

the comparisons should also be matched for type of geographical area. Comparing Flatbush to New York City would have limited relevance, even if correctly matched for age group etc. To compare cannabis use in New York City with somewhere else, one would have to look for a similar area, both in address density and in variation of population and lifestyle. Amsterdam could be compared to San Francisco, because these cities are very similar in size and cultural characteristics, but not to New York City, a metropolis over 10 times as large, or to the USA as a whole. Such comparisons are wrong and without meaning.

We agree with MacCoun & Reuter that decriminalising cannabis merits serious consideration. But we disagree with their observations on “commercialisation”. In this letter we will turn most of our attention to the epidemiological material the authors base their conclusions on.

MacCoun & Reuter focus entirely on cannabis prevalence (assuming that a lower prevalence is better than a higher one) without considering whether this is the most relevant issue; the social and legal consequences of the use of cannabis could be considered at least as important. But given that a comparison of prevalence figures is a useful first step towards informed comparisons, we propose that the conclusion of MacCoun & Reuter that the commercial type of Dutch coffee shop system increases cannabis prevalence is based on statistically ill-founded comparisons of Dutch prevalence figures with those in other Western nations.

MacCoun & Reuter compare cannabis prevalence figures of a Dutch city or nationwide with prevalence figures from the USA or other Western nations. Differences are summed and averaged, resulting in (among others) a mean Dutch–US difference and a mean Dutch–European difference. This is statistically erroneous for reasons we supply below.

First, in 16 cases a Dutch city is compared with a nation (UK, USA, Sweden, etc). By doing this, MacCoun & Reuter presuppose that prevalence rates are the same all over The Netherlands. This is incorrect: in our 1997 national survey we found large geographical differences between locations with different address densities, a measure of urbanisation. For example, lifetime prevalence of cannabis use in Amsterdam (address density >3000/km²) was 36.7%, the average national prevalence was 15.6% and average prevalence in rural

areas (address density <500/km²) 10.5%. Correct international comparisons can be made, but have to be between comparable geographical or urban areas. Despite the sensitivity MacCoun & Reuter demand for correct comparisons, nationwide US figures (260 million inhabitants, including major metropolitan areas) are compared with the small Dutch city of Tilburg (165 000 inhabitants).

Second, comparisons are arbitrarily selected. For example, replacing prevalence figures for Amsterdam (the city most often chosen in MacCoun & Reuter’s comparisons) with figures for Rotterdam changes the outcomes of the average difference in cannabis prevalence between the Dutch and other systems.

Third, MacCoun & Reuter state that the lifetime prevalence of cannabis in The Netherlands has increased consistently and sharply in the age group 18–20, stating: “the increases . . . provide the strongest evidence that the Dutch regime might have increased cannabis use among the young”. This finding is based on school survey data (lifetime cannabis use in 1984: 15%, in 1996: 44%). Again, the choice of figures that are compared is crucial. Moreover, the Dutch school survey data of the age group 18–20 is an extremely biased selection of this age cohort. The school survey takes place in some primary schools, but mostly in secondary educational institutions, that are designed for 12- to 18-year-olds. However, some persons remain much longer in this system for a variety of reasons but they are atypical for the age group in general. They bias the school survey estimate for this age group.

More suitable figures are given by Statistics Netherlands (CBS) and by the Centre for Drug Research (CEDRO), and reflect a much more moderate increase or no increase at all. Statistics Netherlands measures cannabis use prevalence in a national representative sample. For the age group 18–20 lifetime cannabis use remains at the same level over time (17% in 1989, 19% in 1990, 18% in 1991, 20% in 1992 and 14% in 1993; data from D. J. B.). Using CEDRO data, we are able to produce trend data for the city of Amsterdam for the same age group 18–20: in 1987 lifetime cannabis use was 34%, rising to 44% in 1997. This is a rather modest increase in cannabis use, very similar to the slowly rising consumption levels of other European and US measurements. The 18- to 20-year-olds in the samples from Amsterdam

are randomly selected from the citizen registry, and represent the age group much better than 18- to 20-year-olds still attending school. The ‘dramatic’ increase that MacCoun & Reuter hypothesise in Dutch cannabis use in the period 1984–1996 (as reflected in the same age group) does not exist.

Finally, the most serious flaw develops by creating a series of ‘absolute’ differences between Dutch and other data, and averaging them. MacCoun & Reuter create the suggestion that too large or too small differences will be averaged and thereby, in the form of an ‘average’ difference, become more reliable. The opposite is true. If pears can not be compared to apples, their ‘differences’ can not be used for normal mathematical computations.

Declaration of interest

None. The views expressed by D. J. B. are the author’s own and do not necessarily reflect the policies of CBS.

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Authors’ reply: We thank Abraham *et al* for their comments, but they have misrepresented our paper, and we find their arguments either misleading or unconvincing.

Abraham *et al* complain that 16 of our 28 statistical comparisons contrast a Dutch city with a national estimate from the USA or another nation, suggesting that we “presuppose that prevalence rates are the same all over The Netherlands”. We made no such presupposition. As we clearly stated in our article: “American surveys indicate little difference, on average, between large metropolitan samples and the USA as a whole . . . but the estimates in Table 1 suggest that Amsterdam has a higher fraction of

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, 2001b).

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