view is that many unexpected infant deaths are due to a combination of factors in the sense that but for the presence of all of these factors death might not have occurred.[1] The concept of contributory factors is one which is generally accepted—for example, the increased severity of infections in children who also have nutritional deficiencies or reduced pulmonary reserve due to residual lesions in the lung after perinatal damage. Dr Fleming and his group have themselves postulated that overheating and overwrapping may be a contributory factor in some cot deaths.[2]

The confidential inquiries undertaken by Dr Fleming and his group with case conferences being hospital based and hospital orientated are very similar to those undertaken in Sheffield in the 1970s, and their results are very similar to our own at that time.

In 1979 we started to hold our case discussions on all 'non-hospital' deaths in the family doctor's surgery. These case discussions are held when data collection, including a home visit by a doctor not concerned with the paediatric care of the family, is complete and the results of a full paediatric necropsy are available and at a time when the initial emotional impact of the death is lessening and more dispassionate views may prevail. These discussions, at which the general practitioner and health visitor are invariably present, are as much concerned with planning for the future health care of the family and of any subsequent child as with the cause of the baby's death. It is these confidential discussions with the general practitioner and health visitor that have revolutionised our views on the aetiology of unexpected infant death and have shown us the importance of family and background factors, and it is at these discussions that the possibility of filicide is occasionally raised. Deaths are only placed in this category if at the conclusion of the case discussion it is a unanimous decision that this was the *most probable* mechanism of death. The prime reason for identifying this small group (less than 10%) is so that an appropriate level of care—paediatric, psychiatric, and health visiting—can be provided for the family. Although the identification of possible filicide only forms a relatively small part of our inquiries, the denial of the possibility of this mechanism of death would also deny some families the extra care that they need.

Fleming *et al* state that our data should be 'inspected by an independent group of paediatricians and pathologists'. As we collect more than 50 pages of data after each death this would be a major undertaking. However, we have no secrets other than the confidentiality of information relating to individual families and would welcome a visit from Dr Fleming or one of his colleagues to look at our records, discuss our methods, and attend a case discussion.

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Effective bronchodilator treatment by a simple spacer device for wheezy premature infants

SIR,—Yuksel and colleagues describe a cheap and easy solution to a common and difficult problem.[1] One cannot help but be impressed by the size of the improvement seen during active treatment.

There are, however, certain aspects of the study which concern me. Firstly, the study was not randomised and all the children were given active treatment after placebo. Although this is disputed by the authors, in this group of children where symptoms are likely to have been exacerbated by viral infections, one would have expected some improvement with time, irrespective of treatment. This possibility is borne out to some extent, as on active treatment there was a 27% reduction in the score for 'runny nose/unwell'; these are symptoms which are suggestive of a viral infection. These symptoms could not have been expected to respond to a bronchodilator and therefore should not have been added to cough and wheeze (their table 1). Also adding together scores for cough and wheeze (their table 1) could double the score for a single event. Admittedly the percentage fall in each symptom category, with active treatment, is remarkable.

Secondly, the authors state 'Administration of the bronchodilator resulted in an increase in functional residual capacity both immediately and during the two week active period'. This is patently not borne out by the results in their table 3, where there is no significant difference in functional residual capacity before and after terbutaline using a paired *t* test (mean (SD) before and after terbutaline: 252 (75) and 269 (58) in the placebo period and 322 (83) and 325 (95) in the active period). Indeed two and three subjects, respectively, showed a 'clinically significant' decrease in functional residual capacity after terbutaline compared with four and two with a 'clinically significant' increase. If we are to believe, as the authors suggest, that an increase in functional residual capacity reflects an improvement in lung function, then the long term improvement in functional residual capacity on active treatment occurred without any evidence of an immediate response. The authors do not address this discrepancy between the long and the short term benefit but misleadingly suggest that an immediate increase in functional residual capacity had occurred.

One is left with a sneaking suspicion that at least part of the improvement on active treatment, in symptoms and lung function, could be related to changes with time. Before accepting the alternative conclusion, that in this group of children twice daily terbutaline has a dramatic effect on symptoms, which is largely independent of its immediate bronchodilator properties, it would be wise to confirm the results with a better designed study.

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1 Yuksel B, Greenough A, Maconochie I. Effective bronchodilator treatment by a simple spacer device for wheezy premature infants. *Arch Dis Child* 1990;65:782–5.

Drs Greenough and Yuksel comment:
We thank Dr Wilson for her interest in our paper. In that preliminary study, as no previous data was available, we set out to assess if bronchodilator given by a simple spacer had any effect on symptomatic preterm infants in the first year of life. We found over the two week period in which bronchodilator was administered a significant improvement in lung function and reduction in symptom score.

We are surprised Dr Wilson felt it necessary to reanalyse the acute lung function data as we did *not* claim that the change in functional residual capacity 10 minutes after terbutaline was significant. We recognised and stressed in our discussion that the results could in part be explained by an improvement in symptoms with time and felt our results should be confirmed in a randomised trial—which, as a consequence we have subsequently completed (unpublished observations). In a randomised placebo controlled trial inhaled ipratropium bromide administered via a coffee cup, resulted in similar improvements in functional residual capacity and a reduction in symptom score. Unfortunately many infants are symptomatic in the first year of life, the coffee cup technique is cheap and easy; we therefore, like Dr Wilson, are impressed and we are also pleased with the size of improvement seen during active treatment.[1]

Gender and the progression of *Escherichia coli* 0157:H7 enteritis to haemolytic uraemic syndrome

SIR,—The epidemiologic studies of Milford *et al* continue to advance our understanding of Verocytotoxin producing *Escherichia coli* and the associated haemolytic uraemic syndrome.[1] Notable among their findings is the excess of females in the probable prototypic haemolytic uraemic syndrome subgroup and the observation that such excess has been observed previously in other studies. Our own studies have also previously suggested an excess of females among patients with *E coli* 0157:H7 associated haemolytic uraemic syndrome.[2] Although in a univariate analysis this excess was not significantly different from a control group of patients who suffered from *E coli* 0157:H7 enteritis only, a multivariate analysis disclosed that female gender was possibly an important predictor variable for progression to haemolytic uraemic syndrome.

In order to determine whether the latter association was coincidental, we continued our study to include 37 patients with haemolytic uraemic syndrome and 95 patients with only enteritis. The sex ratios (M/F) in the two groups were 17/20 and 53/42 respectively. In univariate analysis, gender was not associated

with progression to haemolytic uraemic syndrome (p=0·414), and gender was not a significant predictor variable in multivariate analysis. Our studies do not, however, include an analysis of the effect of gender upon the severity of haemolytic uraemic syndrome.

Although the potential for young age and prolonged use of antimotility agents to enhance the progression of *E coli* 0157:H7 enteritis to haemolytic uraemic syndrome might be explained on the basis of a toxin per body weight relationship, the designation of female gender as a risk factor might lead to alternate views on disease pathogenesis—for example, a gender difference for toxin receptors. Therefore, future studies should critically evaluate gender in the context of appropriate control groups.

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1 Milford DV, Taylor CM, Guttridge B, Hall SM, Rowe B, Kleanthous H. Haemolytic uraemic syndromes in the British Isles 1985–8: association with Verocytotoxin producing Escherichia coli. Part 1: clinical and epidemiological aspects. *Arch Dis Child* 1990;65:716–21.
2 Cimolai N, Carter JE, Morrison BJ, Anderson JD, Risk factors for the progression of Escherichia coli 0157:H7 enteritis to hemolytic-uremic syndrome. *J Pediatr* 1990;116:589–92.

Ready, steady, hiss

SIR,—Working in a Cambodian refugee camp reminded me how important both definitions and cultural influences are in timing developmental landmarks. In Britain control of micturition implies that the child recognises his full bladder and communicates this to the carer, a process attained by the majority at 1·5 years.

In the camp where there are no nappies and infancy is spent attached by a large sheet to the mother's side, however, mothers have developed a technique for toileting their infants at an earlier age that prevents themselves being wet all day, and may have unknowingly shone new light on the mechanisms of toileting. When she suspects that her infant's bladder is full, the mother hisses in his ear as she holds him in a squat position over the earthen floor. By between 6 and 12 months of age this hiss initiates prompt bladder emptying.

Little is known of the psychological basis for toileting, but if these observations are correct, then at least here classical (pavlovian) conditioning may have a role, in which the conditioned stimulus is the hiss, the unconditioned stimulus is the full bladder, and the response is the bladder emptying. At first sight this technique is reminiscent of the toileting history of B F Skinner,[1] the great proponent of operant conditioning in behaviour theory, who was successfully toileted by 9 months on a potty chair that played 'The Blue Danube' when she produced! Such music may be considered pleasant to the infant and, therefore, supportive of operant conditioning because it provides an incentive to urinate appropriately. The case for my description being of classical conditioning rests on the hiss of the mother being a neutral stimulus that is neither an incentive nor a deterrent to the infant in urinating (as neutral as the bells were to Pavlov's dogs).

I see no reason why this technique should not be studied further, and if found to be effective, may greatly help mothers involved in the exhaustion of nappy changing, regardless of which school of psychology this phenomenon supports.

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1 Skinner BF *The shaping of a behaviourist*. New York: Knopf, 1979.

Terminology in community child health

SIR,—Dr Stone's annotation on terminology in community child health was a useful airing of an important issue.[1] By and large, the definitions he proposes are clear and acceptable.

I have two points to make. Firstly, the definition of surveillance he proposed concerns a limited and a relatively small proportion of the work currently carried out by child health doctors and general practitioners in child health clinics. In many ways, restricting the definition of surveillance to cover the 'collection . . . and interpretation of indices of child health, growth, and development' is useful as it is near to the accepted lay definition of surveillance. However it should be made clear that this is different to the activity known as child health surveillance as described in the Court report[2] and more recently in the Hall report.[3] The latter consists of a more wide ranging activity, the emphasis of which is on the promotion of health and development with the implied active participation of parents in the process. The inappropriateness of the word 'surveillance', in terms of its connotations, for this activity were commented on in the Hall report but no suitable alternative was suggested.

The second point concerns Stone's patronising view of community paediatrics as a rather cosy, anachronistic, and insular specialty which, he states, feels uncomfortable at being neither in the main stream of hospital paediatrics or public health. I would refute this suggestion strongly. The Court report recommended a consultant led community child health service, of the sort that is currently emerging, as the best hope for improving standards of child health to all sections of the population. The working practices in community child health that have evolved since then are positive, effective, and in many cases, novel approaches to the problems of improving and integrating health services for children. That controversy exists, even surrounding terminology, is a measure of the innovatory nature of much of community paediatrics. Rather than seeing community child health 'trapped in a professional and organisational vacuum' I would see it as a model for other health services such as geriatrics, psychiatry, and even perhaps obstetrics, which still remain hospital and medically orientated despite the problems often being community related.

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1 Stone D. Terminology in community child health—an urgent need for consensus. *Arch Dis Child* 1990;65:817–8.
2 Report of the Committee on Child Health Services. *Fit for the future*. London: HMSO, 1976. (Court report.)
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Use of peripheral vessels for exchange transfusion

SIR,—I would like to congratulate Fok *et al* for reminding us again in a documented way that there is no need for exchange transfusions to be carried out 'the conventional way'.[1]

For the past six to seven years we have also used peripheral vessels for exchange transfusions as the preferred route particularly in ill preterm infants, and I would entirely concur with the authors' comments and conclusions. I would like to recommend a modification of their technique, however, which requires only one operator/observer, an important advantage in a busy neonatal unit. Two syringe pumps are required. The first is used to deliver blood through the peripheral vein. The second is modified and its action is reversed (that is, it pulls the syringe plunger) and is used to withdraw blood through the peripheral artery at the same rate chosen for the former. If such a modification is impossible then the operator withdraws blood through the artery at precisely the rate of the syringe pump. By using two syringe pumps, however, the movement of the plunger is more even and smoother and the likelihood of extravasation of blood and arterial spasm is minimised. There is only one potential hazard. The temptation to move away from the baby and let 'the system run on its own.'

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1 Fok TF, So LY, Leung KW, Wong W, Feng CS, Tsang SS. Use of peripheral vessels for exchange transfusion. *Arch Dis Child* 1990;65:676–8.

Reflux vomiting

SIR,—In his comprehensive review of reflux vomiting Dr P J Milla suggests that the prone head raised position is the most effective therapeutic position and cites references to support this.[1] Yet in the first direct comparison of the prone flat and prone head raised (30 degrees) positions it has been shown that the flat position is just as effective.[2] This is an important practical observation. In the past, by a variety of methods, much effort has been spent maintaining infants in the raised position which we now know to be unnecessary. This is good news for both nurses and parents!

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1 Milla PJ. Reflux vomiting. *Arch Dis Child* 1990;65:996–9.
2 Orenstein SR. Prone positioning in infant gastro-oesophageal reflux: is elevation of the head worth the trouble? *J Pediatr* 1990;117:184–7.