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SUICIDE PREVENTION  
—MYTH OR MANDATE?

DEAR SIR,

Britain is the only country in the Western world which has significantly reduced its suicide rate in the last 10 years. Both the Samaritans and psychiatrists have claimed credit for this. In my letter (1), I pointed out that deaths from most forms of suicide have, over the years, remained fairly constant, except for those caused by poisonous substances—mostly pills—which have gone up, and those caused by domestic gas poisoning which have fallen dramatically. The fall in the number of gas suicide deaths over this period has exceeded the fall in the total number of suicide deaths. Hence the fall in gas suicide deaths more than accounts for the fall in Britain's suicide deaths.

From 1963 onwards the Gas Council has steadily reduced the content of lethal carbon monoxide in its domestic gas. I drew what appears to me to be a reasonable conclusion: that the credit for Britain's falling suicide rate should go to the Gas Council rather than to the Samaritans or psychiatrists.

Dr. Bagley in this letter (2) disputes such a simple conclusion. He writes that if this were so there would have been, in the 1960s, an increase in 'failed suicides' by gas poisoning. He produces figures from a casualty department in Southern England of the ratios of failed gas poisoning to other forms of failed suicide. These ratios consistently fell—from 21.1 per cent in 1960 to 4.8 per cent in 1970. On the strength of these falling ratios he rather cryptically argues that 'the fall in the rate of completed suicide has been largely due to factors other than the detoxification of gas'. But:

(i) Gas made from coal contains carbon monoxide, but natural gas and the gas made from the new processes of 'oil gasification' do not. As new plant replaces old, domestic gas contains progressively less carbon monoxide. An extensive 'gas grid' connects the regions of Britain through which the gas is pumped according to demand. Thus in any one region gas may be lethal one day and harmless the next. Dr. Bagley is wrong in assuming that if domestic gas fails to kill a suicide attempter it will leave him ill instead. Inhalation of the new gas may leave him as healthy, if not as hearty, as ever.

(ii) The iatrogenic pastime of taking overdoses is

increasing in Britain at the rate of about 10 per cent each year. Taking overdoses is much more common than 'sticking one's head in the gas oven', a method preferred only by the smaller number of the more serious-minded suicide attempters. The pill swallowers steadily and increasingly overshadow the gas inhalers, and this no doubt accounts for Dr. Bagley's falling ratios.

(iii) Dr. Bagley is of course, perfectly right when he says that 'the similarity of "two curves" on a graph does not demonstrate a causal trend.' But even in statistical analysis there may well be room for common sense.

When the annual rates of Britain's total suicides, suicides by gas poisoning, accidental deaths by gas poisoning, and the average yearly content of carbon monoxide in domestic gas are plotted alongside each other on a graph, all four curves are found to be similar. They took a deep plunge in 1963 and have continued to go downwards. During the years 1963-1970 total suicides fell from 5,639 to 3,940; suicide deaths from gas poisoning from 2,353 to 511; accidental gas poisoning from 1,246 to 270 (3); and the average annual carbon monoxide content of domestic gas from 11.6 per cent to 2.2 per cent.

It is, of course, perfectly legitimate to argue that there is no causal connection between the similarity of these curves—to argue, that the reduction in deaths due to accidental gas poisoning is not due to the reduction of carbon monoxide, but is, for instance, due to the improved psychiatric services. Gas taps are left on less often because the memories of the elderly have been improved, and gas pipes no longer leak because the workers, having been made content by antidepressives, maintain gas pipes in better repair. Personally I prefer the simpler explanation that deaths from accidental gas poisoning and from suicidal gas poisoning have both fallen because domestic gas, when inhaled, is now often no longer harmful.

The point of this letter is not to be beastly to the Samaritans or to psychiatrists. If there were evidence that suicide prevention programmes reduced the suicide rate it would certainly become mandatory upon us to establish more of these programmes. As yet there is no evidence to suggest that they in any way reduce the incidence of either suicide (4) or even of attempted suicide (5). This being so, it seems to me that we should spend our available money not on suicide prevention programmes but on other psychiatric services which may prove more useful.

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FATAL SELF-INJURIOUS BEHAVIOUR  
—A PRELIMINARY COMMUNICATION

DEAR SIR,

A recent publication by Stevens (1973) has emphasized the high risk of suicide in community-centred Day-Hospital/Industrial Rehabilitation Units for psychotic patients. On the other hand, to date, a review of relevant literature concerning the use of token-economy programmes with chronic psychiatric patients has failed to provide evidence of any report concerning the occurrence of suicide during or after programme participation, though there is evidence to suggest that during programme participation some patients are likely to threaten and show self-injurious behaviour (Schaefer and Martin, 1969). The purpose of this communication is to highlight the possible risk of fatal self-injurious behaviour in programmes dealing with chronic psychiatric patients and conducted in controlled prosthetic social-learning environments.

In rehabilitation studies using token reinforcement conducted by this correspondent, four patients out of a total of 56 made suicidal attempts which proved fatal. These four patients (three males and one female), all suffered from psychotic illnesses of long duration, and they committed suicide under the following circumstances: (a) one patient—over three months after successful programme completion, i.e. during the follow-up period, while resident in a half-way hostel and in sheltered employment; (b) one patient—over three months after successful programme completion, i.e. during the follow-up period and while in open employment for over two months, though still resident in hospital; (c) one patient—while out of hospital on a town visit; and (d) one patient—while out of hospital on a home visit accompanied by relatives. No environmental cause could be demonstrated in these suicides.

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POSSIBLE SIDE EFFECTS OF  
FLUPHENTHIXOL DECANOATE

DEAR SIR,

Many patients have benefited considerably from the regular medication provided by the depot neuroleptic drugs, and many of us have found a great use for them—particularly fluphenazine enanthate (Moditen), fluphenazine decanoate (Modecate) and more recently fluphenthixol decanoate (Depixol).

Side effects (apart from extrapyramidal effects) seem to have been relatively rare, but recently two patients of mine have shown quite remarkable weight increases whilst on Depixol. In one case, a female patient of 38, weight increase was so dramatic that she became facially unrecognisable and her ordinary clothes could not be worn. She put up with the weight increase for several months, but then refused further injections. Her weight has reduced and her figure and facial outline have returned to normal since injections were stopped and she reverted to trifluoperazine (Stelazine) by mouth. The other patient is a young man, aged 21, who after a severe psychotic episode and six months in hospital has done well. His psychosis is not now evident, he is working and is apparently doing well, but his weight increase (not apparently embarrassing to him!) is enormous.

We have found Depixol a useful drug, and I would be glad to know if any other clinicians have noted any similar effects. At least one observer (Gottfries) has noted weight reduction in a number of cases. I am not, at present able to offer any explanation how or why the weight increase occurs.

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THE USE OF DISULFIRAM  
IMPLANTATION IN ALCOHOLISM

DEAR SIR,

A representative of the American company that manufactures disulfiram has informed me that the drug is absorbed into the blood stream via the lacteals of the gut and that absorption does not occur by other routes. Documentation for this assertion, however, was lacking, and I was interested in the article by Malcolm and Madden (*Journal*, July 1973,