calculi under 2 mm in diameter.³ To remove most large branched calculi (and we now have experience of 154 such stones) it is usually quite unnecessary to cool the kidney. However, there certainly are occasions when the extra protection afforded by hypothermia is useful in permitting a more thorough and more leisurely search for stones, particularly when these are lying in a relatively healthy thick mass of renal parenchyma. Indeed, as you should have remembered, we have been to some trouble to devise a very simple and effective method of renal hypothermia which any surgeon can use without purchasing expensive apparatus for the purpose.⁴

The need to cool the kidney has arisen about a dozen times in our experience with staghorn stones and about an equal number of times as an aid in doing other conservative operations on the kidney. The surgeon who embarks on any big and difficult operation on the kidney should not do so without providing himself with the means to cool it. But we do not think he is likely to use them very often.—I am, etc.,

JOHN BLANDY

Department of Urology,
The London Hospital,
London E.1

¹ Singh, M., et al., *British Journal of Urology*, 1973, **45**, 593.

² Blandy, J. P., and Singh, M., *Journal of Urology*. In press.

³ Singh, M., Marshall, V. R., and Blandy, J. P., *British Journal of Urology*, 1975, **47**, 125.

⁴ Marshall, V. R., and Blandy, J. P., *British Journal of Urology*, 1974, **46**, 253.

Sexual Life after Gynaecological Operations

SIR,—I am concerned to find two of my personal friends, Mr. Alan Amias, consultant gynaecologist, and Mrs. Claire Rayner, medical journalist and sometime nursing sister, expressing such vituperative disagreement in your columns (9 August, p. 368). As a general practitioner and member of the Medical Journalists Association I stand midway between them.

I believe that both have unanswerable cases. Mr. Amias is quite right to be concerned by "erroneous notions" in patients' minds, but I think (without any hard evidence) that if he were to follow this up he would find a considerable divergence between what patients thought they had read (or even what the girl at the next desk said she had read) and what was actually written. This is in part the fault of journalists but perhaps more significantly may be the product of fear. Nevertheless, outside responsible "agony columns" such as Mrs. Rayner's, there does remain a body of journalism that seeks to titillate or scare and is not always mindful of the human consequence of doing so.

My own experience in this part of London is based not so much on hysterectomy as on abortion, but the same principles must surely apply. The greater part of my time has to be spent in dispelling erroneous fears, whatever their origin, rather than in clinical or practical matters. I know that Mr. Amias would be the first to agree with this approach and, indeed, his article speaks for itself.

Mrs. Rayner has gone farther and is so concerned by patients' erroneous fears that she has changed, or at least modified, her profession in an attempt to get through to

those whose need is greatest. Doctors should give her every encouragement because she may be able to help some among her vast number of readers whom even the medical profession may have made more frightened rather than less.—I am, etc.,

ROBERT LEFEVER

London S.W.7

SIR,—I feel that Mrs. Claire Rayner (9 August, p. 368) deserved a reply from Mr. A. G. Amias more in keeping with the normal tone of the correspondence columns of the *B.M.J.* I for one would be very grateful if Mr. Amias would be kind enough to supply a reference to back his assertion that "some of the poor results of hysterectomy can be directly attributed to the harmful effects of newspaper medicine" (14 June, p. 609). Can he also advise the readers of this journal of any research done anywhere in the world to substantiate his statement that "erroneous notions . . . are fostered by opinionated and ill-informed comment in the lay press"?

I am a humble new recruit to the ranks of medical journalism dealing with medical matters generally and with reader's letters on medical problems written to one of the many women's magazines. It is a humbling experience. Lack of knowledge, fear, guilt, anxiety, all compel the writers to seek help from other than their medical attendants. I am ashamed on occasion by the intolerance of my profession and its lack of ability to communicate. Until the medical profession can learn to do so there is a vital place for people like Mrs. Rayner who can and do answer a very clear and strongly felt need. I am sure that even Mr. Amias would see the need when he has read some of the sad letters we receive and would be relieved and even cheered by the humanity and wisdom shown in the replies given by Mrs. Rayner and others like her.

Of course the profession needs to learn how to communicate, but how can we criticize others who do our task for us because of our inability to do so. Mr. Amias's comments do not help.—I am, etc.,

GRAHAM CURTIS JENKINS

Ashford, Middlesex

Reversed Loops in the Short Gut Syndrome

SIR,—Your leading article (28 June, p. 709) was a welcome précis of the sequelae of massive bowel resection. The following observations on this intriguing subject may be of relevance.

(1) It is agreed that reversed segments may be of value in the management of established cases of the short gut syndrome. However, recent experimental studies¹ showed that reversal at the time of resection significantly lowered the extremely high mortality of this operation ($P < 0.001$). Indeed, in subsequent studies (unpublished) delayed or secondary reversal did not confer this benefit.

(2) The varying capacity of intestinal remnants to adapt structurally and functionally to the insult is undeniable.² Within weeks of primary reversal, and long before sufficient adaptation was anticipated, consistent and progressive improvement in

nutritional indices and survival was recorded.¹ Thus likely candidates for the short gut syndrome may have been retrieved.

(3) Of 27 documented cases reviewed, almost half had primary reversals with good results. While the reports by Venables *et al.*³ and Fink and Olson⁴ cited in your article are excellent, some others^{5,6} unfortunately lack clinical evaluation and are inconclusive. Nevertheless, of all adjunctive surgical procedures, attempted, segmental reversal is the most encouraging⁷ and its potential application in clinical practice must be evaluated further. An understanding of the pathophysiology of extensive resection and its alteration by reversal is required. Gastrointestinal hormonal data from recent animal studies⁸ suggest possible mechanisms for certain secondary disorders such as accelerated transit and gastric hypersecretion.

(4) Rygick and Nasarov⁹ introduced an elegant technique on the misapprehension, not founded in reality,¹⁰ that rotation compromised vascularity. While that technique may prove useful in cases of Crohn's disease with shortened mesentery,¹¹ it was applied originally to 20-cm segments—that is, well above the optimal range quoted in your article. With 10-cm segments it was awkward and unnecessary.¹ The advice of Ellis and Coll¹² to observe the segment for 30 min on completion of anastomoses was adequate precaution after rotation. I am aware of only one case of anastomotic breakdown after secondary reversal.¹³

(5) Reversed loops have no place when 100% resection (ligament of Treitz to ileocaecal junction) becomes obligatory. Despite enthusiastic treatment, including central cannulation and arteriovenous shunts, the serious dilemma of prolonged management remained unsolved. Even recent investigations¹⁴ with colonic interposition were done with 10% intact small bowel.—I am, etc.,

AIRES BARROS D'SA

Department of Surgery,
Queen's University,
Institute of Clinical Science,
Belfast

¹ Barros D'Sa, A. A. B., et al., *British Journal of Surgery*, 1974, **61**, 917.

² Dowling, R. H., *New England Journal of Medicine*, 1973, **288**, 520.

³ Venables, C. W., Ellis, H., and Smith, A. D., *Lancet*, 1966, **2**, 1390.

⁴ Fink, W. J., and Olson, J. D., *Archives of Surgery*, 1967, **94**, 700.

⁵ Madding, G. J., Kennedy, P. A., and McLaughlin, R. T., *Annals of Surgery*, 1965, **161**, 601.

⁶ Blades, J. F., and Peole, W. L., *Virginia Medical Monthly*, 1970, **97**, 3600.

⁷ Grenier, J. F., et al., *Acta Gastroenterologica Belgica*, 1973, **36**, 92.

⁸ Barros D'Sa, A. A. B., et al., *British Journal of Surgery*, 1975, **62**, 152.

⁹ Rygick, A. N., and Nasarov, I. U., *Diseases of the Colon and Rectum*, 1969, **12**, 49.

¹⁰ Hollender, L. F., *Mémoires de l'Académie de Chirurgie*, 1968, **94**, 877.

¹¹ Pettsemidis, D., and Kark, A. E., *American Journal of Gastroenterology*, 1974, **62**, 526.

¹² Ellis, H., and Coll, I., *British Medical Journal*, 1968, **1**, 556.

¹³ Forster, E., et al., *Bulletin de l'Association Nord-Lorraine de Gastroentérologie*, 1968, **15**.

¹⁴ Hutcher, N. E., Mendez-Picon, G., and Salpeter, A. M., *Journal of Pediatric Surgery*, 1973, **8**, 771.

Pleural Effusion

SIR,—It is sad to see that in your leading article (26 July, p. 192) pleural biopsy is not allocated its rightful place in diagnosis—as an automatic step during the first aspiration and not as something that may be needed later in difficult cases. There are good

reasons for this. At a later stage fluid may not be obtainable and the cause of the effusion remains unknown. Bacilli are grown from only a minority of tuberculous effusions and it takes many weeks; biopsy gives a diagnosis within a few days in about three-quarters of cases. In malignant disease biopsy, though less certain than in tuberculosis, adds very considerably to the diagnostic yield of fluid cytology and histology from other sites. We envy you your facility in finding the causes of pleural effusions. We find it increasingly difficult. Those who share our problem and who have also been trying for over 15 years to get pleural biopsies done automatically, have little cause to thank you for your contribution.—We are, etc.,

D. DAVIES
W. H. RODERICK SMITH

Thoracic Department,
City Hospital,
Nottingham

Serum α -Fetoprotein in Cystic Fibrosis

SIR,—Professor R. K. Chandra and others (29 March, p. 714), found raised serum levels of α -fetoprotein (AFP) in a heterozygote carrier of the gene for cystic fibrosis and in patients with the disease and some of the siblings. Dr. J. C. Wallwork and others and Dr. D. J. H. Brock and others (17 May, p. 392), however, state that they have been unable to confirm these findings, whereas Dr. J. A. Smith, of New York, reports findings which support those of Professor Chandra and his colleagues. The results of our estimations of serum AFP concentrations in 46 children with cystic fibrosis, 31 siblings, and 80 heterozygotes are shown in the table. Further data on normal values in infants, in whom high levels are usual, are being collected. The estimations were by a double antibody radioimmunoassay technique (α -Feto Riakit, Abbott Laboratories). The coefficient of variation of this method is 8%. Any results which were outside our normal range for adults (1–25 μ g/l) were repeated and the mean quoted. The log mean $\pm$ standard error of mean is given so that our findings may be compared with those of Professor Chandra and his colleagues.

Our results do not show any significant difference in AFP levels in patients, siblings, parents, or control children. It seems improbable, though not impossible, that the disease could have some very basic difference on opposite sides of the Atlantic. Probably the discrepancy in the findings will be explained by more detailed examination of the technique used. There is a great need for a reliable method for detecting the heterozygote carrier of cystic fibrosis. In this hospital

we now routinely screen all pregnancies for neural tube defects in the fetus by measuring maternal serum AFP. False positive findings have been reported in a number of conditions,¹ but it seems that cystic fibrosis, with a gene frequency of 1:20, need not be included in the list.—We are, etc.,

J. S. FITZSIMMONS
N. SMITH

Genetic Counselling Clinic and
Department of Biochemistry

E. J. HILLER
JANE WYNNE

Cystic Fibrosis Clinic,
City Hospital,
Nottingham

¹ Adinolfi, A., et al., *Journal of Medical Genetics*, 1975, 12, 138.

Pulled Elbow: Evidence for a Genetic Factor

SIR,—We read with great interest Dr. Cynthia M. Illingworth's article (21 June, p. 672) on 100 cases of this disorder. Our own series is much shorter, consisting of only 35 episodes, but may perhaps be of interest as they all occurred in a single family, our own. Five (three boys and two girls) of our seven children suffered between them 35 episodes (see table), the earliest occurring at the age of 8 months and the last episode at the age of 4 years. We always attributed this age limit to the imminence of the appearance of the centre of ossification of the head of the radius (4 years in girls, 5 years in boys). We agree that while the clinical picture is typical and reduction simple with dramatic relief of symptoms, thus confirming the diagnosis, x-ray films are of little diagnostic help. Indeed, after the first few episodes, for which we enlisted the help of orthopaedic and radiological consultants, we dispensed with such diagnostic aids.

Occurrence of Pulled Elbow in Seven Siblings

Sib. No.	Sex	Blood Group	Rh Status	Age at First Episode (years)	No. of Episodes	Age at Last Episode (years)
1	M	O	—	—	0	—
2	M	A	—	8/12	8	3
3	M	A	—	1 2/12	15	4
4	F	A	—	1 9/12	4	2 6/12
5	F	A	—	2	3	2 6/12
6	F	O	+	—	0	—
7	M	A	+	1 2/12	5	3

Perhaps the most striking feature of the present series is the genetic data. While both sexes were affected and both Rh-negative and Rh-positive siblings were involved, all five of the affected siblings and neither of the unaffected siblings were blood group A, suggesting a possible genetic linkage, particularly as one parent is group

O Rh-positive and the other group A Rh-negative.—We are, etc.,

N. F. METCALF
W. K. METCALF

University of Nebraska Medical Center,
Omaha,
Nebraska, U.S.A.

¹ *Gray's Anatomy*, ed. R. Warwick and P. L. Williams, 35th British edn., p. 332. Philadelphia, Saunders, 1973.

Abortion (Amendment) Bill

SIR,—On re-reading my letter to you (19 July, p. 160) I can understand how Dr. J. B. Clarke (9 August, p. 373) has misunderstood me, but I do not believe that it will support Dr. C. B. Goodhart's conclusions (19 July, p. 160). Both writers' deductions rest on the assumption that I have confused my terms.

A "moral action" can result only from a free choice of the best available alternative and may be legal or illegal (I defined "best" in my letter of 19 July). A "legal action" is an action in accordance with a code laid down by some authority and may be moral or immoral. A "political decision" in this context is the result of an assessment of the collective conscience of the nation in the forming of laws by which actions may be judged legal or illegal. "Medical ethics," being the collective conscience of the profession, is a code of behaviour comparable to parliamentary laws, and an action in accordance with it may be moral or immoral, legal or illegal.

I entirely agree with Dr. Goodhart's statement that we need both political and moral decisions and that our choice of legislators should be made on moral grounds. However, he does not seem to have understood that I maintain (1) that it is not possible to draft a law which is morally right in all circumstances; (2) that it is not right morally to obey laws blindly, because that is to relinquish our freedom of choice; and (3) that it may be morally right on occasions to break the law, though we must expect to be punished if we do.

The legislators' job, in my view, is to lay down limits to curb the unscrupulous and protect the rest of us; they should stick to their job and allow the private citizen to make his own moral decisions. That is to say that I believe that Parliament should enact that a fetus of more than, say, 28 weeks should have the right to live; a younger fetus should not have that right, and the decision to kill it or not should be a purely moral issue, untrammelled by legal considerations. The law against fornication is an example of what I think is required; some hold that fornication is always immoral, but it is only illegal before the age of consent.—I am, etc.,

J. B. METCALFE

Madeley,
Telford, Salop

* * This correspondence is now closed.—ED.,
B.M.J.

Administration after Reorganization

SIR,—We, the undersigned, have resigned from administration with the Trafford Area Health Authority, one as a member of the authority and the other as a consultant mem-

Serum α -Fetoprotein Concentrations in Patients with Cystic Fibrosis, Siblings, Parents, and Controls

	No. and Sex	Age Range (years)	Serum AFP Concentrations (μ g/l)		
			Range	Median	Log Mean $\pm$ S.E. of Mean
Patients	.. 46 M. 25 F. 21	1-17	1-44	11	11.4 $\pm$ 0.30
Controls	.. 23 M. 11 F. 12	1-13	3-51	9	9.1 $\pm$ 0.49
Siblings	.. 31 M. 14 F. 17	1-16	1-55	9	8.5 $\pm$ 0.38
Parents	.. 80 M. 35 F. 45	21-48	1-41	9	8.1 $\pm$ 0.22