different outcomes of our trial and Grethe Støa-Birketvedt's trial[3] could be explained by a difference in weight loss before randomisation. The weight of all the patients in our trial, however, remained stable within a few kilograms during the month before the trial, and most patients were at their maximum weight.

We did not show our data on the number of previous attempts at slimming. There was no difference, however, in the mean number of such attempts between the cimetidine and placebo groups. We agree that the different numbers of previous attempts in the cimetidine and the placebo groups in Støa-Birketvedt's trial cannot explain the remarkable weight loss reported.[3]

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1 Mangtani P, Seed P. Cimetidine and weight loss. *BMJ* 1993;**306**: 1543-4. (5 June.)
2 Rasmussen MH, Andersen T, Breum L, Gøtzsche P, Hilsted J. Cimetidine suspension as adjuvant to energy restricted diet in treating obesity. *BMJ* 1993;**306**:1093-6. (24 April.)
3 Støa-Birketvedt G. Effect of cimetidine suspension on appetite and weight in overweight subjects. *BMJ* 1993;**306**:1091-3. (24 April.)

Suicide in doctors

EDITOR,—Alison Tonks reports that "doctors of all grades are two or three times more likely than anyone else to kill themselves. Suicide is particularly common in women doctors."[1] This implies that women are more vulnerable than men to the stresses of a medical career.

Standardised mortality ratios are commonly used to determine whether the death rate of doctors (or any other occupational group) from a particular cause differs from that of the general population.[2,3] The most recent data give a standardised mortality ratio for suicide in female doctors of 311 (95% confidence interval 170 to 522) and in male doctors of 177 (137 to 226).[4,5] A comparison of such ratios is, however, misleading. The table shows clearly that the rate of suicide in women doctors is not higher than that in their male colleagues. If anything, the rate in men is slightly higher, with a male to female age adjusted ratio of 1·2 (0·7 to 2·2).

In the general population of England and Wales the picture is quite different (table). The suicide rate in men is double that in women. The standardised mortality ratios quoted for doctors are ratios of the rates in doctors to the rates in the population standardised for age and given by sex. The standardised mortality ratio for female doctors is higher than that for male doctors not because their suicide rate is higher but because the rate in other women is lower than that in other men.

"Why can't a woman be more like a man?" The answer, in this case, seems to be that a female doctor is all too like a man. Her risk of suicide is determined more by her profession than by her sex.

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1 Tonks A. Doctors are in good health but risk suicide, says BMA. *BMJ* 1993;**306**:1711. (26 June.)
2 BMA Board of Science and Education. *The morbidity and mortality of the medical profession.* London: BMA, 1993.
3 Richings JC, Khara GS, McDowell M. Suicide in young doctors. *Br J Psychiatry* 1986;**149**:475-8.
4 Office of Population Censuses and Surveys. *Occupational mortality. The registrar general's decennial supplement for Great Britain 1979-80, 1982-3.* London; HMSO, 1986. (Series DS No 6.)
5 Office of Population Censuses and Surveys. *Mortality statistics. Review of the registrar general on deaths by cause, sex and age in England and Wales, 1979, 1980, 1982 and 1983.* London: HMSO, 1980, 1982, 1983, 1984. (Series DH2, Nos 6, 7, 9, and 10.)

Comparison of suicide rates per million population per year in men and women in England and Wales[5] and in doctors in Britain[4]

Age (years)	Men: General population	Men: Doctors*	Men: Doctors: general population	Women: General population	Women: Doctors*	Women: Doctors: general population	Men: women: General population	Men: women: Doctors
20-24	93	385 (3)	4·14	34	0	0	2·74	
25-34	136	177 (15)	1·30	43	181 (5)	4·21	3·16	0·98
35-44	152	134 (8)	0·88	69	203 (3)	2·94	2·20	0·66
45-54	166	418 (19)	2·52	103	222 (3)	2·16	1·61	1·88
55-64 or 55-59†	176	457 (20)	2·60	113	449 (3)	3·97	1·56	1·02
20-64 or 20-59†			1·77			3·11	2·06	1·20

*Numbers of suicides by doctors are given in parentheses. †Men up to age 64 and women up to age 59.

Appendicectomy in children

EDITOR,—Rajendra Surana and colleagues have provoked the expected controversy with regard to their policy of delaying operations for appendicitis.[1] In the first 36 hours of acute appendicitis there can be no doubt that urgent operation with perioperative antibiotic prophylaxis is the best treatment. Now that effective antibiotics are widely available the best form of management in cases of doubt is active observation until the diagnosis is obvious.[2]

The same team has shown previously that the benefits of delayed treatment by senior staff outweigh the dangers associated with junior doctors performing urgent appendicectomies on children with perforated appendicitis in the middle of the night.[3] Stone has shown that after 36 hours synergistic peritonitis is present in most children with appendicitis, so urgent operation alone cannot effect a cure in this group.[4] Since 1987 it has been my policy to treat all children presenting with symptoms of more than 36 hours' duration with intravenous fluids, antibiotics, and analgesics and to delay operation until the patient's general condition has stabilised. I have recorded the progress of all of my patients with appendicitis prospectively, and followed up almost all for three months.

My policy has eliminated wound infections and intraperitoneal abscesses (rate of complications = 0%). The median length of stay in hospital is five days for those treated by delayed operation. Before 1987 children who presented late and underwent urgent surgery had a wound infection rate of 40%, and these children had a median stay in hospital of eight days; those without infection had a median stay of five days. Wound infection developed in 72% of children who underwent urgent operation after symptoms had been present for more than 48 hours.

I believe that these figures show that there is no place for urgent operation in children who present with appendicitis after the second day of the illness, but urgent intensive treatment with intravenous fluids, antibiotics, and analgesics is mandatory. On the other hand, I do not believe that Surana and colleagues have shown that children with early appendicitis actually benefit from the operation being delayed, and I agree with Morgan that they deserve better care than is generally provided in Britain.[5]

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1 Surana R, Quinn F, Puri P. Is it necessary to perform appendicectomy in the middle of the night in children? *BMJ* 1993;**306**:1168. (1 May.)
2 Jones PF. Active observation in management of acute abdominal pain in childhood. *BMJ* 1976;**ii**: 551-3.
3 Puri P, Boyd E, Cuiney EJ, O'Donnell B. Appendix mass in the very young child. *J Pediatr Surg* 1981;**16**:55-7.
4 Stone HH. Bacterial flora of appendicitis in children. *J Pediatr Surg* 1976;**11**:37-42.
5 Morgan RJ. Appendicectomy in children. *BMJ* 1993;**306**:1686. (19 June.)

Don't be complacent about cot death

EDITOR,—The news that infant mortality has fallen by one third over the past 10 years in Britain is welcome.[1] So also is some persuasive evidence that contributing to this has been a fall in the number of cot deaths, which has coincided with a change in advice on sleeping position.[2,3] We are concerned, however, that euphoria should not dominate over the fact that cot deaths still occur.

R R Gordon comments that "cot death may well disappear before the scientific cause for the death is fully elucidated."[4] It is far too early to make such a sweeping statement. For the 10 years up to 1991 about 10 cases of sudden unexpected infant death occurred each year in South Glamorgan, an incidence of just under 2/1000 live births. As in the rest of Britain,[2] the incidence halved in 1992. In the first six months of this year six sudden unexplained deaths in infancy occurred in South Glamorgan—more than we saw throughout the whole of last year.

We cannot yet tell whether the present decline in cot death in Britain and elsewhere.[2,3] is real or just a random variation in incidence. To imply that the nut has been cracked is to invite complacency.

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1 Headlines. *BMJ* 1993;**306**:1288. (15 May.)
2 Wigfield RE, Fleming PJ, Berry JP, Rudd TP, Golding J. Can the fall in Avon's sudden infant death rate be explained by changes in sleeping position? *BMJ* 1992;**304**:282-3.
3 Mitchell EA, Ford RPK, Taylor BJ, Stewart AW, Becroft DMO, Scragg R, *et al.* Further evidence supporting a causal relationship between prone sleeping position and SIDS. *Journal of Paediatrics and Child Health* 1992;**28**(suppl 1):S9-12.
4 Gordon RN. Reduction in infant mortality probably due to fall in cot deaths. *BMJ* 1993;**306**:1753.

Mild head injury

Guidelines should be flexible

EDITOR,—Patrick A Nee and colleagues conclude that "skull radiographs should be taken whenever the mechanism of injury suggests considerable force."[1] We believe that this should be an integral part of both national and local guidelines for managing patients with a head injury.

Although guidelines have become increasingly