

of the majority of the population and hence the people for whom doctors had medical responsibility. When military policies involve deliberate and premeditated mass destruction of an enemy population, however, and when both sides share such capability the issues are radically altered. Any doctor who advocates or supports such use of weapons must assume some responsibility not only for the possible death and suffering of many civilians in other nation states but also for the destruction of his own patients and their resources. It should also perhaps be taken into account that the forms of death and morbidity for which he is potentially responsible are probably as appalling as it is possible to imagine, and that many of those on whom such suffering is to be inflicted, particularly children, cannot be held in any way responsible for the course of events.

Those who accept the arguments for deterrence clearly indicate their willingness to use the weapons involved: if this were not the case the deterrent effect would automatically be invalidated. Such support for use of weapons of mass destruction is by itself worthy of ethical evaluation but must be balanced by a proper consideration of the deterrence position. Since they are unlikely to wish to be responsible for destructive effects of weapons far exceeding any in history, those who support deterrence must rely on its effectiveness. If it were guaranteed to be entirely effective in preventing war, deterrent use of weapons would be of major value to mankind and probably worthy of medical support, provided that adverse effects due to the production and deployment of the weapons could be avoided.

In the event that a doctor believes that there is a significant risk that deterrence by implied mass destruction will cease to be effective, however, his continued support for such a policy directly contravenes the principles of preservation of life and prevention of suffering represented in the Hippocratic Oath and more recent statements of medical ethics.<sup>1</sup>

Those who believe that deterrence will be entirely effective are almost undoubtedly in a minority at the present time, and may have failed to note both the alternative judgments of numerous informed opinions and the possible implications of some recent military developments. Since reliable data on which to base an adequate risk analysis concerning the course of future events are unavailable, a significant likelihood that use of weapons of mass destruction will occur cannot be excluded. It would therefore appear to be unethical to support deterrent use of such weapons.

A further contingency is that, as argued recently by Zuckermann,<sup>2,3</sup> deterrence between the major powers with nuclear weapons was implemented early in the decade 1960-70, and that some military developments since that time may have increased the likelihood that deterrence will break down.

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<sup>1</sup> *The Handbook of Medical Ethics*. London: BMA, 1981.  
<sup>2</sup> Zuckermann S. *Proc Am Phil Soc* 1980;124:1-14.  
<sup>3</sup> Zuckermann S. *Nuclear Illusion and Reality*. London: Collins, 1982.

### Cigarette consumption and biochemical measures of smoke intake

SIR,—In criticising (26 June, p 1951) the statistical studies by Mr C J Vesey and others (22 May, p 1516) on carboxyhaemoglobin and plasma thiocyanate in smokers, I was not arguing that studies of blood levels of tobacco or dietary components are of little predictive value for epidemiologists. Indeed, I have for some time advocated the wider use of biochemical measurements to help predict the future onset rate of whatever disease is of interest, and some of the work in which I am

currently engaged is based entirely on this. But, to be of good predictive value, biochemical measures must be reasonably reproducible. To pursue the example of blood cholesterol and other lipids introduced by Dr Michael Russell in the replies from him and from his colleagues (14 August, p 507) to my criticisms, preliminary analyses of a few dozen individuals whose blood was drawn both in 1972 and in 1976 have shown that plasma cholesterol was reasonably reproducible (people who were high in 1972 tending to be high in 1976 as well), but that the plasma free fatty-acid profile was not (indeed, the 1972 values were scarcely correlated at all with the 1976 values), presumably because the recent intake of fatty acids is so variable from day to day.

This suggests that although a single plasma cholesterol can usefully characterise an individual, a single measurement of an individual's plasma free fatty acid profile cannot. Thus, although dietary fatty acid intake directly affects plasma fatty acid profile, correlation of the results of a 1972 survey of such profiles with subsequent disease onset rates is unlikely to be of much help to epidemiologists who want to study the health effects of fatty acid intake. This difficulty cannot be circumvented by discovering that in the 1972 survey one fatty acid is correlated with another: what chiefly matters for prediction of chronic disease is whether the 1972 values characterise an individual, and the critical test of this is the extent to which the 1972 and the 1976 values are correlated with each other.

In the light of such considerations, I objected to the claim, made without any proper discussion of long-term reproducibility, that carboxyhaemoglobin and thiocyanate had been shown to be: "Superior to self-reported cigarette consumption as an index of exposure to tobacco smoke," and I cannot agree with the authors' replies that: "The issue of stability over time is a secondary one," nor that their statistical analysis is "valid," if by "valid" they mean that it demonstrates their stated conclusions.

I hope their conclusions are true, of course, because I too would greatly like some measures of tobacco exposure to be established as being as or more reliable than self-reported consumption, and I would strongly support their final suggestion that biochemical and questionnaire information could be complementary. Such complementarity may, for example, be especially important for lung cancer, since this is a disease that originates chiefly from the cells of the *main* airways, and the determinants of exactly what is deposited in them as the cigarette smoke rushes past are so complicated that, among heavy smokers, those who describe themselves as "inhalers" may even be at lesser risk than smokers of similar amounts who because they inhale more slowly do not describe themselves as "inhalers."<sup>1</sup>

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<sup>1</sup> Doll R, Peto R. *Br Med J* 1976;ii:1525-6.

### Nicotine chewing-gum

SIR,—The excellent results of the double-blind trial of nicotine chewing-gum of Mr M J Jarvis and colleagues (21 August, p 537) should lead to its more widespread use especially for

intervention in "at risk" smokers by general practitioners. My ongoing study shows the rewards of a stop-smoking clinic in general practice and reaffirms that patients should be encouraged to use the gum over a longer period of time.

Following earlier suggestions made by the London group, attention was paid to careful instruction on chewing technique, completion of smoking diaries, the offer of a substitute for hand inactivity, and, especially, regular follow up. A hundred patients, whose cigarette consumption exceeded 15/day and with either a pre-existing or a predisposition to a smoking-related disease were given a supply of Nicorette provided they expressed a sincere desire to give up smoking. After two, four, six, 12, and 26 weeks the number abstinent were 37, 51, 38, 29, and 28 respectively.

A full six months free of all cigarettes was verified by expired-air carbon monoxide measurement in 28. Of these long-term successes, 24 had used Nicorette for a minimum of three months. Preliminary findings also suggest that a six-day gum-acclimatisation programme before complete smoking withdrawal improves the acceptability and efficacy of the product. Sixty-two per cent of patients who were successful received this approach. A control group were advised to substitute all cigarettes with gum from the outset.

Assessment is continuing up to 12 months and will be reported in due course.

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SIR,—In concluding that nicotine chewing-gum is the first treatment for smokers that "has a specific effect over and above that attributable to an attention-placebo response," Mr M J Jarvis and his colleagues (21 August, p 537) showed an extraordinary neglect of the recent publications on psychological treatment. When several behavioural treatment methods are combined into "packages," abstinence rates of 33-76% have been reported at six months' follow-up<sup>1-9</sup> and 32-46%<sup>10,11</sup> at one year. These results are clearly superior to the frequently cited attention-placebo effects. There is more than one effective way of helping people to stop smoking.

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<sup>1</sup> Best JA, Bass F, Owen LE. *Can J Public Health* 1977;68:469-73.  
<sup>2</sup> Chapman RF, Smith JW, Layden TA. *Behav Res Ther* 1971;9:255-64.  
<sup>3</sup> Delahunt J, Curran JP. *J Consult Clin Psychol* 1976;44:1002-7.  
<sup>4</sup> Elliott CH, Denney DR. *J Consult Clin Psychol* 1978;46:1330-9.  
<sup>5</sup> Hamilton SB, Bornstein PH. *J Consult Clin Psychol* 1979;47:598-600.  
<sup>6</sup> Lando HA. *J Consult Clin Psychol* 1977;45:361-6.  
<sup>7</sup> Lando HA, McCullough JA. *J Consult Clin Psychol* 1978;46:1583-5.  
<sup>8</sup> Paxton R. *Behav Res Ther* 1980;18:45-50.  
<sup>9</sup> Paxton R. *Behav Res Ther* 1981;19:117-23.  
<sup>10</sup> Lando HA. *Addic Behav* 1981;6:123-33.  
<sup>11</sup> Pomerleau O, Adkins D, Pertschuk M. *Addict Behav* 1978;3:65-70.

SIR,—The report by Mr M J Jarvis and others (21 August, p 537) of a carefully planned trial at the Maudsley Hospital might lead practitioners to conclude that the most effective help they can give to patients wishing to stop smoking is to prescribe nicotine chewing gum. While any adjunct which leads to an abstinence