referred to our thyroid and endocrine clinics, and indeed our experience would support Dr Cooke's "suspicion that they represent the tip of an iceberg."

It has been rightly emphasised that primary thyroidal failure is a graded phenomenon,⁵ and in contrast to the frank clinical picture of hypothyroidism in Dr Cooke's patients we have seen several patients who presented with mild and non-specific symptoms following an uneventful pregnancy. Such patients are often reassured their tiredness is due to the "stress and strain" of motherhood or they are prescribed iron treatment despite the absence of any evidence of such deficiency. The subsequent improvement observed tends to perpetuate this practice, although the clinical course merely reflects the transient nature of the thyroid disturbance. The underlying thyroid dysfunction can be easily overlooked in the absence of the characteristic symptoms and signs. We have encountered patients subsequently proved to have primary subclinical hypothyroidism in whom the sole presentation was that of persistent inappropriate galactorrhoea 9-12 months after a normal delivery.⁶ In three of our patients the primary subclinical hypothyroidism was transient and two became pregnant within 3-6 months of achieving spontaneously the euthyroid state.

It would be ideal to screen all patients six months after confinement as advocated by Dr Cooke, but such a scheme is beyond the clinical and laboratory resources of the NHS. We agree with the guidance given in your leading article that a selective group of patients should be carefully watched, and in our experience these include women known to have circulating thyroid antibodies, a family history of autoimmune thyroid disease, or an increase in size of the thyroid gland following pregnancy.

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¹ Ross, F, and Nusynowitz, M L, *Journal of Clinical Endocrinology*, 1968, **28**, 591.

² Bayliss, P F C, and Van't Hoff, W, *Lancet*, 1969, **2**, 1399.

³ Kinch, R A H, Plunkett, E R, and Devlin, M C, *American Journal of Obstetrics and Gynecology*, 1969, **105**, 766.

⁴ Kleinberg, D L, Noel, G L, and Frantz, A G, *New England Journal of Medicine*, 1977, **296**, 589.

⁵ Evered, D C, *et al*, *British Medical Journal*, 1973, **1**, 657.

⁶ How, J, and Bewsher, P D, *European Journal of Obstetrics, Gynecology and Reproductive Biology*. In press.

Review of patients after ¹³¹I treatment for thyrotoxicosis

SIR,—We were interested to see the report from Dr A D Toft and others (21 October, p 1115). Studies which will promote the development of appropriate long-term surveillance programmes for patients treated with radioiodine should be encouraged, but in view of the data which the authors present we feel that their conclusion (namely, that the optimum follow-up interval for post-radioiodine patients with normal serum thyrotrophin (TSH) levels is three years) cannot be accepted as a practicable working guideline. Their sample of patients is the end result of a considerable number of selection procedures; in particular the post-treatment interval of 6 to 18 years indicates that these patients are survivors of many events, including hypothyroidism. It is not indicated how the

post-treatment intervals are distributed in the geographical subsamples. It is probably not justifiable to extrapolate these results to the management of patients in the first six years after treatment. Losses exceeding 11% in the four-year study is an important feature worth noting when interpreting the results.

The finding in this study given most prominence is the occurrence of overt hypothyroidism in only one patient, in the group with normal initial serum TSH levels, after four years' follow-up. It is difficult to draw any serious conclusions about one event of this kind occurring in a relatively small sample of 31 patients. However, taking this figure in isolation we might conclude that the expected incidence of overt hypothyroidism at four years is 3.2%, with 95% confidence limits of 0.08% to 16.7%. The presence of a raised TSH level in only one of the seven lost patients would, of course, make a big difference to the outcome of the study.

What is probably much more important is the development of a raised TSH level in 19 out of the original 61 patients, in which state the risk of overt hypothyroidism is shown to be high. We have applied a follow-up life-table analysis to their data to show that the five-year cumulative incidence of hypothyroidism in the presence of a raised serum TSH concentration is 20.7±5.2 (SE)% and that the cumulative incidence of a raised TSH level in those with an initial normal level is 35.9%±6.5 (full details of the calculations on application). It is not possible to quantify precisely the risk of hypothyroidism for this latter group, because we do not know how long the TSH levels had been raised in those who became hypothyroid in the first group. However, it is clear that an arbitrary three-year follow-up interval may not offer sufficient protection to many patients who have a normal TSH level at a particular point in time. Plans for the long-term surveillance of patients with thyroid disease must be evaluated carefully and shown to be cost-effective, but solutions to the problem are unlikely to be found in small ad hoc studies from which large numbers of patients are lost.

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Incidence of congenital rubella

SIR,—In the parliamentary news section of your issue of 18 November (p 1441) you refer to the reply by the Secretary of State to a question on congenital rubella put by Mr Lewis Carter-Jones. In your report you include the statement that "the average annual number of children affected by congenital rubella is 400" and from the text it might be supposed that this was a correct figure accepted by the Secretary of State.

The relevant paragraph of the reply given by the Secretary of State runs as follows:

"The report which I received from the Children's Committee in September suggested that the number of infants born this coming winter with congenital rubella could, as a result of the recent steep increase in the incidence of the disease, possibly rise to 1500 to 2000. This figure was based on the assumption that the average annual number of children affected by congenital rubella was 400; while there is no firm evidence on which to base statistics, a surveillance scheme which has been in progress since 1971 suggests that that figure is

probably twice as high as the true figure even without allowing for the effect of pregnancy terminations." (*Hansard*, vol 957, col 110-2)

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*.*We regret that this reply was condensed for publication in such a way as to give a wrong impression.—Ed, *BMJ*.

Distalgesic and paracetamol poisoning

SIR,—We cannot accept the views of Drs B A Gennery and R A Lucas (28 October, p 1226) regarding the apparent safety of dextropropoxyphene and their denial of its being the frequent cause of sudden death. Their opinion was based on hospital experience in a small area of Manchester and all overdose cases reported to the coroner in that city over one year (approximate population $\frac{1}{2}$ million). Dextropropoxyphene is a widely used and effective analgesic drug, most commonly prescribed in combination with paracetamol as Distalgesic (paracetamol 325 mg and dextropropoxyphene 32.5 mg).

In the Midlands (estimated population 5 million) quite a different picture appears. The Home Office Forensic Science Laboratory in Birmingham does many of the analyses in the area in cases of possible overdose. In the last 20 months there were 34 cases in which dextropropoxyphene had been the principal cause of death and only three cases were attributed to paracetamol alone. These were all sudden deaths occurring outside hospital. All these persons except one had taken dextropropoxyphene in the form of Distalgesic. Analysis was carried out by quantitative ultraviolet spectroscopy, quantitative thin-layer chromatography, and latterly by high-performance liquid chromatography on samples from stomach contents, blood, and liver. The three methods corresponded and therefore confirmed their accuracy. About half these persons had also taken alcohol.

The findings from the Midlands agreed with those from Belfast,¹ where 30 sudden deaths due to dextropropoxyphene abuse occurred in three years. The possibility of Distalgesic causing sudden death by respiratory depression is well known to coroners, forensic scientists, and pathologists, who recognise the characteristic cyanosis and acute pulmonary congestion at necropsy. Death is rapid and usually occurs before medical treatment is available. The Forensic Science Laboratory finds that dextropropoxyphene is the commonest individual drug to cause death. It is not to be confused with paracetamol, which, in contrast, usually causes death from hepatorenal failure after several days. The contradictory reports from Manchester could be explained by different local prescribing habits, but close study of a report² from a Manchester hospital does not confirm this. Dextropropoxyphene deaths usually occur outside hospital and would not be included in any hospital figures. Analysis too can be confusing, since paracetamol, which is easily detected, may mask the presence of dextropropoxyphene, so that unless special methods of separation are employed the presence of significant quantities of dextropropoxyphene may be overlooked.

The tragedy is that persons, particularly

young people, may take a small number of Distalgesc tablets, possibly as few as 10, with rapidly fatal results. These tablets are now widely available at home and may be consumed by persons to whom they were not originally prescribed. Frequently the overdose is taken impulsively, sometimes with alcohol and possibly without serious suicidal intent. If these people took a similar number of aspirin or paracetamol tablets they would not be harmed or would, at the worst, be responsive to treatment.

The toxic effects of dextropropoxyphene should not be confused with those of paracetamol, with which it is usually combined. Furthermore, the relatively small safety margin of dextropropoxyphene, which is chemically related to morphine, should be more widely recognised.

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¹ Carson, D J L, and Carsen, E D, *British Medical Journal*, 1976, **2**, 105.

² Sivner, A L, and Goldberg, L A, *Pharmaceutical Journal*, 1978, **221**, 206.

Extending the role of the clinical nurse

SIR,—Your leading article on this subject (11 November, p 1320) ignored one factor. If nurses undertake tasks traditionally performed by doctors who does the nursing? I raised this question with a community nurse who was telling me how nursing had "advanced" (her word). In the practice to which she was attached nurses took blood and cervical smears, recorded electrocardiograms, and often did house calls to assess the need for a doctor's visit. Apparently the days of trained nurses doing blanket baths are long gone. Nursing auxiliaries—"bath ladies"—do them. But would a bath lady notice a full bladder or remove a hot water bottle from an ischaemic foot?

The attachment of community nurses to group practices can result in improved care for the sick at home, and this should surely be its objective rather than the reduction of the doctors' work load. Nurses should beware of being conned into believing that to plug a machine into a wall and record an ECG is somehow superior to cleaning a dirty mouth or preventing a pressure sore.

Before basic nursing care is further devalued, perhaps someone will do a study comparing the relative skills of doctors and nurses in administering enemas.

M F L WRIGHT

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SIR,—It may not have been the intention of your leading article (11 November, p 1320) to imply that nurses are inevitably less skilled than doctors, but this is the suggestion that emerges. Yet it is everyday experience that anxious patients make confident and warm contact with nurses, while doctors may be viewed as remote or even threatening. You are correct to emphasise the central importance a patient places on a relationship of trust but incorrect in the implication that this is a medical prerogative.

In the field of mental health nurses have been encouraged to utilise their advantages of easy and frequent contact with patients to

further treatment and rehabilitation. Some nurses have taken advantage of particular learning opportunities to demonstrate ability in psychotherapy¹ and behaviour therapy² which matches that of more prestigious professionals. This should be more widely acknowledged and pursued, not least by nursing authorities themselves.

I was saddened, in a recent visit to south-east Asia, to see the great emphasis developing countries still place on the role of the all-too-scarce doctor while failing to utilise at all fully the available skills of paramedicals and other care-givers. But in Britain, too, the psychotherapeutic needs of patients in hospital and in general practice often go unmet despite the availability of nurses, health visitors, and social workers. It is true that further training by means of seminars, supervised experience, and consultation may often be necessary before the potential of such colleagues is realisable. But perhaps first of all doctors need to acknowledge more directly and humbly the resources of care and skill at their elbows.

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¹ Morrice, J K W, *Nursing Times*, 1975, **71**, 1634.

² Marks, I M, et al, *British Medical Journal*, 1973, **3**, 156.

Terminal symptoms in children dying suddenly and unexpectedly

SIR,—We write to congratulate Dr A N Stanton and his colleagues on their preliminary report (4 November, p 1249) about terminal symptoms in children dying suddenly and unexpectedly at home. In trying to prevent any of these deaths there are at least four separate aspects worth considering: the appearance of symptoms in the child; the recognition of illness by the parents; concern about the illness by the parents or their doctor; and initiation of action by the parents or doctor.

In about 40% of the 145 children described in the preliminary report who died unexpectedly at home the parents had recognised no symptoms of illness before death. At present it is not possible to suggest any specific measure to prevent such deaths, and this is perhaps the hard core of the problem which led to the Foundation for the Study of Infant Deaths being set up in 1970.

Of the remaining 85 children, 69 are reported to have had "major" symptoms during the 48 hours before death, yet four out of five of these children were not seen by a doctor in the last 24 hours of life. We agree with the authors that "a large proportion of deaths might be prevented now if existing knowledge were better applied" and "if doctors and parents were more aware of the importance of non-specific symptoms as markers of life-threatening illness."

This foundation has frequently been approached by families in which a baby has died suddenly and unexpectedly for guidance on how to identify potentially serious symptoms and when to seek medical help with subsequent infants. In response to these requests the foundation, with the assistance of its consultant and paediatric general practitioner members, has published a card entitled "Some suggestions: when to consult a doctor about your baby." This describes "urgent" and "sometimes serious" symptoms, including those non-specific symptoms of "unusual drowsiness . . . excessive crying, an

altered character to the cry, or being off feeds" which the authors discuss in their report.

A written list does not, of course, replace personal advice, but it does complement it as a reference aid for literate parents. Trials of this card in child health clinics and comments of many health visitors and parents indicate that the advice is helpful to many new mothers and not just those who have formerly lost babies, giving them a better understanding of the significance of symptoms and confidence in describing their baby's condition or pressing for an immediate consultation with their doctor.

While some local medical committees have expressed concern that this advice may cause unnecessary alarm, the experience so far of parents from a wide variety of social backgrounds is that they feel reassured to be given advice which they need and which is often not explicitly available in infant care books. We believe it is better to give preventive advice than to receive such sad comments as the following, which was part of a letter sent to us by bereaved parents who had sought advice with their next child: "If we had had such advice for our first baby (who died) we would have contacted the doctor much earlier."

The card "When to consult a doctor about your baby" is available from The Foundation for the Study of Infant Deaths, 5th floor, 4 Grosvenor Place, London SW1X 7HD.

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SIR,—We wish to take issue with several aspects of the recent preliminary report of the DHSS multicentre study of postneonatal mortality (4 November, p 1249). In our opinion the methods of acquiring data are open to bias, the controls are not realistic, the weighting and selection of prodromal symptoms are inappropriate, and some of the concluding remarks are misleading and potentially harmful.

In the DHSS study the parents of infants dying unexpectedly and suddenly are interviewed soon after the child's funeral. So-called major or minor symptoms in the days before death are elicited and their frequency compared with the frequency of such symptoms retrospectively determined in a control group of children, the majority of whom remain well. Interviewer bias cannot be eliminated as the interviewers know in advance if they are interviewing the parents of a live or dead baby. With regard to symptomatology the definition of a major or minor symptom is open to varying interpretation. For example, is every cough a major symptom requiring a medical opinion that day? We were also surprised to find that signs previously described in association with "near-miss" sudden infant death syndrome (SIDS)¹ such as pallor and cyanotic attacks were not included in this report. A realistic control group may be impossible to obtain, but a comparison of symptoms in SIDS babies with symptoms in babies dying unexpectedly and suddenly from known diseases may be more appropriate.

Our principal concern, however, is with the counterproductive statement contained in the penultimate paragraph of the paper. This states that prospective studies must wait until further epidemiological studies have