could account for the shorter time taken to find the first gland, eg 'learning curve' factors, as we do not know the temporal relationship of these cases to this unit's experience.

The results, although encouraging for the preoperative identification of adenomas, are not very encouraging for the preoperative identification of hyperplastic glands, particularly glands less than 180 mg in weight. The normal weight of a parathyroid gland is 30-40 mg, however normal glands up to 70 mg in weight have been described[1]. The authors identified only 44% of glands weighing less than 180 mg and thus they demonstrated that small glands are difficult to identify by scintigraphy. The small glands are the crux of the problem in the surgical management of primary hyperparathyroidism, as they are the parathyroids that most commonly lead to difficulty in identification at exploration of the neck. As an experienced parathyroid surgeon may expect to identify up to 95% of diseased glands[1] at an initial neck exploration, what is the impact of routine scintigraphy on patients with small adenomas or small hyperplastic glands?

The main use of preoperative localization techniques will surely continue to be for cases requiring re-exploration, where even the most experienced operator may find difficulty in localizing a gland at surgery. The authors state that they believe that 'the results in potentially difficult second-look operations will be improved if based on a wide experience of straightforward cases'. This is undoubtedly true, but is difficult to justify in the light of the new regulations for exposure of patients to ionising radiation[2], where benefit must be shown for an individual patient before an irradiating investigation. A case could perhaps be justifiably made for this policy in a unit with a large referral practise for reoperative cases, but these do not seem to form a significant portion of the cases in this series.

The technique described in this paper is exciting and the complementary information it provides may well prove to be a useful addition to the diagnostic armamentarium of the endocrine surgeon. We would suggest however, that the indications should be more selective.

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The homeopathic conundrum

In the editorial (September 1990 *JRSM*, p 543) which reviews three papers which show that homeopathic remedies are successful, you conclude by saying 'Yet, if these clinical trials are correct, modern pharmacology may yet be rocked to its foundations . . .' Perhaps another explanation would be that there is still a flaw in our system of performing clinical trials. Maybe this would rock many others to their foundations!

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Advanced head and neck cancer

We read with interest the excellent and original report by Maher and Jefferis (June 1990 *JRSM*, p 356) on the decision making in patients with advanced cancer of the head and neck and we would like to comment on some aspects:

(1) The most evident handicap of this paper is the lack of medical oncologist participating in the survey. In the diagnostic workup and therapeutical decision making of the head and neck cancer, as in majority of neoplasms, a multimodality approach is needed and it must be conducted by a medical oncologist. In this regard, a question from this report remains to be addressed: Who uses the chemotherapy among the questioned practitioners?

(2) At our hospital we discuss the case records at Committees with different specialists (otolaryngologists, maxillo-facial surgeons and radiotherapists). In patients with advanced disease we use initial chemotherapy (neoadjuvant) and further surgery (if the tumor is reduced to make surgical resection suitable) or radiotherapy[1].

(3) Finally, three observations: (a) we would like to emphasize the aggressive therapeutical attitude of the majority of the surveyed physicians, (b) this paper calls into question the importance of quality of life in cancer patients, (c) we think that further questioning on the majority of neoplasms therapy should be undertaken to confirm the high doses of empirism with which modern oncology moves on.

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Reference

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Non-specific abdominal pain - an expensive mystery?

Gallegos and Hobsley (June 1989 *JRSM*, p 343; January 1990 *JRSM*, p 60; August 1990 *JRSM*, p 536) are to be supported in their campaign to dispel the mystery of non-specific abdominal pain. All their arguments are valid. What would clinch their argument that tenderness of the soft tissues is a major factor in 20% of cases of abdominal pain, would be cure of these patients. The use of local anaesthetic to confirm the diagnosis of 'trigger points' and nerve entrapment syndrome (Hall and Lee, January 1990 *JRSM*, p 59; Galegos, January 1990 *JRSM*, p 60) may protect the patient from an unnecessary laparotomy, but does not cure the pain.

I hope that Hall and Lee and Gallegos and Hobsley will re-examine and treat their patients in the light of my results in a series of 98 patients with painful conditions of the abdominal wall[1]. There were four groups of patients: accident and stress (which included anterior branches of the intercostal nerves), scar pain, linea alba strain, and 'maladie d'amour' (associated with conjugal activities).

Injection of a mixture of triamcinolone acetonide and lignocaine into the site of pain cured the pain in 75-100% of the above groups.

It is important to remove the pain if reassurance is to have optimum effect.

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Reference

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A tale of two paintings

In his account of Robert Smirke's paintings depicting resuscitation (August 1990 *JRSM*, p 520), John Griffin mentions two prints of these pictures at the 'Royal Bath Hospital' and suggests that their provenance may have been William Heberden. I assume the author is referring to the Royal National Hospital for Rheumatic Diseases (formerly known simply as the Bath Hospital). As far as I am aware, the only connection that this hospital has with Heberden, other than a mutual interest in rheumatological disease, is the possession of one of the physician's cut-throat razors which was donated to the hospital after the last war.

The prints in question now hang in the Post-graduate Medical Centre at the Royal United Hospital following their donation to the Bath Clinical Society by Drs Henry and Zeta Eastes who acquired them in 1939 from the widow of Dr Ivan Hawes, a general practitioner in the village of Wick, a few miles east of Bristol. One must assume that Dr Hawes interest in the prints was one of family connection.

Ironically, it is fitting that these prints should have come to rest in Bath. It was in this city that Dr Thomas Cogan, the co-founder of the Royal Humane Society, spent some time in practice and, in 1805, founded the Bath Humane Society which still flourishes. The Bath society was originally run on identical lines to its London counterpart, though in recent years it has merely provided funds for prizes in life saving.

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Saint Vitus' dance

Having read with interest and appreciation the paper on 'Saint Vitus dance: vital misconceptions by Sydenham and Bruegel' by Park and Park (August 1990 *JRSM*, p 512), it seems appropriate to draw attention to the remarks of Felix Platter (1539-1614) on this condition.

Platter refers to Saint Vitus' dance in his *Praxeos Medicae* (1602) as 'that frightening and remarkable though rare disease' and comments 'There are of course some who for the sake of extorting a sizeable alms or because they have been put up to it do imitations of this disorder to impress on the minds of men the erroneous popular conception of the disease. ... An alienated mind however, may be impelled into this insanity and perverse appetite'[1].

It is evident that Platter, like Paracelsus, considered Saint Vitus' dance to be heterogeneous in nature.

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Reference

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I don't fancy cheiromancy

I presume that Newrick *et al.* (August 1990 *JRSM*, p 499) are writing tongue (or hand) in cheek when they relate length of lifeline to longevity. In the normal hand the main palmar creases, including the so-called lifeline, are present by the 14th week of embryonic life and indicate binding down of skin to deeper structures. Do these palmar creases elongate with age? Certainly my own main creases have not altered when I compare prints I took 20 years ago. However, the late Professor L S Penrose mentioned that the lifelines may lengthen and become more complex as age increases[1]. This suggests that orthodox medicine is correct to dismiss out of hand the idea of a predictive value for lifeline length. Indeed the Old Testament notes that '... ye have not found ought in my hand' (I Samuel 12:5) and '... there is nothing in his hand' (Ecclesiastes 5:13).

Will there now be a proposal to form a Royal College of Palmistry?

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Reference

1 Penrose LS. Fingerprints and palmistry. *Lancet* 1973; i:1239-42

We wish to applaud the adroit attempt by Newrick *et al.* to get to grips with the neglected science of palmistry (August 1990 *JRSM*, p 499). They claim a strong statistical correlation between lifeline length in the right hand and age at death, lifeline length in the left hand being less significant. However, the dexterity with which they put across their case disguises a vital omission. One might expect 10 of the 100 cases to be left-handed, yet the data they present was not corrected for hand dominance. One of us (JRN) is a southpaw and is mortified that his death might be predicted from the examination of the right (ie wrong) extremity. Does this disregard mere sleight-of-hand, or something more sinister? It would be a shame if an exciting new diagnostic tool were to slip through our fingers as a result of such high-handedness.

Finally, regarding their suggestion of 10-year meetings in exotic locations, may we suggest Las Palmas for the first conference?

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