from the Veterans Administration Cooperative Study, in which patients studied between 1972 and 1974 were randomly allocated to medical and surgical treatment groups. After excluding those with left main stem coronary artery disease, there was no significant difference in survival at three years between medical treatment (87%,) and surgical treatment (88%). Nor was there any significant difference in survival in subgroups classified according to the extent of coronary artery disease or pretreatment left ventricular function. Results in the subgroup with left main stem coronary artery disease had already been reported and showed significant improvement in survival with surgical treatment.

In patients with unstable angina pectoris many surgical series have offered encouraging results, but aggressive medical treatment can claim comparable successes, and most centres pursue this course initially. Investigation and surgical treatment are pursued either electively or with the failure of medical treatment. There is a place for surgery in patients with acute myocardial infarction—either to salvage the myocardium or for complications such as cardiogenic shock—but the published reports are of small heterogeneous series, and the place of emergency operation remains to be clarified. Finally, what about asymptomatic patients? Our experience is limited, and the present studies on survival do not justify extensive screening to find and treat such patients.


To sign or not to sign?

Journalistic anonymity is no longer fashionable: correspondents now have bylines, book reviews are signed, and the curious can usually put a name to the initials which accompany an obituary notice. Yet on this side of the Atlantic more leading articles are still unsigned than signed, and even that champion for the naming of parts, the ebullient editor of *World Medicine*, still keeps his readers guessing about the authorship of its authoritative, experienced, and even prejudiced leading articles. Is this attitude outdated? Should editors give the reader what benefit there is of knowing the author of an editorial, as Dr Gatherer argues (p 647)? And would the authority of a journal be diminished or enhanced by revealing all?

Authoritative opinions may certainly be found to support the signing argument. The Committee on Editorial Policy of the Council of Biological Editors had forthright views: “Because of the original purpose of editorials, the first person was expressed in the plural (we, us), and the writer was not identified. These practices are still widely observed but may be questioned today as artificial and anachronistic conventions. Many current editorials do not represent the views of the editors or the publishers, but rather of an invited expert. For this reason, it would seem preferable for editorials to be signed, and to be written in the first person singular. Such identification also enhances the writer’s sense of responsibility.”

Two thoughtful British experts are equally convinced that anonymity is bad. In a draft of a book which they have generously allowed us to quote they write: “Why not, then, name the authors and make it clear that they are real people, who can be mistaken in their judgment, rather than immortal beings whose prophetic utterances cannot be challenged or refuted?”

With so much authority amassed against editorial anonymity, then, can the BMJ justify continuing the practice? We believe that we can, on several grounds. Firstly, the choice of the author is the editor’s responsibility, usually with the aim of getting an authoritative and middle-of-the-road article. If, conversely, the aim is to put forward a polar view, then the editor will commission a signed article to appear elsewhere in the journal. The occasions for a propagandist leading article have to be chosen with care. Secondly, if the case argued by the leader writer is wrong, his readers will soon let the editor have details of any mistakes in angry contributions to the correspondence column—and, indeed, these may be a better way of ensuring that the truth gets across to the non-expert reader than any formal correction.

Next, many authors, though experts, are still young and unknown, yet those readers with traditional professional snobbery would assign more weight to an article according to the length of a curriculum vitae in the *Medical Directory* than to what the editorial actually says. By naming an author, moreover, the journal may run into two other difficulties: doctors may be tempted to criticise the article unjustly because they have a feud with its author; or, alternatively, because they are his friends, refrain from justified criticism with the aim of not hurting his feelings.

Finally, the fourth argument justifying editorial anonymity is the “political” element—not only medico-politics, but also an editorial policy that the drafter may not necessarily agree with, and few topics are now free of such overtones. For example, a lecturer in obstetrics might be happy to sign an article dealing solely with the technique of suction termination of pregnancy but not one which had a piece added by the editor on the desirability of making early abortions more widely available. Another focus for authors’ objections if their names were given is the heavy rewriting that most drafts undergo, including those written by the editorial staff: in the BMJ office each draft is subedited by no fewer than three people—not editorial nitpicking, but necessary, we believe, in the interests of accuracy and clarity.

Finally, anonymity does enable somebody confidentially concerned in a contentious issue to bring it to the surface. Such occasions are rare, but every editor can recall instances when informed comments have altered official policy in the making for the general good. Some might argue that this is cowardice, and that such a person should resign from whatever committee is concerned and publish his view. Nevertheless, given the way our society works, this would result in fewer disclosures rather than more and would be against the public interest.

These remarks apply only to general biomedical journals—such as the BMJ, Lancet, and Nature—and there may be a good case for special journals having signed editorials: here experts are writing for experts, and often opinions rather than facts are paramount. Nevertheless, for all the reasons we have given, the BMJ still prefers editorial anonymity, and at present one thing alone would make us relinquish this principle—a shortage of leader writers (which, it is rumoured, is bulked larger than any moral scruples as a deciding factor when several American journals changed their policy). Few of our leader writers have said that they would like to be named.
many are strongly against such a practice. Like our expert assessors, they work hard and unpraised for scant reward, yet the quality of the journal depends on them: we owe them a great debt of gratitude.


Sleep-induced respiratory obstruction and the heart

Readers of Dickens’s Pickwick Papers will be familiar with the fat boy: he was for ever falling asleep, “and snores as he waits at table.” In 1965 Gastaut et al described how patients with the Pickwick syndrome had nocturnal sleep characterised by an endless succession of apnoeic periods, each of which ended with violent snorts and arousal—to such a degree and with such frequency that the sleepiness by day could be understood as a consequence of lack of deep nocturnal sleep. When such patients were given tracheostomies their nocturnal sleep became normal, and their daytime sleepiness vanished, to reappear if, a year or more later, the tracheostomy was occluded.5

Obesity had been a traditional feature of the syndrome, but Lugaresi et al9 reported similar phenomena in non-obese patients, again relieved by tracheostomy. They suggested that sleep-induced upper-airway obstruction should be considered among the causes of insomnia, a fact later championed by workers at Stanford.8 Upper airway obstruction from adenoids and tonsils was already known to cause pulmonary hypertension and congestive heart failure in children,10 and it now became clear that tracheostomy was the only way of arresting or reversing progression to cor pulmonale in adults with the syndrome of sleep-induced apnoea.1

Recently the Stanford group have described the cardiovascular accompaniments of the syndrome in 15 patients,11 twelve of whom were treated by tracheostomy. The recurrent apnoeic episodes lasted 10-90 seconds each and occupied over half the total duration of sleep. During each episode the pressure in the pulmonary artery rose, and in most patients there was a progressive rise of both pulmonary and systemic arterial pressure throughout the whole night. In a few patients the airway obstruction disappeared during each period of REM (paradoxical) sleep, and when this happened the hypoxaemia and raised arterial pressure always abated.

Whereas normal sinus rhythm was present during wakefulness, a cyclic pattern of gross sinus arrhythmia appeared with the endless succession of apnoeas during sleep, the heart rate falling below 30 per minute and rising up to 120 every minute or so. Prolonged sinus pauses of as much as six seconds, second degree atrioventricular block, complex ventricular contractions, and limited runs of ventricular tachycardia were also observed. These abnormalities virtually disappeared after tracheostomy, but recurred if it was occluded. Twenty-four-hour monitoring of the electrocardiogram led the Stanford group to propose that the cardiac arrhythmias might account for the sudden deaths to which these patients are prone during sleep. The mechanism of the respiratory obstruction brought about by sleep is not yet clear and probably varies among patients, but oropharyngeal spasm or the falling backwards of the tongue may account for most cases.

Poisoning with alkylmercury compounds

Isolated instances of human mercury intoxication have been known for centuries, but the use of organic mercury compounds as fungicides has led to large-scale poisoning of whole communities. In the past 20 years there have been thousands of cases and something approaching 1000 deaths.1

The first reported widespread epidemic took place in Japan in the 1950s. Methylmercury, a byproduct of the plastics industry, was discharged into Minamata Bay in effluent and accumulated in high concentrations in the fish. Residents of the bay were poisoned after eating the fish.2 One of the first outbreaks associated with mercurial fungicides was in Pakistan in 1961, when over 100 people had chronic mercurial poisoning after eating treated seeds. Of 34 patients admitted to hospital, four died and five were removed while seriously ill by their families.3 Nevertheless, the worst outbreaks have been in Iraq, where ethyl mercury p-toluene sulphonamide was introduced as a pesticide in seed dressing by the Ministry of Agriculture in 1955. The farmers were supplied with the treated seeds and warned against eating them. Some farmers thought that washing the seeds with water would rid these of the mercury and then used it for making domestic bread. The first few sporadic cases were seen at the Republican Hospital in Mosul, in the north of Iraq, with neurological manifestations, including unsteadiness, tremor, and loss of vision and hearing. A total of 100 were seen between 1955 and 1959, and of these 14 died from the effect of the poison.4

The most catastrophic epidemic ever recorded took place among farmers and their families in Iraq in the winter of 1971-2, when 6530 patients were admitted to hospital. An account of the clinical features and progress of some affected children appears at p 613. Again, exposure to alkylmercury took place when farmers ate home-made bread made from grain treated with a methylmercury fungicide.5, 6 The signs of