by computed tomography in our patient. Although the lesion was discernible in retrospect, it was not possible to make a confident diagnosis from the images obtained at the time. Finally, these tumours in patients with acquired cystic disease of the kidney have a metastatic potential of 5%, but the current consensus is that lesions under 3 cm diameter should be treated conservatively.[3-5] Although there was no evidence of metastatic spread in our patient, the tumour had invaded deeply into the renal cortex, and we would therefore suggest that this policy is kept under critical review.

N J VINER
A G MACIVER
J C MACKENZIE

Departments of Nephrology and Pathology,
Southmead Hospital,
Bristol BS10 5NB

1 Bommer J, Waldherr R, Van Kaick C, Strauss L, Ritz E. Acquired renal cysts in uraemic patients—in vivo demonstration by computed tomography. *Clin Nephrol* 1980;14:299-303.
2 Chung-Park M, Ricanati E, Lankerani M, Kedia K. Acquired renal cysts and multiple renal cell and urothelial tumours. *Am J Clin Pathol* 1983;79:238-42.
3 Ishikawa I. Uraemic acquired cystic disease of the kidney. *Urology* 1985;26:101-8.
4 Grantham JL, Levine E. Acquired cystic disease: replacing one kidney disease with another kidney. *Kidney Int* 1985;28: 99-105.
5 Bretan PN, Busch MP, Hricak H, Williams RD. Chronic renal failure: a significant risk factor in the development of acquired renal cysts and renal cell carcinoma. *Cancer* 1986;57:1871-9.

Raised concentrations of plasma atrial natriuretic peptides in cardiac transplant recipients

SIR,—We read with interest the report by Dr Donald R J Singer and others (29 November, p 1391). We have measured plasma immunoreactive atrial natriuretic peptide (ANP) concentrations in five cardiac transplant recipients after one hour at rest (sitting) and during a period of lower body positive pressure. The latter is a complex stimulus but we have shown that it results in an increase in right and left atrial dimension, systemic blood pressure, and plasma immunoreactive ANP concentrations in healthy volunteers.[1]

The five cardiac transplant recipients were all men, aged 28 to 53 years (mean 38 years), who had had their transplants 15 to 80 months (mean 38·6 (SD 11) months) earlier. All were well and showed no signs of rejection or cardiac failure. Mean (SEM) blood pressure was 123/78 (4·8/3·9) mm Hg and serum creatinine 149·5 (16·4) μmol/l. They were taking the following medication: cyclosporin (4), azathioprine (3), prednisolone (3), nifedipine retard (2), diuretics (3), methyldopa (1), and hydralazine (1).

These patients followed the same protocol as our healthy volunteers.[1] They attended a hospital sideroom on two mornings. After one hour sitting at rest they wore a medical antishock trouser (MAST) suit for one hour. On one occasion for each volunteer this was inflated to 40 mm Hg pressure. After 60 minutes the suit was removed and the patients remained sitting for a further hour. Blood was taken before inflating the suit (time 0) and at 5, 20, and 60 minutes of suit inflation and at 90 minutes—that is, 30 minutes after the suit had been removed. Plasma immunoreactive ANP concentration was measured by radioimmunoassay.[2]

The cardiac transplant recipients had a higher mean basal plasma immunoreactive ANP concentration than our healthy volunteers (n=6; aged 20 to 28 years) by a factor of 9 (table). This is greater than that reported by Dr Singer and colleagues, but our two study groups were not matched. Our healthy volunteers were younger, had lower blood pressures, and had normal renal function, all of which may have enhanced but would not have accounted for the difference in ANP values between the two groups. Plasma immunoreactive ANP concentration was raised in our one patient (aged 30 years) with normal blood pressure and renal function 80 months after transplantation. There was a tendency within the group for ANP values to fall with time since transplantation.

Mean (SEM) basal plasma immunoreactive ANP concentration and response to lower body positive pressure in healthy volunteers (n=6) and cardiac transplant recipients (n=5)

Time (mins)	Plasma immunoreactive ANP (pmol/l)	
	Healthy volunteers*	Transplant recipients
0	5·7 (1·1)	58·7 (12·6)
Inflation		
5	5·3 (0·7)	58·2 (10·5)
20	8·9 (1·8)†	69·2 (11·3)
60	9·8 (1·3)†	94·0 (15·1)†
90	5·7 (0·8)	59·0 (9·6)

*Values for healthy volunteers reproduced by kind permission of the editor, *Journal of Hypertension*.
†$p<0·05$ compared with time 0.

Plasma immunoreactive ANP values increased in all five patients during lower body positive pressure. The response seemed to be more delayed in the transplant recipients than in the healthy volunteers. The percentage rise at 20 minutes (mean (SEM) 128·8 (15·1)%) was less than that for our healthy volunteers (157·8 (9·7)%) but at 60 minutes the difference was less pronounced (171·2 (13·4) *v* 189 (28·6)%).

There was no significant change in mean plasma immunoreactive ANP concentration in either group on the non-inflation day. The blunted rise in ANP concentrations in response to lower body positive pressure compared with that in healthy controls is in keeping with the observations of Tomlanovitch *et al*, who (using urinary cyclic guanosine monophosphate as a measure of ANP activity) found an attenuated response to water immersion in a group of cardiac transplant recipients.[3] The explanation for this blunting effect in these patients is unclear but may in part be due to increased atrial size, which might dilute the expected increase in atrial dimension after lower body positive pressure.

M R WILKINS
M D GAMMAGE
HELEN M LEWIS
L B TAN

Departments of Pharmacology and Cardiology,
University of Birmingham,
Birmingham B15 2TJ

1 Wilkins MR, Gammage MD, Tan LB, *et al*. Effect of lower-body positive-pressure on atrial dimension and plasma atrial natriuretic peptide concentration. *J Hypertension* (in press).
2 Wilkins MR, Wood JA, Adu D, *et al*. Change in plasma immunoreactive atrial natriuretic peptide concentration during sequential ultrafiltration and haemodialysis. *Clin Sci* 1986;71:157-60.
3 Tomlanovitch S, Peterson C, Molina C, *et al*. Role of atrial natriuretic hormone (ANH) and cardiac nerves in renal response to hypervolaemia in man. *Clinical Research* 1986;34: 702A.

Heat inactivation of specimens—AIDS

SIR,—Our attention has been drawn to some apparent misunderstanding of the wording of paragraph 61 of the revised guidelines on AIDS issued by the Advisory Committee on Dangerous Pathogens in July of this year (*LAV/HTLV III—The Causative Agent of AIDS and Related Conditions*. HN(86)20).

The impression in some quarters is that this paragraph implies that the AIDS virus is not susceptible to heat treatment. This is not the message which it was intended to convey. Spire *et al* reported on the successful inactivation of viral enzyme by what might be regarded as the standard heat treatment regimen—namely, 56°C for 30 minutes.[1] But a later paper by Resnick *et al* cast some doubt on the efficacy of inactivation of the virus over this period.[2]

Although Resnick *et al* showed that the high titred virus they used was inactivated by tenfold in 20 minutes, there is to our knowledge no reliable estimate of the range of infectious virus titres or the rate of inactivation of virus in the serum of naturally infected subjects. Little could therefore be said about what combination of time and temperature would give a reasonable margin of safety. Also, there appears to be little or no information on the effects of longer heating times on serum analytes. Most published work refers only to periods of 30 to 60 minutes.

The committee was therefore unable, without further supporting evidence, to recommend any particular heat treatment regimen. Thus in paragraph 61 of the revised guidelines the use of the phrase "practical heat treatment procedure" was intended to convey both uncertainty about the duration of heating required to achieve inactivation of the virus and the lack of information on the reliability of results obtained from subsequent tests.

MICHAEL CHAPMAN
PETER LISTER

Joint Secretariat of the Advisory Committee on Dangerous Pathogens,
DHSS,
London SE1 6TE

1 Spire B, Dormont B, Barré-Sinoussi F, Montagnier L, Chermann JC. Inactivation of lymphadenopathy-associated virus by heat, gamma rays, and ultraviolet light. *Lancet* 1985;i:188-9.
2 Resnick L, Veren K, Salahuddin SZ, Tondreau S, Markham PD. Stability and inactivation of HTLV-III/LAV under clinical and laboratory environments. *JAMA* 1986;255:1887-91.

Primary pulmonary hypertension

SIR,—We would like to clarify one or two matters which seem to have been obscured in Dr Tim Higenbottam's leading article (6 December, p 1456).

It is fundamental to understand that gas transfer for carbon monoxide is *always* low in primary pulmonary hypertension because of a greatly reduced pulmonary capillary volume secondary to constriction or obliteration of pulmonary arterioles. Primary pulmonary hypertension due to this cause has to be distinguished from pulmonary veno-occlusive disease and also from thromboembolic pulmonary hypertension, in neither of which is there benefit from vasodilator drugs. The clinical and radiological signs, incidentally, are of the pulmonary hypertension and not of the raised pulmonary vascular resistance which causes this hypertension.

Treatment is aimed both towards raising the low cardiac output and to reducing the pulmonary artery pressure in the hope of retarding progression of hypertensive damage to the lung bed.[1 2]

A good response to a vasodilator is most likely in patients who are discovered at an earlier stage of the disease. These have lower pulmonary artery pressures and higher cardiac outputs. We found that the acute response to prostacyclin was no better at predicting the degree of reversible vasoconstriction than the response to nifedipine but had the virtue of being immediately reversible in the event of an unfavourable response with a fall in systemic blood pressure. Since oral nifedipine was

just as effective as intravenous prostacyclin in removing the reversible component of pulmonary hypertension in our patients, we regard the use of long term intravenous prostacyclin infusion[3] as unnecessary, complicated, dangerous, expensive, and certainly of unproved benefit compared with oral vasodilator therapy.

CELIA M OAKLEY

Royal Postgraduate Medical School,
Hammersmith Hospital,
London W12 0HS

ADRIAN ROZKOVEC

Bristol Royal Infirmary,
Bristol

1 Wagenvoort CA, Nauta J, von der Schaar PJ, Weeda HWH, Wagenvoort N. Effect of flow and pressure on pulmonary vessels. A semiquantitative study based on lung biopsies. *Circulation* 1967;35:1028-37.
2 Reeves JT, Groves BM, Turkevich D. The case for treatment of selected patients with primary pulmonary hypertension. *Am Rev Respir Dis* 1986;13:342-6.
3 Jones DK, Higenbottam TW, Wallwork J. Practical experience of long-term prostacyclin with primary pulmonary hypertension. *Am Rev Respir Dis* 1985;31:85.

What is a good GP?

SIR,—This series would appear to have touched many a raw nerve. This was to be expected and I shall always remain openminded to informed criticism.

Dr Roger Jones's vehement attack (6 December, p 1503) on my article (1 November, p 1152) seems unnecessarily acerbic. I will confine my reply to three points. Firstly, specialists by definition know more about their subject than general practitioners. Whether or not a GP acts on a specialist's advice may be influenced by an individual patient's circumstances. Foreknowledge of these circumstances by the specialist will make the advice more appropriate and acceptable. Secondly, all radiology departments work to budgetary constraints not imposed on GPs. In discussing cost-benefit analysis in the overall management of the patient, Dr Jones is supporting my case. All I ask is that the reasoning behind each request be communicated. Finally, his last paragraph is misleading. I did not work at this hospital 12 years ago and cannot speak for relationships then. The guidelines issued locally two years ago after full discussion with GP representatives have been generally well received and the regular workshops which I hold for trainees are well attended.

NICHOLAS L BISHOP

Royal Sussex County Hospital,
Brighton BN2 5BE

Corticosteroids and bone mass in asthma

SIR,—Dr D M Reid and his colleagues (6 December, p 1463) express concern because of their finding of a slight reduction of total body calcium (mean of 8·8%) in patients with bronchial asthma treated with conventional doses of inhaled corticosteroids.

They state that only 16 of a small group of 22 patients had been given "booster" courses of oral prednisolone but that this information was "calculated retrospectively from the case records supplemented when necessary by information from the patient." Since many of these patients were under my care I find it embarrassing to have to admit that such precise information is not available from our case records. It is conceivable that these patients had received much more systemic corticosteroid therapy than was apparent from the case records or even from questioning the patients. I believe this to be the case and suggest this as one explanation of the differences in the total body calcium estimations between patients being treated with low dose inhaled corticosteroids only (group 3) and those in whom corticosteroid therapy of any kind had never been used (group 4). It is interesting to note that the patients who had had no corticosteroid treatment had a mean age of 37·9 compared with 55·6 for the inhaled corticosteroid group. This age difference in itself could at least partially explain the differences between the total body calcium results.

Although I have read this paper many times in draft form and now after its publication I am still unclear about the derivation of the normal range of total body calcium. The demographic details of the 40 controls are not included in the paper, and therefore one has to question the validity of the "normal values." It is of interest to see that the small group of 12 asthmatic patients who had had no form of corticosteroid treatment (according to the case records) was found to have a mean total body calcium value just within the lower limit of the mean control value.

If the finding that total body calcium concentration is below normal in patients receiving only inhaled corticosteroids in conventional doses is correct Dr Reid and his colleagues must be congratulated for drawing our attention to a potential danger to which we are allowing many of our patients to be exposed. However, I do not think that on the present evidence we should have much concern about the use of inhaled corticosteroids in the doses quoted by Dr Reid and his colleagues.

GRAHAM K CROMPTON

Respiratory Unit,
Northern General Hospital,
Edinburgh EH5 2DQ

Neurological and neurosurgical approaches in the management of malignant brain tumours

SIR,—Many patients with malignant glioma in the United Kingdom do not have a histological confirmation of the diagnosis of any treatment other than steroids—a practice which is increasingly at variance with that in North America and Europe. There are few centres in the UK which take an interest in developing treatment, and little of the current literature on treatment comes from Great Britain. Some of the reasons for this are encapsulated in the article by Dr J S Wroe and colleagues (18 October, p 1015). The main conclusion which should be drawn from their report is that a non-randomised retrospective analysis of the value of treatment in which only half the patients in the therapy group were treated and a quarter of the patients in the control group were also treated and where no histological diagnosis was made in half of the control group is unlikely to give a reliable measure of the value of treatment. This would not matter and the analysis could be ignored were it not that therapeutic nihilism in the UK does not need further encouragement.

Tumour diagnosis using computed tomography and the role of free hand biopsy have been examined by other correspondents (8 November, p 1236). We would like to address several additional questions about the paper and the subsequent reply (22 November, p 1373). There is a major discrepancy in the results in the paper. The text refers to only 18 cases surviving three years, but table V lists 27 cases surviving three years, a difference of 50%.

In fig 2 of those receiving radiotherapy almost all underwent surgery, in some cases radical resection, while of those not receiving radiotherapy two thirds did not undergo surgery. The beneficial effects of radiotherapy are probably greater than indicated because the latter group probably contained patients with undiagnosed low grade gliomas, benign tumours, or no tumours. It should be straightforward to decide whether patients who received radiotherapy survived longer as a result of treatment rather than performance status or age using multivariate analysis. The authors cite a reference of Salcman as support for their contention that radiotherapy has little value.[1] But this is seriously misquoted. Salcman says: "The recent prospective studies of the BTSG [Brain Tumor Study Group] have clearly indicated the value of radiation therapy in prolonging the survival of patients with glioblastoma.[2] The present analysis of a large population of patients is unequivocal in finding that the addition of radiotherapy is decisive both in producing long term survivors and in markedly improving the survival percentage at intermediate periods during the first 18 months after operation. Withholding radiation treatment from the glioblastoma patient is unwarranted except when special clinical circumstances supervene."

Dr Wroe and colleagues recommend that future trials should include patients treated only with steroids. This has already been done in a major randomised prospective controlled study.[2] In this and other trials[3] radiotherapy has been shown to be superior to conservative management. There is little point in repeating such work. Furthermore, chemotherapy combined with radiotherapy produces a modest, but statistically significant, further improvement in results.[4,5] They suggest that controlled trials are unavailable for malignant glioma. Both the EORTC and the MRC are conducting multicentre trials of this type in which several centres in the UK are already participating. Future trials are planned, and if Dr Wroe and his colleagues want to improve the clinical results for glioma it would seem sensible for them to participate in them.

D G T THOMAS
J L DARLING

Neuro-oncology Section,
Gough-Cooper Department of Neurological Surgery,
Institute of Neurology,
National Hospital,
London WC1N 3BG

J S TOBIAS
R L SOUHAMI

Department of Radiotherapy and Oncology,
University College Hospital,
London WC1E 6AU

E A PAUL

Department of Community Medicine,
St Thomas's Hospital,
London SE1 7EH

1 Salcman M. Survival in glioblastoma: historical perspective. *Neurosurgery* 1980;7:435-9.
2 Walker MD, Alexander E, Jr, Hunt WE, *et al.* Evaluation of BCNU and/or radiotherapy in the treatment of anaplastic gliomas. *J Neurosurg* 1978;49:333-43.
3 Kristiansen K, Hagen S, Kollevold T, *et al.* Combined modality therapy of operated astrocytomas Grade III and IV. Confirmation of the value of postoperative irradiation and lack of potentiation of bleomycin on survival time: a prospective multicenter trial of the Scandinavian Glioblastoma Study Group. *Cancer* 1981;47:649-52.
4 Walker MD, Green SB, Byar DP, *et al.* Randomized comparisons of radiotherapy and nitrosources for the treatment of malignant glioma after surgery. *N Engl J Med* 1980;303:1323-9.
5 Green SB, Byar DP, Walker MD, *et al.* Comparison of carmustine, procarbazine and high-dose methyl prednisolone as additions to surgery and radiotherapy for the treatment of malignant glioma. *Cancer Treat Rep* 1983;67:121-32.

SIR,—Dr S J Wroe and colleagues suggest that a conservative approach to management of glioma with steroids alone can be justified ethically. Their conclusion is supported by neither their data nor an impressive body of reports indicating that maximally feasible surgical resection followed by radiotherapy (and perhaps chemotherapy) will extend both the duration and quality of life of patients with malignant gliomas.[1,2] In their study patients of neurosurgeons underwent "craniotomy with biopsy" more often and "no surgical treatment" less often than did patients of neurologists. Shockingly, only 12% of the neurological patients and 7% of the neurosurgical patients received surgical resection. More patients in the surgical group received radiotherapy. More (not statistic-