marijuana users than smaller Dutch communities. US rates are basically identical to those in Amsterdam and Utrecht, and higher than those Tilburg". We then note that "unfortunately, many of the available contrasts between The Netherlands and her European neighbours suffer from the same weakness, comparing rates for an entire nation as a whole to those in the largest city of another nation". And we state that the contrasts where the Dutch rates are higher are mostly "attributable to comparisons limited to Amsterdam". We conclude that "Dutch rates are somewhat lower than those of the USA but somewhat higher than those of some, but not all, of its neighbours. Amsterdam's level of marijuana use is comparable to that of the USA".

Abraham *et al* further complain that our comparisons were "arbitrarily selected". In fact, our 1997 *Science* article included every Dutch cannabis prevalence rate for which we could find a reasonable international contrast matched by year, age range and type of prevalence. Our recent update in the *British Journal of Psychiatry* added another 13 comparisons. We welcome further comparisons but a fair reading of both papers makes it clear that we attempted to be exhaustive, given the limited availability of Dutch drug prevalence data in English-language sources. (Indeed, where possible we had Dutch-language sources translated.) In any case, we emphasise that we drew no policy conclusions from these static cross-sectional comparisons. That portion of our article was an attempt to correct grossly misleading comparisons of Dutch and US rates in the American media.

We are taken to task for using the Dutch school survey data from the Trimbos Institute, rather than data from Statistics Netherlands or the CEDRO Amsterdam survey. As noted below, we did in fact report CEDRO estimates. But the 1990s Amsterdam trends mentioned by Abraham *et al* are not relevant to our commercialisation thesis; as we explained in our article, the dramatic growth in cannabis commercialisation in Amsterdam occurred between 1980 and 1988 and almost every Western nation saw increases in cannabis use after 1992 for reasons apparently unrelated to drug policy.

We are delighted to learn of the national Statistics Netherlands estimates, which as far as we can tell have not been cited previously in the English-language literature – although the search engine on their website produces no statistics for "drugs", "drug", "cannabis" or "marijuana". But now we are puzzled as to why a 1997 paper by Marieke Langemeijer announcing CEDRO's own national survey stated that "The implementation of the national survey means that finally, The Netherlands will have a decent source of data that serves multiple purposes among which the basic information for health care, prevention, education and drug policy. Hopefully, it is the beginning of a high quality drug research tradition". Similarly, a CEDRO press release of 14 April 1998 stated that "figures for the entire country will soon no longer have to be based on local surveys since a national study on drug use in The Netherlands is currently being carried out by CEDRO". Moreover, neither the CEDRO nor the Trimbos researchers mention these data in their English-language monographs on Dutch drug use trends.

Our Fig. 1 showed that during the 1984–1992 period the Trimbos lifetime prevalence estimates rose even more steeply for the age 16–17 group than for the age 18–20 group. This clearly undermines the concern raised by Abraham *et al* about a selection bias involving older students, but at any rate, that criticism misses the point. Sampling biases of the Trimbos school survey do not preclude its use for studying trends over time. Moreover, our trend analysis compared it to age 18–20 trends from the US Monitoring the Future school survey. The Trimbos researchers state that their survey was designed to permit comparisons to that US survey (see Plomp *et al*, 1991: 11).

Abraham *et al* complain that we averaged non-comparable estimates, but fail to mention that we grouped our estimates so that 'city *v.* nation' averages and 'nation *v.* nation' averages were presented separately. We think our averaging was well within contemporary standards of meta-analysis, but no matter – we presented the raw data so readers could decide for themselves. At any rate, no conclusions of our work hinged on these averages – indeed, we did not even include them in our presentation of these data in our forthcoming book, *Drug War Heresies* (MacCoun & Reuter, 2001*b*).

Abraham *et al* suggest that our alleged inattention to the geographical issue undermined our inferences about the effects of commercialisation. On the contrary, the fact that cannabis prevalence is higher in Amsterdam is quite consistent with our hypothesis. During the 1980s, when we contend the commercialisation effect occurred, various estimates suggest that over a quarter of all Dutch cannabis coffee shops were in Amsterdam, yet Amsterdam accounted for only about 5% of the total Dutch population. As late as 1997, Abraham *et al* (1999) reported that last-year users from the highest-density Dutch addresses were more likely to cite coffee shops as their cannabis source than were users from low-density Dutch addresses.

As we stated in the article, the evidence for our commercialisation hypothesis was indirect and at best purely correlational, though we noted that it is consistent with evidence on gambling, tobacco and alcohol marketing. Moreover, the quasi-legal status of the Dutch system, which tends to keep prices high, almost surely understates the likely commercialisation effects of full legalisation. Given weak data, our inferences may well be wrong, but we think the comments of Abraham *et al* shed little light on that question.

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Prognosis of depression and generalised anxiety in primary care

Van den Brink *et al* (2001) studied general practitioners' (GPs') prognostic predictions for depression and general anxiety. They found the prognosis was in general more pessimistic than the observed course and failed to attain maximal performance in

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comparison with a statistical model based on baseline variables. I would like to express three concerns about the technical details of this article.

First, the kappas they report are Cohen's kappas whereby the disagreement between "full recovery within 6 months" and "partial recovery" is penalised equally to disagreement between "full recovery within 6 months" and "no recovery". Clinically, however, the former is apparently less grave than the latter. More appropriate statistics would be weighted kappas, which are 0.31 (95% CI 0.15–0.46) for GP prognosis for depression, 0.35 (95% CI 0.16–0.54) for GP prognosis for anxiety, 0.56 (95% CI 0.43–0.70) for model prognosis for depression and 0.51 (95% CI 0.33–0.69) for model prognosis for anxiety. These figures are appreciably larger than those originally reported.

Moreover, regardless of whether we use Cohen's kappas or weighted kappas, the authors did not examine whether the GP prediction is indeed statistically significantly worse than the model's. The reported 95% confidence intervals overlap, and we do not know whether the clinicians are actually performing worse than the maximally attainable model.

Third, as the authors rightly note in the Discussion, their way of using the total sample to construct a predictive model may have 'overfitted' the model to the data and produced artificially inflated agreement. A more ideal way may have been the 'leaving-one-out method' (Lachenbruch, 1975), in which analysts would repeatedly build a model based on a sample minus one observation and examine whether each model could predict the one excluded observation.

In this connection it may be worthwhile to point out that the comparison between human performance and that of a statistical model is a theme repeatedly found in clinical psychology (Meehl, 1954; Goldberg, 1970). These studies conclude that, because of the inevitable random error in human judgement, the latter almost always outperforms the former. It will, therefore, be most interesting to see how, in the authors' next round of proposed investigation, clinicians can improve their performance if they are given feedback on prognostic factors.

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The stigma of suicide

The Royal College of Psychiatrists is leading a campaign to reduce the stigma attached to mental illness. Stigmatisation of suicide has very deep roots in our collective thinking and judgement. Suicide was tolerated by the Greeks and Romans (Alvarez, 1990), but Aristotle argued that suicide weakens the economy and upsets the gods, and in so-doing he initiated stigmatisation of the act. Hinduism and Buddhism, among other Eastern religions, have not had a traditionally negative view of suicide. In the Judaeo-Christian tradition, stigma against suicide is not evident until the fourth century; the Bible does not condemn suicide (Barraclough, 1992), but St Augustine considered suicide as unacceptable within Christian values (Pritchard, 1996). Gradually, the stigma against suicide intensified in Europe and became a great sin, shame and eventually a crime. A number of philosophers and writers including William Shakespeare sought to encourage a more understanding and compassionate view but this movement had little impact before Durkheim's studies made clear the social rather than moral origins of suicide (Retterstol, 1993). Although suicide and attempted suicide were decriminalised in 1961 (Levine & Pyke, 1999), we have practised since within a culture of ambivalence wherein stigma is neither high nor totally eliminated. Indeed, the multicultural/multifaith dimension within society and its thinking has complicated matters considerably.

The stigma surrounding suicide remains just high enough to discourage people – especially the elderly – from talking about their suicidal thoughts. Some people feel that they might be labelled as weak, lacking faith, coming from bad families or indeed 'mad' if they were to declare their suicidal thoughts. This does not help when we are trying to detect early signs of suicide or reaching out to help victims of despair.

Any approach to prevent suicide should include the removal of blame and stigmatisation of that individual and his or her family. One would hope that all teachers and professionals from the different faiths will take into account this insight into the condition. Scientific approaches and spiritual approaches can work together in order to eliminate this kind of stigma and to make people more comfortable in trying to seek help in their moments of despair.

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Who is politicising psychiatry in China?

Having researched on *qigong*-related mental health problems in China, I am upset to read the statement of Lyons (2001), based indirectly on estimates from Amnesty International and a letter to the *Lancet*, that "Soviet-style psychiatry is alive and well in the People's Republic".

In China, resurgence of interest in *qigong* ('exercise of vital energy') started as early as 1980, when Chinese people were recovering from the social chaos brought about by the Cultural Revolution (1966–1976). It is worth noting that *qigong*-induced mental disorder was reported by Chinese psychiatrists long before recent accusations that psychiatry in China is used to imprison people who practise a specific kind of *qigong* known as *falungong*. There have been a sizeable number of controlled phenomenological, treatment and outcome studies published in the past two decades that testify that *qigong*-related mental disorders do not fall into a specific disease category recognised in the modern classifications (see Lee, 1996, for a brief review). In my own field studies, I interviewed people who suffered from acute psychosis induced by the inappropriate practice of *qigong* in several regions of China as well as in Hong Kong. The condition is intriguing but real, and is deserving of

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