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How to read a paper on the short-term impairing effects of cannabis: A selective and critical review of the literature

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Abstract

The prosecution of cannabis-presence driving offences (in the absence of any behavioural evidence of impairment) is ultimately based on the assumption that there is a tight causal relationship between positive toxicology for cannabis and impairment. The main purpose of this review is to examine the evidence for that relationship. We show that most experimental studies have failed to elicit statistically-significant cannabis-induced impairments for many of their possible outcomes. And many studies failed to demonstrate any impairment at all in *regular* users of cannabis (because of the development of tolerance). We argue that selective reporting by researchers, editors and the media has created the false impression that the evidence for cannabis-induced impairment is strong and consistent. Human beings are 'over-engineered' for the psychomotor skills required to drive safely. A benchmark level of cannabis-induced impairment is therefore required to distinguish unproblematic from 'real-world' impairment. The conventional benchmarks of statistical significance, effect size and BAC-equivalence are shown to be inadequate. However, a benchmark in terms of 30 years of normal cognitive aging has good face validity. The recent use of cannabis is indicated toxicologically by the presence of delta-9-tetrahydrocannabinol (THC) in blood or oral fluid. Evidence is provided that most THC-positive drivers are not impaired, and certainly not meaningfully impaired. It follows that the justice of stand-alone cannabis-presence driving offences must be questioned.

Keywords

review, cannabis, performance, impairment, driving, road safety

Types of drug-driving enforcement regime, and the purpose of this review

In some jurisdictions, the police are allowed to stop drivers for impairment testing (toxicological or behavioural) without any prior behavioural indication that they might be impaired. For example, in Australia, extensive programs of *random* roadside drug testing (RDT) are conducted by the police (Moxham-Hall and Hughes, 2020). In other jurisdictions, some behavioural indication of impairment (such as weaving) is required before a driver can be stopped. For example, in the U.S., under the Fourth Amendment, a 'reasonable suspicion' that the driver is impaired must be formed before the driver can be pulled over. That is followed by a preliminary assessment that a drug is the 'probable cause' of the impaired driving (based, e.g., on slurred speech), before any evidentiary-level behavioural or toxicological assessments can be undertaken (DuPont et al., 2012).

The legal requirements for detaining a potential drug-driver (as discussed above) can be quite different from the requirements for a drug-driving offence to have been committed. In some jurisdictions (e.g. in some U.S States and in Australia), the drug-driving offence does not require any evidence of impairment: all that is required is that the driver test positive to a proscribed drug in a body fluid. Such toxicologically-defined offences can be described as '*per se*' or 'presence' offences. In other jurisdictions (e.g. in some *other* U.S. States) behavioral evidence of impairment is required to establish that a drug-driving offence has been committed, and any toxicological information plays only a supporting role. (Lacey et al., 2010 surveyed the drug-driving laws in the U.S., and reported that 15 of the 50 States had *per se* drug-driving offences, while the other 35 had only impairment-based offences.)

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Where the offence is defined toxicologically, it may be sufficient for any trace of the proscribed drug (above the limit of detection of the testing equipment) to be present ('zero-tolerance'), or the concentration of the drug may have to be above a threshold level that purportedly indicates that the driver is impaired. (According to Lacey et al. (2010), only 3 of the 15 U.S. States with *per se* drug-driving offences had above-zero *per se* thresholds, while the other 12 had zero-tolerance *per se* thresholds). Where there *is* an above-zero threshold level of a drug, it is possible that two or more threshold values have been set that purportedly identify different degrees of impairment, and are therefore used to determine different levels of penalty. That is currently the situation, for example, in Norway (Vindenes et al., 2012).

In summary, there are four main types of drug-driving enforcement regime, as defined by the two main types of justification for a traffic stop (random vs. the appearance of impaired driving) in combination with the two main types of evidence that an offence has been committed (*per se* drug presence as determined toxicologically vs. behavioural evidence of impairment). When describing the overall enforcement regime, we use the term 'stand-alone *per se*' to describe the situation where no behavioural evidence of impairment is required to either apprehend the driver or to charge him or her with an offence. In most jurisdictions where there are stand-alone *per se* drug-driving enforcement regimes, they are ostensibly carried out as road-safety initiatives, and not simply as a means of prosecuting the so-called 'War on Drugs'. As such, the initiative is predicated on the assumption that there is a tight relationship between positive toxicology for a proscribed drug and being impaired by that drug. The purpose of this review is to examine the evidence for the strength of that relationship in relation to cannabis.

Indicative experimental evidence for high cannabis-crash risks is contradicted by evidence from the most rigorous epidemiological studies

A number of recent reviews of the literature on the risks of driving after using cannabis have commented that the findings from experimental (laboratory, simulator and on-road) and epidemiological (culpability and case-control) studies seem discrepant. Four examples are provided. In their 2019 review of *Methodologies for establishing the relationship between alcohol/drug use and driving impairment*, Gjerde et al. (2019) observed that 'The results from different types of studies show apparently different findings regarding the risk posed by cannabis in road traffic: experimental studies … found significant impairment of mental and psychomotor functions, whereas epidemiological studies found a fairly low … increase in crash risk' (p. 153). In his 2019 report to the U.S. Congress on *Marijuana use and highway safety*, Peterman noted that 'Although laboratory studies have shown that marijuana consumption can affect a person's response times and motor performance, studies of the impact of marijuana consumption on a driver's risk of being involved in a crash have produced conflicting results, with some studies finding little or no increased risk of a crash' (Summary page). In his 2015 legal treatise on *Medical or recreational marijuana and drugged driving*, Larkin commented that 'Interestingly, there appears to be a disagreement between the laboratory cognitive studies, which largely show that marijuana impairs the skills necessary for driving safely, and … the epidemiological studies, which do not always point in that direction' (pp. 474–475). And in their 2020 chapter on *Cannabis-impaired driving: Evidence and the role of toxicology testing*, Wood and Dupont observed that 'Laboratory experiments prove that cannabis adversely affects the psychomotor skills and cognitive functions required for safe driving in both occasional and chronic cannabis users. Whereas the laboratory evidence is strong and consistent, the epidemiological evidence is far less so' (p. 509).

While there is widespread agreement that the moderate cannabis-crash risks implied by the findings from experimental studies are often not manifested in the epidemiological findings, there is no agreement as to the reason for the discrepancy. Gjerde et al. (2019) believe that the epidemiological studies routinely underestimate the crash risks, while the experimental evidence, which can be interpreted as pointing to higher risks, is more trustworthy. They identify one of the main problems with the epidemiological evidence as the fact that, for many of the studies where a driver is defined as cannabis-positive on the basis of toxicological evidence for the presence of delta-9-tetrahydrocannabinol (THC), the cannabis-impairment window (three or four hours after using cannabis) is much shorter than the THC-positive window (many hours). Consequently, many of the drivers who are classified as THC-positive are not actually impaired - which has the effect of deflating the cannabis-crash risk. In their own words, in relation to some of the epidemiological studies, Gjerde et al. say that 'It is likely that only a small proportion of those who were categorised as cannabis-exposed were actually intoxicated or 'high', thus underestimating the risk posed by driving while intoxicated by cannabis' (p. 152).

Although Wood and Dupont (2020) agree with Gjerde et al. (2019) that the experimental studies are more valid than many of the epidemiological studies, they do not focus on the hypothesised mis-match between the cannabis-impairment- and THC-positive- windows. Instead, they propose a number of other reasons why the epidemiological studies report unrealistically low cannabis-crash risks. For example, in relation to the rigorous case-

control study that was sponsored by the U.S. National Highway Transport Safety Administration (NHTSA), and conducted by Lacey et al. (2016), which reported a cannabis-crash odds ratio (OR) of 1.0 (indicating no increase in crash risk), Gjerde et al. identified four reasons why the risk was underestimated. Briefly, the reasons are: (1) the voluntary participation of the case and control drivers; (2) the inclusion of low-severity crashes; (3) the fact that not all of the crashed drivers were culpable for their crashes; and (4) the fact that the study took place in a region with low rates of illicit drug use. None of those reasons is convincing: (1) voluntary participation is only a problem when it applies differentially to the cases and controls, which was not the situation for this study; (2) the use of alcohol and drugs can cause crashes at all levels of severity, including those where nobody is injured; (3) it is an inherent feature of all case-control studies of road crashes that not all of the crashed drivers are culpable for their crash; and (4) the calculation of odds ratios takes drug prevalences into account: low drug prevalences are not problematic.

In contrast with the views above, Peterman (2019) and Larkin (2015) believe that the epidemiological evidence for low cannabis-crash risks is trustworthy, while the indicative experimental evidence for higher risks is deficient. Peterman makes the general point that 'The levels of impairment that can be identified in laboratory settings may not have a significant impact in real world settings, where many variables affect the likelihood of a crash occurring' (Summary page). Larkin agrees, and provides one specific reason for the failure of the identified impairments to 'have a significant impact in real-world settings': 'Unlike alcohol users, who *underestimate* the effect of alcohol on driving skills and engage in risky driving behaviour, such as driving faster and more aggressively, marijuana users *overestimate* the drug's effect and compensate by driving more slowly, passing less frequently, and spacing their cars further from other vehicles by increasing their following distance' (p. 476; emphases added).

The main point made in this section has been well expressed by Ramaekers (2018: 1433): 'Epidemiological findings on the role of THC in vehicle crashes show that cannabis use among drivers is associated with a moderate (about 1.2- to 2.0-fold) increase in crash risk, less of an effect than might have been predicted from experimental research'. The degree to which 'Ramaekers' dilemma' is considered problematic by a researcher will, of course, depend on the researcher's understanding of the crash risk posed by the use of cannabis. From our perspective the dilemma is very stark, because we believe that the crash risk is very low. We have recently completed a systematic review with meta-analyses of the most rigorous epidemiological studies of the effect of the acute use of cannabis on the risk of being involved in a road crash (White and Burns, 2021), where we were unable to reject the null-hypothesis that there was *no* increase in the risk of crashing (OR = 1.0). Our finding is compatible with that of two other recent cannabis-crash reviews, where the null-hypothesis was unable to be rejected (Hostiuc et al., 2018; and Rogeberg, 2019). So, with respect to the apparent conflict between the epidemiological evidence from recent reviews for low cannabis-crash risks and the indicative experimental evidence for higher risks, we strongly favour the epidemiological evidence. While we agree with Larkin (2015) that cautious driving by cannabis-positive drivers helps them to avoid crashes, we believe that the discrepancy between the indicative experimental findings and the epidemiological findings is mainly a consequence of the flawed reporting and interpretation of the experimental findings, as described in this critical, selective review. We understand that not all readers of our review will share our perspective. For those who believe that the crash risk from cannabis is higher than, say, a 50% increase, there may be no dilemma to resolve.

Cannabis-induced impairments are sometimes absent and usually weak

Wood and Dupont (2020: 509) say that the experimental evidence for impairment by cannabis is 'strong and consistent'. The truth of that claim is explored in this section.

Regular users of cannabis may not be impaired by its moderate use

Colizzi and Bhattacharyya (2018) conducted a systematic review of all available studies that examined the acute effects of cannabis as a function of previous cannabis exposure. They discovered that regular users have different levels of tolerance for different types of effect. There was only partial tolerance for the euphoric and relaxing effects. But, more relevant to the current context, they found that 'Cognitive function is the domain showing the highest degree of tolerance, with some evidence of the complete absence of an acute effect (full tolerance)' (Abstract). It follows, for regular users, that those driving skills that are primarily dependent on cognitive function may not be measurably impaired by its moderate use. For that reason, some drug-impairment researchers, who are naturally keen to discover impairments, exclude regular users from their experiments. For example, Arkell et al. (2020: 2185) commented that 'This study … was limited to healthy volunteers who were *occasional* cannabis users. The applicability of these findings to more frequent users, including medical cannabis patients, is unclear given that daily cannabis use may produce at least partial tolerance to the impairing effects of THC'. So, the impairing effects of cannabis on regular users are more likely to be weak or absent than

'strong and consistent' (for a recent example, see Wickens et al. (2022a)).

Effects of the acute use of cannabis on lateral control

It is now widely agreed that the extent of weaving within the lane (operationalised, e.g., as the standard deviation of lateral position – SDLP) is one of the most sensitive means of assessing drug-induced impairment. For example, in a broad review of tests to measure the effects of drugs on driving, Brookhuis (2014: 120) concluded 'To date, SDLP has proved itself as the most valid and reliable indicator of performance deterioration'. In a 2016 review of *Cannabis and its effects on driving skills*, Bondallaz et al. said 'Experimental studies have shown dose-dependent alteration of several cognitive and psychomotor functions, particularly … by increasing lane position variability [SDLP]' (p. 100). In a recent review of the magnitude and duration of cannabis-induced impairment, McCartney et al. (2021a: 188) concluded that 'Some measures of driving performance and driving-related cognitive skills may be more sensitive to the impairing effects of THC than others'; and went on to say that 'SDLP was one of the outcomes that demonstrated the greatest sensitivity to THC's effects'.

Given Wood and Dupont's (2020) claim that the experimental evidence for impairment by cannabis is 'strong and consistent', and given that the control of lateral movement is probably the most sensitive measure of such impairment, it might be concluded that cannabis-induced impairment of lateral control would be easily elicited and strongly manifested. But that is not so. While many studies have found statistically-significant effects of cannabis on lateral control (as measured, e.g., by SDLP and reviewed by Bondallaz et al., 2016), there are some exceptions. There are at least five published driving-simulator studies that failed to find *any* decrease in lateral control from the acute use of cannabis: Anderson et al. (2010); Ronen et al. (2010); Brands et al., (2019); DiCiano et al. (2020); and Miller et al. (2020). Anderson et al., Ronen et al. and Miller et al. did not measure SDLP, but used alternative measures of lane control such as steering angle or lane departures. The subjects in the study by DiCiano et al. were medical users of cannabis, and might not, therefore, have been expected to be measurably impaired. And in some other studies, increases in SDLP were unpredictably found under only *some* of the experimental conditions. Three recently published simulator studies can illustrate that point. The study by Arkell et al. (2019b) involved two sequential driving conditions: 'car-following' for 5 min, and then a 'secondary-task' for 25 min. A statistically-significant increase in SDLP from the use of cannabis was found only for the briefer car-following condition. In a study by Brooks-Russell et al. (2021), two groups of subjects (occasional and regular users) drove on two types of roads (straight and curved). A statistically-significant increase in SDLP was found only for the occasional users on the straight road. And a study by Fares et al. (2021) reported results for either the whole driving course or for only the straight segments, and for driving conditions that were either distracting or non-distracting. Statistically-significant increases in SDLP were found only for the two outcomes for the overall course (whether or not the drivers were being distracted). These patchy results again illustrate that the evidence for impairment by cannabis is far from 'strong and consistent'.

Where statistically-significant increases in SDLP *are* found, they are not large. Increases in SDLP were demonstrated in a series of on-road driving studies commissioned by NHTSA nearly 30 years ago, as reported by Robbe and O'Hanlon (1993), who described the increases as 'not exceptional in comparison with changes produced by many medicinal drugs and alcohol' (Abstract). More recently, a study by Arkell et al. (2020) involved 'real-world' on-road driving. Although a statistically-significant increase in SDLP was found, the authors commented that 'In agreement with previous studies involving smoked cannabis or oral THC (dronabinol), this impairment was modest in magnitude and similar to that seen in drivers with a 0.05% BAC' (p. 2182).

A review by Simmons et al. (2022) of studies of the impairing effects of cannabis

Simmons et al. (2022) have very recently completed a systematic review and meta-analysis of studies of the impairing effects of cannabis and alcohol (separately and combined) on driving performance and behaviour. As the combined use of cannabis and alcohol is not a focus of our review, we will comment only on the findings for cannabis - alone, and as compared with the effects of alcohol alone. The presence of cannabis was treated as a dichotomous variable, while the presence of alcohol was treated as a quantitative variable, as measured in terms of BAC. The reviewers focused on simulated and on-road driving, and did not cover laboratory studies of psychomotor skills. They found that the use of cannabis had very little or no effect on most of the measured outcomes, including: hazard reaction-time; headway; speed 'exceedances'; and 'crash' rates. The only demonstrable detrimental effects of cannabis were on some measures of lateral control (such as SDLP). The reviewers also commented (p. 10) that 'With respect to speed, cannabis and alcohol had opposite effects. Cannabis led to decreases in speed, whereas alcohol led to increases in speed, with generally greater increases in speed at higher BAC levels'. One laudable objective of Simmons et al.'*s* review was to *quantify* the magnitude of any detrimental effects of cannabis, which they did in terms of impairment-equivalent BACs. For those few lateral-control variables that cannabis *did* affect,

the reviewers concluded that the effects were comparable to those at a BAC of about 0.05. (Later in our review, we will consider how to interpret an impairment-equivalent BAC for a cannabis-induced impairment).

Given the elusiveness of cannabis-induced impairments, and the weakness of the impairments that are found, the question naturally arises as to how the misunderstanding has evolved that the impairments are 'strong and consistent'. From the discussion in the following sections, it will be evident that selective reporting by researchers, publishers and commentators has fostered that misunderstanding.

Experimenters can exaggerate their findings of cannabis-induced impairment

One point made in the previous section was that most experiments fail to demonstrate impairments for at least *some* of their outcomes, even for occasional users of cannabis. The main point to be made in this section is that some putative laboratory demonstrations of impairment are illusory. By focusing on a few positive results from a study with mostly negative results, researchers can argue that real impairments have been found. This section starts with a brief overview of the meta-scientific literature on false-positive findings, before investigating one study (Ogourtsova et al., 2018) where a strong case can be made that false-positive findings of cannabis-induced impairment have been misconstrued as real.

False-positive findings and the replication crisis

Since 2005, when John Ioannidis wrote his controversial paper on *Why most published research findings are false*, there have been a number of attempts to estimate the actual proportion of reported research findings that are false, and therefore do not replicate (e.g. Camerer et al., 2016, 2018; Open Science Collaboration, 2015). In summarising the results of their own replication studies and those of other behavioural scientists, Camerer et al. (2018: 642) estimated that between 25% and 65% of reported findings could not be replicated.

Many causes have been proposed for the high rate of unreplicable false-positive findings (Frias-Navarro et al., 2020). In a *Nature*-sponsored survey by Baker (2016), 1576 researchers identified the main causes as the selective reporting of findings by researchers and the selective publication of submitted papers by editors. Selective reporting by researchers normally involves the interplay of three dubious research practices. The first is 'p-hacking' for statistically-significant (and probably false-positive) results within a large set of mostly non-significant results (Munafo et al., 2017; Simonsohn et al., 2014; Ware and Munafo, 2014). 'P-hacking' is also known as 'data dredging' and 'significance chasing'. The second is 'HARKing', which is the acronym for Hypothesising After the Results are Known (Kerr, 1998; Rubin, 2017). HARKing creates a plausible story from the random results of p-hacking. The third is 'spinning', whereby researchers highlight statistically-significant (and potentially false-positive) results while overlooking non-significant results (Boutron et al., 2010; Boutron and Ravaud, 2018; Chiu et al., 2017; Jellison et al., 2020). Spin is particularly common in the failure of Abstracts to adequately reflect the prevalence of non-significant results in the raw findings (Boutron et al., 2010; Lazarus et al., 2015). In the words of Boutron et al. (p. 2062), 'Our results highlight the prevalence of spin in the Abstract as compared with the main text of an article. These results have important implications because readers often base their initial assessment of a trial on the information reported in an Abstract'.

Potentially false-positive findings derived from these unacceptable, but common, research practices can then become entrenched through two further processes. The first is the paradoxical preference by authors to cite non-replicable publications over replicable publications (Serra-Garcia and Gneezy, 2021). That is probably because non-replicable results are anomalous and therefore of more interest than predictable results. The second is the selective publishing of positive over negative findings (Murad et al., 2018; Song et al., 2013). Failures to replicate apparently have not been of great interest to editors, although they should have been. Processes such as these can achieve what Nissen et al. (2016) describe as the 'canonization of false facts'.

In the study referred to above, Boutron et al. (2010) conducted an in-depth review of 72 randomized controlled trials (RCTs) with statistically non-significant results for all of their primary outcomes. They reported 'high' levels of spin in 24 (one third) of the RCT Abstracts. The spin usually involved distracting the reader from the fact that none of the primary outcomes was statistically significant (e.g. by failing to mention that fact) and by focusing the reader's attention on the results of statistically-significant secondary analyses. We report similar findings here. With respect to the study investigated below, we argue that the researchers failed to find evidence that cannabis had *any* impairing effects, but that they nevertheless presented impairment as an important outcome.

Ogourtsova et al. (2018)

The 45 young (18 to 24-year-old) participants in Ogourtsova et al.'s (2018) laboratory experiment all performed the same battery of tasks, which took up to 60 min to complete, from which nine performance measures were obtained. Two of the performance measures were derived from the Useful Field of View test (UFOV; Woutersen et al., 2017) (and related to divided attention and selective attention), and seven were obtained from a

driving simulator (and related to braking, steering, intersection crossing, vigilance and two aspects of obstacle avoidance). For all participants, the test battery was administered at four different times in relation to the time of smoking marijuana: once, on arrival at the laboratory, in the absence of smoking (to establish baseline levels of performance), and three times after smoking, at delays of 1, 3 and 5 h (to estimate the duration of any impairments). Test scores were calculated in relation to baseline performance. It might have been exhausting for the subjects to be tested four times on a single day (once pre-smoking, and three times post-smoking), so the four testing sessions were conducted on four separate testing days (day 1, day 2, day 3 and day 4). To avoid confounding between the four testing sessions and the sequence of testing days, the four sessions were evenly distributed between the participants across the four days. There was no preliminary training session before the first of the four testing days, so some practice effects might be expected, especially on day 1. This large-scale experiment seems to have been rigorously conducted. The administered dose of cannabis was strong enough to be considered by most of the participants to give a typical 'high'.

To evaluate the results of this experiment it is necessary to know how many potential outcomes could have been affected by the use of cannabis. There were 27 *main* outcomes (as determined by the nine performance measures at the three post-smoking delays). And there were an additional 108 *conditional* outcomes (as determined by the four testing days on which the 27 main outcomes could have occurred). The researchers reported that *none* of the 27 main outcomes was statistically significant. So, there was no overall impairing effect of cannabis on any of the nine performance measures at any of the three post-smoking delays.

Ogourtsova et al. then explored the 108 conditional effects. Only three of them were statistically significant: UFOV divided attention at 3 h delay when tested on day 1; UFOV selective attention at 3 h delay when tested on day 1; and UFOV selective attention at 5 h delay when tested on day 1. There were no significant impairments for the seven driving-simulator tasks. There were no significant impairments for the 1-h delay. And there were no significant impairments for day 2, day 3 or day 4. Obtaining three statistically significant outcomes from a set of 108 outcomes would be expected by chance alone at the $p = 0.05$ level. It is difficult to impose any sensible explanation on these findings. If there had been any *real* impairing effects of cannabis, they should have been most evident at one-hour post-smoking rather than at three or five hours. If the three instances of impairment were not simply random false-positives, the fact that they were all found only on day 1 might somehow reflect the lack of preliminary training. The most obvious interpretation of the full set of results is that Ogourtsova et al. had failed to demonstrate *any* real impairing effects of cannabis. So, how were the three presumably false-positive results reported by the authors?

Ogourtsova et al. described both of the two UFOV tasks and four of the seven driving-simulator tasks as 'complex'. The other three driving-simulator tasks were described as 'simple'. In their Abstract, they concluded that the cannabis 'had no effect on *simple* driving tasks, but there was significant impairment on *complex* tasks, especially when these were novel [administered on day 1]. These effects lasted up to 5 h after use'. The most straightforward interpretation of that claim is that statistically-significant effects of cannabis were found for all six complex tasks at all three post-smoking delays (18 statistically-significant impairments). In fact, *none* of those 18 outcomes was statistically significant. In stating their conclusions, the researchers focused on three of 108 outcomes, while failing to mention that the vast majority of outcomes were negative.

It can sometimes be instructive to investigate the reasons for biased scientific reporting. One recommendation from *Canada's Lower-Risk Cannabis Use Guidelines* (Fischer et al., 2017) is that: 'Users *categorically* refrain from driving (or operating other machinery or mobility devices*) for at least six hours* after using cannabis. This wait time may need to be longer, depending on the user and the properties of the specific cannabis product used'. The 6-h-wait was recommended from before the legalization in Canada on 17 October 2018 of the recreational use of cannabis (as smoked marijuana). The pending legalisation heightened the anxiety of Canadian road-safety agencies about a possible increase in crash rates. As an advocate for road safety, the Canadian Automobile Association (CAA) decided that they should fund a study to explore the effects of smoked marijuana on a driver's ability to perform driving-related tasks. In their Conclusion, Ogourtsova et al. claimed 'The present finding, that the first five hours after cannabis use affected driving-related performance, substantiates the recommendations of *Canada's Lower-Risk Cannabis Use Guidelines*, which recommend waiting 6 h after cannabis use before driving' (p. E462). On 1 October 2020, the CAA released a TV advertising campaign to discourage driving after consuming cannabis products. According to campaign-related information on the Cannabis Impaired Driving home-page of the CAA website (Canadian Automobile Association, 2020), the campaign relied on Ogourtsova et al.'*s* results to substantiate the 6-h-wait recommended in *Canada's Lower-Risk Cannabis Use Guidelines*. The funder relied on the results of Ogourtsova et al.'*s* research - but only as described in their Conclusion and Abstract.

Ogourtsova et al. (2018) are not the only researchers to have over-interpreted their results. We could have reported similar critiques for many other studies. It is common practice for researchers to highlight findings of impairment

while neglecting many outcomes where no impairment is found. We urge the readers of cannabis-impairment papers to enumerate all of the study outcomes for comparison with the few that are typically selected for reporting in paper Abstracts and Summaries.

Impairments that are statistically significant can nevertheless be trivial

An argument over the existence of trivial impairments

Moskowitz et al. (2000) conducted a review of *Driver characteristics and impairments at various BACs*, from which they concluded that alcohol had some impairing effects at BACs well below 0.05. They claimed that these relatively minor impairments would naturally increase the risk of crashing: 'Logic suggests that the impairment found at low BACs should be paralleled by crash and fatality data. The relationship, however, may be obscured in on-the-road data by uncontrolled variables, which can be controlled in an experiment. The laboratory data, therefore, yield conclusions about causal relationships that frequently cannot be detected in epidemiological data' (p. 23). Here, Moskowitz et al. are presuming that their *indirect* laboratory evidence for low-BAC crash risks is more relevant than any *direct* epidemiological evidence that there are no such risks. In his 2018 book on *Cannabis crashes: Myths and truths*, Macdonald makes a semantic distinction between a 'performance deficit', which is a small and inconsequential decrement in driving-related skilled performance, and an 'impairment', which is a decrement that is large enough to have road-safety consequences (and legal consequences for a driver charged with a driving-while-impaired (DWI) offence). Macdonald was critical of Moskowitz et al. for failing to make that distinction and for claiming that small decrements in performance would necessarily play a causal role in road crashes. We agree with Macdonald's opinion that epidemiological evidence about crash risks is more relevant than experimental evidence. We also agree that some statistically-significant performance decrements (or 'impairments') are too small to have any real-world significance. In this essay, however, we depart from Macdonald's preferred usage in that we can see no problem with using the terms 'performance decrement' and 'impairment' interchangeably. In other words, we do not reserve the term 'impairment' for only the most serious performance decrements.

Statistically-significant impairments can be small in relation to individual differences

In a perceptive 1957 presidential address to the American Psychological Association on *The two disciplines of scientific psychology*, Cronbach compared the two main approaches to the conduct of psychological research. One is the *experimental* approach, where the primary focus is on group means, and where individual differences around the mean are treated as 'random error' in the measured variable. The other is the *correlational* approach, where the individual differences are the focus of interest, and are treated as meaningful variations in the measured variable. Cronbach's comments on the attitude of experimentalists to variation around the mean are relevant to the main message we are hoping to convey in this section. He said that 'Individual differences have been an annoyance rather than a challenge to the experimenter. His goal is to control behavior, and variation within treatments is proof that he has not succeeded. Individual variation is cast into that outer darkness known as 'error variance'' (p. 674). In this section, we argue that individual differences in driving-related psychomotor skills have inappropriately been cast into the outer darkness by experimentalists, who may focus on small, but statistically-significant, differences in mean performance scores, even when the mean difference is considerably less than the spread of individual performance scores.

The possibility that a statistically-significant impairment could be trivially small is explored here by modelling the effect of cannabis on SDLP scores. The point of this exercise is to investigate the individual-level 'anatomy' of a statistically-significant finding in such a way that the real-world relevance of the finding is questioned. The model involves 50 simulated participants. The placebo condition has a mean SDLP of 20 cm. with a standard deviation (SD) of 3.5 cm, while the increase in SDLP due to cannabis is 1.0 cm. with an SD of 3.0 cm. (On the particular run of the model depicted in Figure 1, the placebo condition had a mean SDLP of 20.05 cm. with a SD of 3.53 cm, while the increase in SDLP due to cannabis was 1.11 cm. with a SD of 2.81 cm.) In Figure 1, each participant's result for the cannabis (THC) treatment is plotted against his or her result for the placebo treatment. A higher SDLP indicates more weaving and therefore a worse performance. The individual placebo and cannabis SDLPs were strongly correlated ($r = 0.74$). A paired-samples t-test comparing the SDLP means of the placebo and THC conditions gave a p value of 0.0038, indicating a statistically-significant impairing effect of cannabis.

Referring to Figure 1, the 'Equality line' is the line of zero impairment (where each SDLP for the placebo and THC treatments are equal). It divides the 50 simulated participants into those whose SDLP scores after using cannabis are better (below) or worse (above) than after using the placebo. It can be seen that 18 of the 50 cannabis-affected participants (36%) actually performed *better* than when under the placebo condition. The 'Mean Placebo SDLP line' simply identifies the constant value that is the mean SDLP value for the placebo condition. It divides the

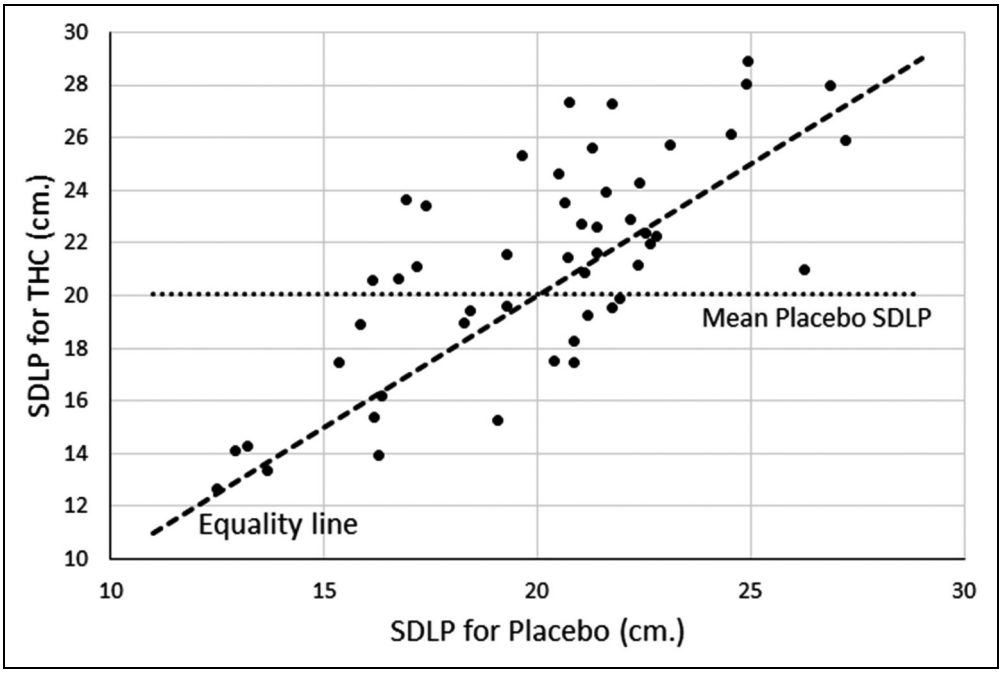

Figure 1. Scatterplot for the standard deviation of lateral position (SDLP) for 50 simulated participants, with the results for the cannabis (THC) treatment plotted against the results for the placebo treatment.

50 simulated participants into those whose SDLP scores after using cannabis are better (below) or worse (above) than the mean for the placebo condition. It can be seen that 19 of the 50 cannabis-affected participants (38%) actually performed *better* than the mean SDLP for the placebo condition. When the findings are summarised in this way, with a focus on individual performances rather than averaged results, a nuanced picture emerges, and the conventional summary of the results in terms only of a single mean difference can be seen to be potentially misleading.

There is nothing here that would be surprising to a statistician. However, a policy advisor who was told that cannabis had a statistically-significant impairing effect on SDLP might be less disturbed by such information if also told that, when affected by cannabis, 36% of people performed *better* than when unaffected, and 38% performed *better* than the mean of the placebo condition. The moral of this story is that a statistically-significant impairment that is small in relation to the spread of individual differences should be considered of questionable relevance to road safety. That observation may seem too obvious to mention. However, the authors of many impairment studies automatically interpret the statistical significance of their findings as proof of substantial impairment.

One response to the questionable relevance of the statistical significance of a finding is that the 'effect size' for the finding (where the mean difference is expressed in relation to the spread of individual scores) should also be considered. However, not much is achieved by a shift of focus to effect sizes, because the problem then becomes how to recognise a trivially small effect size for a particular performance decrement. If statistical significance and effect sizes are unsatisfactory as criteria for the real-world relevance of an impairment, the question must then be asked as to what might constitute a satisfactory criterion. In the following section a commonly accepted criterion is considered, but then dismissed as invalid. Then, in the next section, a criterion is proposed that seems to have acceptable face validity.

The invalid notion of a BAC crash-equivalent concentration of THC

The focus of the DRUID program of research

A large-scale program of research on *Driving under the Influence of Drugs, Alcohol and Medicines* (DRUID) was commenced in 2006 to 'provide scientific support to E.U. road-safety policy makers by making scientific-based recommendations concerning combatting driving under the influence of psychoactive substances' (Schulze et al., 2012: 5). More specifically, the DRUID program 'aimed at assessing possibilities to determine blood concentration thresholds for various drugs analogically to BAC thresholds applied in European countries to combat driving under the influence of alcohol' (Schulze et al., p. 5). The DRUID researchers knew that 'Calculating accident risks from epidemiological data is a straightforward approach with face validity', but recognised that the epidemiological evidence was usually not available, so they acknowledged that 'the

impairment approach must be chosen' (Schulze et al., p. 5). So, the compromised intermediate aim of the DRUID program was to find BAC impairment-equivalent cut-off concentrations for a number of psychoactive drugs. To achieve the final aim would then involve the use of an algorithm to convert impairment-equivalent cut-offs to proxy measures of crash risk.

It is important to note that our use of the term 'impairment', in the context of the DRUID work on establishing BAC-equivalent concentrations of psychoactive drugs, refers only to the impairment of driving-related psychomotor skills. The term excludes any drug-induced emotional effects, such as risk taking or impulsivity, that may also affect driving behaviour. To obtain BAC-impairment-equivalent drug concentrations, it is necessary to use performance measures that are positively correlated. So, it would not be sensible to use a measure of risk-taking, which is normally *reduced* following the use of cannabis (e.g. Brands et al., 2019; Wickens et al., 2022b) but *increased* following the use of alcohol (e.g. Hoiseth et al., 2021; Yadav and Velaga, 2019a). More explicitly, as described by Berghaus et al. (2011, Table 3), the DRUID tests of impairment, in the context of measuring BAC-equivalence, included 13,191 outcome measures in the following categories: attention in general and divided attention (30.1%), encoding and decoding (20.2%), reaction-time (13.6%), visual functions (13.2%), psychomotor abilities (12.8%), tracking (6.6%), and driving behaviour (3.6%). While the category 'driving behaviour' probably included some emotional responses, it represented only a vanishingly small proportion of all outcome measures.

The complex algorithmic procedures used by the DRUID researchers to convert experimental results for drug impairment into proxy measures of crash risk were described in detail at the outset of the DRUID research program by Kruger et al. (2008), who commented that their goal was to 'Establish a method in order to interpret experimental results of driving studies as accident risks'. The researchers attempted to achieve that goal through using 'information about one prominent substance – alcohol – as a reference for other substances' (p. 8). By the end of the DRUID program, Hargutt et al. (2011) had calculated proxy crash risks for various concentrations of many medicinal and illegal psychoactive drugs (including THC).

The DRUID researchers were particularly interested in identifying drug concentrations that had the same impairing effect as alcohol at a BAC of 0.05. In the words of Ramaekers et al. (2011: 10) 'Any drug-induced performance-change greater than the performance change induced by BAC 0.5 mg/ml was qualified as a 'clinically relevant' drug effect'. From a meta-analysis of alcohol and drug impairment studies (Berghaus et al., 2011), the DRUID researchers concluded that the clinically-relevant concentration of THC was about 2 ng/ml in whole blood (Schulze et al., 2012: 84). Any THC cut-off is relevant only insofar as it relates to a risk of crashing. Given that a BAC of 0.05 roughly doubles the risk of crashing (McLean and Kloeden, 2002), it follows that the clinically-relevant concentration of any drug is the concentration that roughly doubles the risk of crashing.

The DRUID approach of using the results of impairment studies to set BAC-equivalent cut-off levels for a number of psychoactive drugs has influenced the development of drug-driving legislation in many countries. In Norway, for example, the BAC cut-offs that define increasing levels of drink-driving penalties are 0.02, 0.05 and 0.12 (NMTC, 2014). Consistent with the DRUID approach, Norwegian researchers identified drug concentrations that caused the same levels of impairment as BACs of 0.02, 0.05 and 0.12. For cannabis, the respective whole-blood concentrations of THC were determined to be 1.3, 3.0 and 9.0 ng/ml (Verstraete et al., 2011: 13–16; Vindenes et al., 2012, 2014). So, in Norway, a THC concentration of 3.0 ng/ml in whole blood is deemed to be the 'clinically relevant' level. The fact that the increasing *per se* drug-driving penalties in Norway that are associated with increasing drug concentrations are ultimately (albeit not directly) justified in terms of increasing crash risks is described by Vindenes et al. (2014: 176) in the following terms: 'Norway has defined limits for graded sanctions for 13 of the 20 non-alcohol drugs in accordance with scientific evidence that higher concentrations lead to increased impairment and thus increased risk of traffic accidents'. They go on to say that 'The increased risk of causing a traffic accident is the reason for giving a more severe penalty, and from a political point of view in Norway, it has been important to establish a system where more serious incidents result in more severe penalties'.

Formalising the DRUID argument for a BAC-crash-equivalent concentration of THC

For the purpose of this part of our review, the focus will be on cannabis rather than any other potentially impairing drug, and the term 'impairment' will refer specifically to the impairment of driving-related psychomotor skills. Depending on the context, the 'BAC-equivalent' of THC is the concentration of THC that either has the same impairing effect as a particular BAC (the 'impairment equivalent') or increases the risk of crashing to the same extent as a particular BAC (the 'crash equivalent'). Although BAC-equivalent concentrations of THC are ultimately defined in terms of crash risks from epidemiological studies, in practice they are usually obtained from experimental studies of impairment. The argument that links the experimental and epidemiological domains, which was *implicit* in the writing of the DRUID researchers (as

discussed above), is made *explicit* below in terms of five assumptions and two conclusions.

- *Assumption 1*: There is a monotonic causal relationship between THC concentration in blood and degree of impairment.
- *Assumption 2*: There is a monotonic causal relationship between BAC and degree of impairment.
- *Interim conclusion*: For any degree of impairment, the impairing concentration of THC can be matched 1:1 with the impairing BAC. The matched drug concentration can be called the 'BAC impairment-equivalent' of THC.
- *Assumption 3*: For both cannabis and alcohol, there is the same monotonic causal relationship between *degree of impairment* and *risk of crashing*.
- *Assumption 4*: For cannabis, the causal relationship between THC concentration in the blood and the increased risk of crashing is *always and only* mediated by the drug's *impairing* effect.
- *Assumption 5*: For alcohol, the causal relationship between BAC and the increased risk of crashing is *always and only* mediated by alcohol's *impairing* effect.
- *Conclusion*: The crash risk pertaining to a particular concentration of THC can be measured by proxy: it is the same as the crash risk pertaining to the BAC that impairs performance to the same extent as the concentration of THC. That drug concentration can be called the 'BAC crash-equivalent' concentration of THC.

The purpose of presenting the implicit DRUID argument clearly, as a set of assumptions and conclusions, is to show what it *must* look like if the notion of a BAC-crash-equivalent drug concentration is to have any legitimacy. The truth of the assumptions is another matter. If any of the assumptions is not true (as we will argue), then the notion becomes untenable.

For the purpose of this section of our review, all but the fifth assumption are taken to be true, enabling the truth of that assumption, and its implications, to be the focus of our immediate attention. Later in our review, the truth of the first assumption will also be investigated. The fourth assumption probably also fails to tell the full story, because the impairing effects of cannabis may be countermanded, at least to some extent, by its effects on cautious driving.

The gold standard test for determining clinically-relevant drug concentrations

According to Ramaekers (2017: 319) 'Over the years, the *on-road* driving test has been acknowledged by researchers in the field and legislators as the golden standard for measuring drug effects on driving'. Furthermore, 'The primary outcome variable of the on-road driving test is the standard deviation of lateral position (SDLP) in centimetres'. And in particular, 'The mean increment in SDLP (i.e. 2.5 cm) that was observed during driving at a BAC of 0.05 has been defined as the minimal cut-off value to represent clinical relevance' (p. 320). Ramaekers acknowledged that the value of 2.5 cm. had been established in an alcohol impairment review by Jongen et al. (2017). The value of 2.5 cm is consistent with that found in a similar review by Irwin et al. (2017). So, the 'clinically relevant' concentration of any psychoactive drug can now be considered to be the concentration that increases on-road SDLP by 2.5 cm.

Some researchers, including Hartman et al. (2015) in America, and Arkell et al. (2021) in Australia have explicitly endorsed the idea of a clinically relevant increase in SDLP. For example, the idea was central to a study of the validity of *per se* THC limits by Arkell et al. who commented that the 'Determination of driving impairment … was based on whether participants' SDLP increased by more than 2.0 cm from their placebo condition. This cut-off is consistent with what is considered to be the lowest criterion for clinically relevant driving impairment (Jongen et al., 2017)'. (It is not clear why Arkell et al. chose the value of 2.0 cm. rather than 2.5 cm. for the clinically relevant increase in SDLP, but the difference is of no real consequence.)

A distinction must be made between cognition and emotion

As noted above, the validity of the idea of a BAC *crash-equivalent* concentration of a drug rests on five assumptions, the fifth of which is that the causal relationship between the acute use of alcohol and the increased risk of crashing is always and only mediated by the impairing effect of alcohol on driving-related psychomotor skills. The truth of that assumption is now challenged.

The first step in the challenge is to confirm the conventionally accepted distinction between *cognition* and *emotion* (e.g. Forgas, 2008), where 'emotion' can also be called 'affect' or 'mood', and should be taken to include acute motivational states. (Making this distinction does not imply that these two major types of psychological functioning are independent: they obviously interact in many ways). In the category of cognition, the focus here is on the types of driving-related psychomotor skills that can be investigated experimentally. In the category of emotion, the focus is on what can roughly be described as 'bravado', which refers to a psychological dimension that ranges from responsible cautiousness to irresponsible foolhardiness. The broad term 'bravado' is deliberately chosen to cover more familiar terms such as 'risk taking', 'thrill seeking', 'impulsivity' and 'aggression', while sidestepping the extensive psychological theorising that

pertains to each of those terms. The characterisation of the emotional dimension as 'bravado' may not be very accurate, but that does not really matter, as this term is only an attempt to describe the obviously necessary emotional component of the driver's psychological contribution to crash causation. So, the basic distinction here is between *driving-related psychomotor skills* and *bravado*. This distinction should not be blurred by reference to 'the impairment of judgement', as though bravado can simply be reduced to a cognitive impairment. The view that psychomotor impairments are not the full story on drug-crash causation, and that emotional states must also be invoked, is described here as the 'two-dimensional' theory of drug-crash causation. The alternative is the 'one-dimensional' impairment theory.

The one-dimensional theory does not provide a complete explanation of alcohol-related crashes

The inadequacy of the one-dimensional framework is now explored for alcohol-related crashes. A dose-response relationship between BAC and crash risk was first identified by Robert Borkenstein and his colleagues in 1964, and has been replicated many times since. It is sometimes assumed that this relationship is due solely to the impairing effects of alcohol on driving-related psychomotor skills (the one-dimensional theory). For example, in summarising some of their findings, Moskowitz et al. (2000: 22) said: 'The results indicate that as BACs rise, the percentage of individuals exhibiting impairment, as well as the magnitude of the impairment, grows. Thus, there is great consistency in the relationship between the degree of impairment and BAC. These findings are consistent with the findings from epidemiological crash data'. (Moskowitz (1985) had previously reviewed the *cannabis*-crash literature from the same one-dimensional perspective.)

In contrast, the need for a two-dimensional understanding of the causes of alcohol-related crashes was clearly recognised almost 50 years ago by Barry (1973) in his review of the *Motivational and cognitive effects of alcohol*, where he concluded that the use of alcohol increased the risk of crashing because, in addition to its psychomotor impairing effects, it gave rise to 'speeding or risky manoeuvres' as a consequence of 'decreased fear and increased assertiveness' (p. 218). Beirness et al. (2006: 12–13) also support the two-dimensional approach in advising that: 'Laboratory findings are informative but limited as an indicator of actual on-road driving risks. Laboratory tests can address the effects of drugs only on skills, not on judgment, and the latter may be as important when it comes to driving. Thus, even if drugs are found to affect driving skills in laboratory tests, actual crash risk may or may not be affected'. Coming from the same two-dimensional perspective, Laude and Fillmore (2015) describe their study as 'the first to isolate alcohol-induced increases in risky driving [from] alcohol-induced impairments in driver skill'. Given that there is an extensive literature on the effects of alcohol and other drugs on motivational and emotional states (not broached here), it may seem surprising that much of the drug-crash literature has been written within the one-dimensional impairment framework.

The epidemiological study by Keall et al. (2004) clearly illustrates the inadequacy of the one-dimensional approach to the role of alcohol in crash causation. The study extended the findings of Borkenstein et al. (1964) by breaking the alcohol-crash risks down by the number of passengers in the car as well as by age and sex. The researchers found that 'The risk of crashing for drivers carrying two or more passengers rises very steeply even at modest BAC levels' (p. 60). The increased risk was particularly marked for younger drivers on Friday and Saturday nights. It would be very difficult to explain these findings in terms of impaired driving skills, but it is easy to explain them in terms of the bravado generated in a group of young people after a drink or two. Other findings that are more readily explained by the bravado hypothesis, are that the use of alcohol is strongly associated with speeding (e.g., Hartman et al., 2016), with speed-related crashes (e.g., Bogstrand et al., 2015), and especially with speed-related crashes at low BACs (Phillips et al., 2015).

The one-dimensional approach particularly fails to explain low-BAC crashes

We will use the term 'low BAC' to describe BACs in the vicinity of 0.05 and below. The inability of the one-dimensional approach to fully explain the elevated crash risk at low BACs is now further considered. As noted above, there is good evidence that alcohol roughly doubles the risk of crashing at a BAC of 0.05 (McLean and Kloeden, 2002). And there is no doubt that alcohol can severely impair many psychomotor skills at BACs above 0.05, in such a way as to increase the risk of crashing. However, the weaker impairing effects at BACs of 0.05 and below (e.g. Hoffman and Nixon, 2015), although often statistically significant (e.g. Moskowitz et al., 2000), are nevertheless arguably below the threshold for crash causation. So, why does the use of alcohol increase the risk of crashing at BACs of around 0.05? The most plausible answer is mainly through its effect on bravado.

There is an abundance of evidence from outside the laboratory that alcohol causes a range of maladaptive risk-taking behaviours (e.g. Brooks et al., 2019); and corroborating evidence from within the laboratory has recently been reviewed by Harmon et al. (2021). While the laboratory evidence for risk-taking and related behaviors *at low BACs* is not plentiful, the available evidence was reviewed by

Weafer and Fillmore (2016), who defined a 'low BAC' as below 0.08 – the general driving limit in the U.S. They concluded that 'Overall, below-limit doses of alcohol impair inhibitory control and increase risk-taking …' However, they emphasised that only *some* laboratory tests of inhibitory control and risk-taking are affected by low BACs.

We acknowledge that our bravado theory of low-BAC crash causation is controversial, given the following facts. Alcohol at a BAC of about 0.05 and below has real and statistically-significant impairing effects on many psychomotor outcomes (Moskowitz et al., 2000), including SDLP (Louwerens et al., 1987). In their recent review of 179 experimental studies, Jongen et al. (2016) summarised the impairing effects of alcohol found at 'low', 'medium' and 'high' BACs for a range of psychomotor outcomes, such as sensorimotor function, divided attention, reaction time, processing speed and decision making. They found that slightly more than half of the 251 outcomes at 'medium' BACs, in the range from 0.03 to 0.06, were statistically significant. We will refer to both this range and the lower range, as 'low BACs'. So, we acknowledge that an explanation of low-BAC crashes in terms of low-BAC impairments is very plausible, and widely accepted. We assume, however, that the wide range of psychomotor impairment caused by alcohol at low BACs makes little or no contribution to alcohol-related crash risk. We assume that humans are over-engineered for safety, and that impairments from alcohol at BACs of about 0.05 and below are mostly within human safety tolerances. We assume that alcohol-induced crash risk at low BACs is primarily caused by alcohol's effects on bravado and not by its effects on psychomotor function. It would be useful if epidemiological evidence from crash studies could be used to arbitrate between the impairment and bravado theories of low-BAC crash causation, but, unfortunately, such studies are normally silent on crashed drivers' levels of psychomotor impairment or bravado.

While alcohol-related bravado can be expressed in many ways, its most worrying manifestation on the road is an increase in speeding. But before considering the results of some studies of low-BAC speeding, some findings from the broader body of research on speeding are briefly reviewed. The first is that speeding is now recognized as the greatest contributor to fatalities on the roads (Job and Brodie, 2022; NTSB, 2017) – especially given that a large proportion of alcohol-related fatalities are known to be caused by speeding (NTSB, Figure 4). The second finding is that there is a substantial increase in the risk of crashing from travelling only slightly above the mean speed for the location (Kloeden et al., 2002); and this increase is much greater than most drivers realize (Mooren et al., 2013). The third finding is that, contrary to an earlier view that driving slower than the mean travelling speed could increase the risk of crashing (e.g. Solomon, 1964), it is now recognized that slower driving does *not* increase the risk of crashing (Kloeden et al.). Given the tight causal relationship between excessive speeding and the risk of crashing (Kloeden et al.), it would not be surprising if driving at a BAC of 0.05 doubled the risk of crashing - through the effects of bravado on speeding, in the absence of any substantial contribution from psychomotor impairment.

Experiments that explore the effects of low BACs on speeding

There are four studies known to us that involved the use of driving simulators to explore the effects of *both* a low-BAC treatment *and* a cannabis treatment on measures of speeding (as well as on other outcome measures that are not discussed here). Ronen et al. (2008) found that their subjects drove significantly faster at a BAC of about 0.05 than in the alcohol-placebo condition. In contrast, they drove significantly slower after using cannabis than in the cannabis-placebo condition. Ronen et al. (2010) repeated and extended the methods of their previous study. They concluded that they had essentially replicated their previous results: 'As in our previous study, when THC alone was administered, subjects drove significantly slower than when they were under the influence of alcohol, which actually made them drive faster' (p. 1864). Despite that wording of their results, the post-hoc comparisons of alcohol vs alcohol-placebo and THC vs THC-placebo were actually non-significant. The authors speculated that their failure to fully replicate their previous findings could somehow be attributed to the greater complexity of the driving task in the second study (p. 1864).

Lenne et al. (2010) assessed the effect of two levels of alcohol (with mean BACs of 0.02 and 0.05) and two levels of cannabis on a number of measures of simulated driving performance, including mean speeds. In their Abstract (p. 859), they claimed that 'alcohol was associated with a slight increase in mean speed' and that 'high-dose cannabis was associated with decreased mean speed'. However, they did not mention in their Abstract that both the higher dose of alcohol and lower dose of cannabis had no such effects. So, their results for the predicted effects of the two drugs on speeding were weak and patchy. However, their experimental design was not facilitative of the effects of drugs on speed: For roughly the first half of the simulated 6.6 km course, the subjects were instructed to follow a lead vehicle at a fixed distance of 40 metres, while for roughly the second half of the course they were instructed to 'maintain an appropriate speed for the conditions' (p. 861). Overall, therefore, there may not have been much opportunity for the subjects to select their preferred driving speed.

Hartman et al. (2016) examined the separate effects of alcohol (median BAC = 0.05) and a recreational dose of cannabis on a number of speed-related variables, including

the mean speed, the percentage of time spent more than 10% above the speed limit (percent speed high), and the percentage of time spent more than 10% below the speed limit (percent speed low). They concluded that 'The compensatory behavior exhibited by cannabis-influenced drivers distinctly contrasts with an alcohol-induced higher-risk behavior, as evidenced by a greater percent speed high' (p. 1427)'. In more detail (p. 1240): blood-THC was associated with a decreased mean speed, and an increased percent speed low; while breath-alcohol was associated with an increased percent speed high, but not with an increased mean speed. So, the results, albeit in the predicted direction, were again weak and patchy.

Let us now consider the effects of low BACs on speeding, as found in simulator studies that did *not* also explore the effects of cannabis on speeding. Studies of the effects of a range of BACs on speeding that were published prior to 2017 were reviewed by Irwin et al. (2017). Their forest plot (Figure 4) indicated a weak positive overall effect of alcohol on speeding. The particular results for low target-BACs of 0.03 and 0.05 were mixed: Of the six effects from four studies, two showed statistically significant increases in speed, while four showed no significant change (their Table 2a). Three reports of relevant simulator studies have been published since Irwin et al.'s 2017 review. Brown et al. (2018) found a statistically significant increase in speed in an urban environment from the use of alcohol at a target-BAC of 0.05. They found stronger effects at a target BAC of 0.10. Yadav and Velaga (2019b) investigated the effects of target-BACs of 0.03, 0.05 and 0.08 on speeding on rural highways. They reported (p. 4) that the mean speed at the target-BAC of 0.03 'was significantly higher than in the sober state', and that 'the mean speed of participants increased significantly with increases in BAC levels'. Yadav and Velaga (2020) replicated those findings for both a rural and an urban environment

The overall pattern of results from these simulator studies is clear: Where low levels of alcohol have been shown to have an effect on speeding, the effect has always been an increase. Not all of the studies had methodologies that were facilitative of speed effects. For example, the driver was sometimes constrained by the requirement to remain a fixed distance behind a lead car. And for low-BAC speeding to translate into low-BAC crashes would only require that a small percentage of drivers were triggered by low doses of alcohol to engage in excessive speeding. Such unpredictable relationships are not easily investigated in the laboratory.

The dubious notion of a BAC crash-equivalent concentration of THC

In the previous section of this review, the benchmarks of statistical significance and effect size were rejected as means of distinguishing between trivial and real-world levels of impairment by cannabis. The purpose of this section has been to investigate the plausibility of another possible means: the establishment of BAC crash-equivalent concentrations of THC, such as the 'clinically relevant' concentration of THC at which performance is impaired to the same extent as for a BAC of 0.05, and, by implication, the crash risk for cannabis is the same as for a BAC of 0.05. For example, using a driving simulator, Hartman et al. (2015) found that a blood THC level of 8.2 ng/ml caused the same increase in SDLP as a BAC of 0.05: How should that finding be interpreted?

The arguments presented in this section in favour of our bravado theory of low-BAC crash causation are now summarised. Alcohol at a BAC of 0.05 and below has real and statistically-significant impairing effects on many psychomotor outcomes (Jongen et al., 2016; Moskowitz et al., 2000), including SDLP (Louwerens et al., 1987). We assume, however, that the wide range of psychomotor impairment caused by alcohol at these concentrations makes little or no contribution to alcohol-induced crash risk.

The impairing effect of alcohol on SDLP at the BAC of 0.05, which is in the vicinity of a 2.5 cm increase in SDLP from on-road studies, is widely used as a benchmark for the psychomotor impairment caused by recreational and medical drugs (van Dijken et al., 2020; Vinckenbosch et al., 2020). The 'clinical relevance' of the 2.5 cm increase in SDLP is based on the fact that alcohol at a BAC of 0.05 increases the risk of a road crash (e.g. Ramaekers, 2003: 21). In the words of Vinckenbosch et al. (2020: 878): 'SDLP as measured in the on-the-road driving test is not merely a measure of driving impairment but also predicts crash risk'. (Kloeden et al. (2002) estimate that the risk of crashing is doubled at a BAC of 0.05.) However, we assume that humans are over-engineered for safety, and that minor impairments (such as at a BAC of 0.05) are probably below the threshold for crash causation. We argue that the increased risk of crashing at a BAC of 0.05 could be entirely due to bravado. It follows that the BAC-impairment-equivalent concentration of a drug (especially for low BACs) is not relevant to the crash risk pertaining to that concentration of the drug. The BAC = 0.05 impairment-equivalent concentration of a drug (the 'clinically relevant level') is thereby stripped of any real-world relevance. We do not deny that it is possible to determine BAC = 0.05 impairment-equivalent concentrations for some drugs; we simply think that the exercise is of little value.

Two reasons why the notion of a BAC crash-equivalent concentration of THC is of dubious validity

There are two main reasons why the notion of a BAC crash-equivalent concentration of THC is of very dubious validity. The first, in relation to the truth of Assumption

5, has been discussed in this section. The second is that Assumption 1 (There is a monotonic causal relationship between THC concentration in blood and degree of impairment) is false - although it was assumed to be true for the purpose of the discussion in this section. The falsity of Assumption 1 is discussed later in this paper, in the section on *Impairment cannot be predicted from THC concentration or presence: Implications for stand-alone per se cannabis-driving offences.*

A benchmark for unproblematic impairment is proposed in terms of cognitive aging

In previous sections of this review, it has been argued that a criterion of impairment needs to be identified below which an impairment should be considered unproblematic from the road-safety perspective. Criteria in terms of statistical significance, effect sizes, and BAC crash-equivalence were suggested, but then rejected. In this section, a criterion is suggested in terms of age-related performance decrements.

Cognitive aging

As well as adequate physical health and visual capacities, a number of cognitive skills are required for safe driving. Reviews of the literature on the required skills (Adler et al., 2005; Anstey et al., 2005; Apolinario et al., 2009; Chaudhary et al., 2013; Lococo and Staplin, 2006; Mathias and Lucas, 2009; Reger et al., 2004) have regularly identified five main skill domains: information-processing speed; divided and selective attention; spatial visualisation; episodic and working memory; and executive functions. Most specific cognitive skills involve more than one of those domains. Speed of processing is arguably the most basic of the domains, because it sets performance limits for the other domains (Salthouse, 1996).

Many cognitive skills deteriorate with age (Correia et al., 2018; Harada et al., 2013; Karthaus and Falkenstein, 2016; Wagner et al., 2011). The deterioration is normally slow but steady from about 20 years to about 70 years, after when there can be a sharp decline. In their review of the literature on information-processing speed over the lifespan, Cerella and Hale (1994: 184) concluded that 'Beyond age 20, [choice reaction times - CRTs] increased throughout middle- and old-age, in a manner that could be described by a positive exponential function of age over the range from 20 to 85 years'. And from their meta-analysis of studies on the decline of cognitive skills in adulthood, Verhaeghen and Salthouse (1997: 246) concluded that 'Significant non-linearity in the age relation, indicating acceleration of the age deficit with advancing age, was found for the variables of [processing] speed and reasoning; for the other variables, this trend was in the same direction'. They went on to note that 'Comparisons across age groups indicated that the influence of age was generally greater for those over the age of 50, but sizeable relations were found in both age ranges'.

Der and Deary (2006) have shown that CRT increases steadily with age from the age of about 20 to the age of about 70, after when the deterioration accelerates. They found statistically-significant increments for every 10 years (or less) in the range from 20 to 70 years (top right panel of their Figure 2). So, the CRT performance of 30-year-olds is significantly worse than that of 20-year-olds, and the CRT performance of 40-year-olds is significantly worse than that of 30-year-olds, etc. To state the obvious: it has never been proposed that 60-year-olds should be banned from driving because they are significantly more impaired (at least, with respect to processing speed as measured by reaction times) than 50-year-olds - and extremely more impaired than 20-year-olds.

The effect of cognitive aging on road crashes

From the age of about 30 onwards, the relationship between age and crash involvement (per distance travelled) is essentially two-phased: from about 30 to about 70 there is no increase in crash involvement with age; but from the age of about 70, the risk of crashing increases sharply with age (Cicchino and McCartt, 2014; Evans, 2000; Tefft, 2012). The phases are very different in two respects: The older drivers are much more likely than the younger to be culpable for the crashes that they are involved in (Eustace and Wei, 2010; Stutts et al., 2009; Tefft, 2008); and, in comparison with the younger-driver crashes, the older-driver crashes are much more likely to be caused by the failure to correctly perform basic psychomotor skills (Eustace and Wei, 2010; Stutts et al., 2009).

The conclusion reached from the information provided above is that the relationship between cognitive aging and crashing is two-phased. In the first phase, from about 20 to 70, the gradual decline in psychomotor performance occurs within the capacity of humans to drive safely, such that the decline is not reflected in an increasing risk of crashing. In other words, across these years the incremental impairments from normal cognitive aging remain below the threshold for crash causation. But, beyond the age of about 70, the further decline in psychomotor performance can occur outside the capacity of humans to drive safely, such that the decline is reflected in an increasing risk of crashing.

Comparing the effects of cannabis and normal cognitive aging on the risk of crashing

The information provided above enables us to propose a benchmark degree of impairment from the use of cannabis

below which the impairment should be considered irrelevant in the context of road-safety. Given that normal cognitive aging from about 20 to about 70 years has no demonstrable effect on the risk of crashing, it is considered that any impairment from the use of cannabis that is less than the impairment from those 50 years of normal cognitive aging might be considered unproblematic from the road-safety perspective. To err on the side of caution, it is proposed that the benchmark should comprise three-fifths of that degree of age-related impairment (corresponding to the impairment from 30 years of normal cognitive aging, from, say, 30 years to 60 years of age). That benchmark assumes that humans are over-engineered for safe driving, and that age-related impairments within the capacity limits are of no consequence in a normal driving environment. There is no road-safety authority that advises removing 60-year-olds from the road because of their degree of normal cognitive aging. It therefore seems reasonable to propose that cannabis-caused performance decrements that are less severe than the normal deterioration in performance that occurs over a 30-year period (below the age of 70 years) should be considered unimportant in the context of road-safety.

Applying the benchmark to choice reaction time (CRT)

We know of only four laboratory studies where CRT has been an outcome variable in relation to the acute use of cannabis: Kvalseth (1977), Pickworth et al. (1997), Ligouri et al. (1998) and Wesnes et al. (2010). All but Wesnes et al. failed to find any effect of cannabis on CRT. Wesnes et al. found that the higher of two doses of the synthetic cannabinoid Nabilone caused a statistically-significant increase of 55 milliseconds in CRT.

By inspection of Figure 2 in Der and Deary's (2006) population study of reaction times, it can be seen that the increase in CRT between the ages of 30 and 60 years is about 100 milliseconds. Taking an increase of 100 milliseconds as the benchmark for triviality in CRT, it would seem that the 55-millisecond increase found by Wesnes et al. (2010) is of no real consequence. However, this type of research needs to be carried out using the same experimental procedures for both age groups, involving cannabis and placebo treatments, so that the separate impairing effects of cannabis and cognitive aging can meaningfully be compared.

Applying the benchmark to standard deviation of lateral position (SDLP)

As noted previously, increases in SDLP from on-road driving studies are often considered to be the 'gold standard' measure of drug-related impairment (e.g. Helland et al., 2016; Verster and Roth, 2014). And, as also noted previously, an increase in SDLP of about 2.5 cm. is typically found from the use of cannabis (e.g. Arkell et al., 2021). It would be interesting to compare that value with the increase that accompanies the normal aging process.

A review of pre-2004 simulator and on-road studies of the effects of aging on SDLP (Green et al., 2004) found a non-significant increase of about 2.0 cm. per decade of age from the age of 20 years, giving a total increase of about 6.0 cm. for 30 years of aging. If that finding were to hold up in the face of more recent research, then the effect of cannabis on SDLP (an increase of about 2.5 cm.) would have to be considered trivial. However, because Green et al.'*s* finding was not statistically significant, we consider that it is of no evidential value.

A number of recent *simulator* studies have investigated the effect of aging on SDLP (Doroudgar et al., 2017; Michaels et al., 2017; Ortiz et al., 2018; Ortiz-Peregrina et al., 2020; Ranney et al., 2013; Son et al., 2010). However, none is able to provide a reliable benchmark measure of the increase in SDLP for a 30-year period. One reason for that inadequacy is that in all of the studies, except that by Ortiz-Peregrina et al., the older drivers were permitted to drive more slowly, so any potential increases in SDLP could have been compensated for. The study by Ortiz-Peregrina et al. compared the simulated driving performance of 21 younger (25 to 40 years) and 21 older (56 to 71 years) drivers. The results considered here are for the 'mountain road' segment of the simulated driving course. The subjects were instructed to adhere to the speed limit; and both age groups drove at similar average speeds. The mean SDLP for the older drivers (65 cm.) was considerably larger (by 14 cm.) than for the younger drivers (51 cm). If the very high value of a 14 cm. age-related increase in SDLP were to be taken as the benchmark for a non-trivial impairment, then most drug effects, including for high levels of BAC (Irwin et al., 2017), would be deemed to be trivial, which is obviously not a plausible outcome.

SDLP results from simulator studies (as described above) can depend critically on the programming of the simulator, and may not be comparable with 'gold standard' results from on-road studies. A more relevant study is the *on-road* highway investigation of the effects of a drug to treat sleep disorders (Lemborexant) by Vermeeren et al. (2019), where both a younger and an older group of drivers were instructed, where feasible, to 'maintain a constant speed of 95 km/h' (p. 3). Under placebo drug conditions, the younger drivers, with a median age of 49 years, had a mean SDLP of 17.24 cm, while the older drivers, with a median age of 67 years, had a mean SDLP of 18.42 cm (their Table 1). The age-related difference in SDLP of 1.20 cm was for an age range of 18 years. That difference could be proportionately increased to 2.00 cm for a 30-year age range, which is less than the 'clinically

relevant' criterion of 2.50 cm for the impairing effects of a psychoactive drug.

None of the research described above compares the separate effects of cannabis and aging on SDLP in a single study. A benchmark research project needs to be carried out using a large number of younger and older subjects, and using the same experimental procedures for both age groups (including constraints on slow driving), to thoroughly investigate the separate effects of both cannabis and aging on SDLP, so that the cannabis-related and age-related increases in SDLP can meaningfully be compared.

Impairment cannot be predicted from THC concentration or presence: implications for stand-alone *per se* cannabis-driving offences

The argument in favour of above-zero THC thresholds

We will first investigate the validity of the claim that the level of cannabis-induced impairment can reliably be predicted from the concentration of THC in oral fluid or blood. The lack of a quantitative relationship would invalidate the rationale for any particular above-zero THC threshold.

In the early 2000s, meetings of drug-driving experts were convened in the US (Walsh, 2002) and in Europe (Pompidou Group, 2004) to consider the introduction of zero-tolerance *per se* drug-driving offences. The Walsh meeting, which focused on illegal drugs, recommended that 'The U.S. should consider *per se* laws which prohibit driving, operating, or being in actual physical control of a motor vehicle when *any amount of a drug* is present as measured in blood, urine, saliva, or other bodily substance'. While the views of the Pompidou Group members on drug-driving interventions were diverse, one conclusion from a seminar held in 2003 was that member countries should consider introducing *per se* zero-tolerance drug-driving offences for illegal drugs (p. 379), as had already been done in a few European countries, such as in Germany in 1998 (see Moeller et al., 2000) and in Sweden in 1999 (see Jones, 2005). So, in the early 2000's, the widespread introduction of zero-tolerance *per se* drug-driving offences for illegal drugs was seriously being considered on both sides of the Atlantic.

In 2004, Franjo Grotenhermen convened an expert panel in Germany to assess whether the accumulated scientific evidence on the effect of cannabis on driving performance was sufficient to support the development of an *above-zero per se* approach to the enforcement of driving under the influence of cannabis. The panel was particularly concerned with the apparent injustice of zero-tolerance *per se* laws. In their words (Grotenhermen et al., 2005: 4):

> Many recent *per se* laws for DUID [driving under the influence of drugs] prescribe a zero tolerance for specific drugs, classifying drivers as being under the influence of a drug if any amount of a listed drug or its metabolites can be detected in blood or other body fluids. This approach effectively sets the legal limit at the technically achievable limit of detection (LOD), making the legal limit a function of the detectability of a drug, rather than the impairment caused by it. For example, under German federal law the LOD of about one nanogram per millilitre (1 ng/mL) THC in blood serum has been the *de facto* legal limit since 1999. This strict approach facilitates law enforcement, but is not based on science and does not target only impaired drivers. For example, THC is detectable in blood and urine for several hours to about two days after cannabis use, and its metabolites are detectable for days or weeks, whereas even a strong smoked cannabis dose will affect driving skills for only a few hours. Thus, zero-tolerance laws will classify many cannabis users as impaired drivers even if they separate drug use and driving by many hours. The same applies to the increasing number of individuals who legally use cannabis for medicinal purposes and, while not acutely impaired, may present with measurable THC concentrations at all times.

The Grotenhermen panel, which was strongly influenced by an impairment review conducted under the E.U. DRUID program (Berghaus et al., 2011), considered that the scientific evidence was sufficient to recommend a single, above-zero, *per se* THC threshold of 7 to 10 ng/ml. The panel findings were published as a report in 2005 and as a journal article in 2007 (Grotenhermen et al., 2005, 2007).

The argument in favour of above-zero THC thresholds cannot be sustained

Recent findings from experimental studies fail to support the early indicative evidence that was cited by Grotenhermen et al. (2005) for a dose-response relationship between THC concentration and level of impairment. Four recent examples are provided. From their U.S. study of drugs in DUI suspects, Logan et al. (2016: 2) concluded 'There is no evidence from the data collected, particularly from subjects assessed through the DRE [Drug Recognition Expert] exam, that any objective threshold exists that establishes impairment, based on THC concentrations measured in specimens collected from cannabis-positive subjects placed under arrest for impaired driving'. Similarly, from their U.S. laboratory research, Spindle et al. (2018) concluded 'Collectively, findings from this study and others indicate that blood THC concentrations are not a valid indicator of a user's intoxication and/or impairment from cannabis use, and highlight the need to explore other biological and behavioral means of detecting acute cannabis impairment' (p. 12). Evidence against the dose-response relationship can also be found in a number

of other studies, but none prior to Arkell et al.'s (2021) Australian simulator study was specifically designed to refute the validity of any above-zero THC threshold. The study investigated the relationship between THC concentrations in body fluids and levels of impairment. As noted above, the supporters of *per se* offences claim that the relationship is strong. However, Arkell et al. concluded that 'There appears to be a poor and inconsistent relationship between magnitude of impairment and THC concentrations in biological samples, meaning that *per se* limits cannot reliably discriminate between impaired and unimpaired drivers' (Abstract). An example from Arkell et al. may be helpful. In the case of alcohol, someone who is five times over the legal BAC limit of 0.05 might not be able to stand upright. However, in the case of cannabis, where the zero-tolerance legal limit could effectively be 1.0 ng/ml of THC in oral fluid, Arkell et al. (see their Figure 1) found that one of their subjects was completely unimpaired at about 500 times that limit (500 ng/ml) and another at about 800 times the limit (800 ng/ml). In the Introduction to their paper, Wurz and DeGregorio (2022) observed that 'Published research in the last several years, however, has shown that there is no clear relationship between specific blood or oral fluid concentrations of THC and impairment. In other words, there is currently no scientific justification for the use of *per se* legal limits for THC blood concentrations' (p. 1). They replicated those null findings in their own study of regular users of cannabis, by showing that impairment (as defined in terms of self-report and horizontal gaze nystagmus) could not be predicted by the concentration of THC in blood or breath.

Three recently published reviews have concluded that experimental research has failed to demonstrate any meaningful dose-response relationship between THC concentration and degree of impairment. In their broad review of *Cannabis and Driving*, Pearlson et al. (2021: 8) advised that 'While legislators may wish for data showing straightforward relationships between blood THC levels and driving impairment paralleling those of alcohol, the widely different pharmacokinetic properties of the two substances … make this goal unrealistic'. A review by McCartney et al. (2021b) focused specifically on the question of whether there was any good experimental evidence for dose-response relationships between THC concentrations in blood or oral fluid and degree of impairment. They found that there was no evidence for any such 'biomarker-performance' relationships for *regular* users of cannabis. They further found that, while there was some evidence for such relationships for *occasional* users, those relationships were 'very weak' (Abstract). They concluded (p. 8) that 'The use of *per se* limits as a means of identifying cannabis-impaired drivers should therefore be re-considered. Indeed, it seems that there is a significant risk of unimpaired individuals being mistakenly identified as 'cannabis-impaired' (and vice-versa) under this approach'. In a similar vein, a review by Blandino et al. (2022) of *Correlations between blood psychoactive drug concentrations and cognitive impairment* found that '… the relationship between blood levels [of THC] and impaired driving lacks a clear and proven direction of correlation' (p. 4).

Marilyn Huestis is one of the world's leading cannabis toxicologists. She retired in 2016 from her role as the Chief of Chemistry and Drug Metabolism at the U.S. National Institute on Drug Abuse (NIDA), where one of her main research interests was the impairing effects of cannabis. Dr Huestis is of no doubt that impairment studies have comprehensively failed to demonstrate a meaningful dose-response relationship between THC concentration and degree of impairment. In a media release (Cell Press, 2018) associated with the publication of a cannabis-toxicology review article (Huestis and Smith, 2018), Huestis said that 'There is no one blood or oral fluid concentration that can differentiate impaired and not-impaired. It's not like we need to say 'Oh, let's do some more research and give you an answer'. We already know. We've done the research'. In the same vein, the U.S. National Highway Traffic Safety Administration (NHTSA) has advised the U.S. Congress that there is a 'poor correlation of THC concentrations in the blood with impairment' from which the NHTSA concluded that 'setting *per se* levels is not meaningful' (Compton, 2017, Abstract).

We noted previously that the Norwegian government had identified drug concentrations that purportedly cause the same levels of impairment as BACs of 0.02, 0.05 and 0.12, and that, in the case of cannabis, the respective whole-blood THC concentrations were determined to be 1.3, 3.0 and 9.0 ng/ml (Verstraete et al., 2011: 15; Vindenes et al., 2012). In the light of the findings described above, this penalty system must be seen as deeply flawed.

Three policy options

Three main policy options are available to a jurisdiction that wants to implement a toxicologically-informed cannabis-driving offence regime:

1. *Per se* with a purported impairment-based THC threshold (or thresholds)
2. *Per se* with zero tolerance for any trace of THC
3. Behaviourally-based, but where behavioural indicators of impairment are supported by toxicological evidence for the presence of THC

We have seen above that the first option is not scientifically supportable. It has garnered little or no support in the U.S., or anywhere else in the world, except a few E.U. countries. Within the U.S., there are well-qualified and influential advocates for each of the other two options.

Robert DuPont is an American psychiatrist known for his strong opposition to the legalisation of marijuana - which he has described as 'the most dangerous drug'

(Dupont, 2012). He was the first Director of the National Institute on Drug Abuse (NIDA) from 1973 to 1978, and was the second 'White House Drug Czar' (i.e. Director of the White House Office of National Drug Control Policy - ONDCP) from 1973 to 1977. In 1978 he founded the Institute for Behavior and Health (IBH), which is a 'non-profit organization that develops new ideas to reduce illegal drug use' (IBH, 2021). In response to the irrefutable fact that there is no meaningful relationship between THC concentration and degree of impairment, DuPont advocates strongly for the U.S. States to adopt zero-tolerance *per se* drug-driving laws. For example, after acknowledging that 'The relationship between [drug] concentration in blood and behavior is sufficiently complex that the development of *per se* impairment levels for drugs does not appear to be practical or feasible', DuPont et al. (2012: 37 and 38) go on to advise that 'The enactment of zero-tolerance drug-impaired-driving *per se* laws for illegal drugs offers multiple possibilities for increasing the intensity of drugged-driving enforcement and creating greater deterrence of drugged driving'. The same opinion is expressed in a paper co-authored by DuPont (Reisfield et al., 2012), where the title of the paper says it all: 'The mirage of impairing drug concentration thresholds: A rationale for zero-tolerance *per se* driving-under-the-influence-of-drugs laws'. Consistent with DuPont's legacy as the Drug Czar, one of the Obama Administration's drug-control strategies was to 'Curtail drugged driving by encouraging States to establish and enforce laws that impose penalties for the *presence* of any illicit drug while driving' (ONDCP, 2010; emphasis added).

Richard Compton retired in 2019 from his role as the Director of the Office of Behavioral Safety Research at the U.S. Department of Transportation's National Highway Traffic Safety Administration (NHTSA) after more than 40 years of service with the NHTSA. In 2015, he received the National Safety Council's Robert F. Borkenstein Award - for individuals who have made outstanding contributions through a lifetime of service in the area of alcohol/drugs in relation to traffic and transportation safety; and in 2016 he received the International Council on Alcohol, Drugs and Traffic Safety's Widmark Award for an outstanding, sustained and meritorious contribution to the field of alcohol, drugs and traffic safety. In his 2017 report on behalf of the NHTSA to the U.S. Congress on *Marijuana-impaired driving*, Compton was emphatic that the published cannabis-impairment research 'does not show a relationship between THC levels and impairment' (p. 11), and that, as a consequence, 'setting *per se* levels is not meaningful' (Abstract). In contrast to DuPont, who used the lack of a dose-response relationship between THC concentration and degree of impairment to support his advocacy of zero-tolerance *per se* cannabis-driving offences, Compton went on to argue that 'Without a chemical test, the alternative is to develop a psychomotor, behavioral or cognitive test that would indicate the degree of driving impairment and elevated risk of crash involvement due to marijuana use' (p. 13). However, he admitted that no fully satisfactory behavioural test was yet available. Dr Huestis agrees with Compton's approach. In a 2015 Editorial, she advised that 'proof of impairment standards' are required to justify the imposition of serious cannabis-driving penalties, such as the loss of a driver's licence or high fines (p. 1224). And in a 2018 review, she advised that 'The development of sensitive and specific behavioural and motor impairment markers collected onsite is needed, with the cannabinoid biological markers defining the agent(s) responsible for the observed performance impairments' (Huestis and Smith, 2018: 169–170).

Blackstone's ratio

In 2004, the Australian state of Victoria became the first jurisdiction in the world to implement zero-tolerance *per se* drug-driving offences (for cannabis, methamphetamine and ecstasy) along with a large-scale random roadside drug testing (RDT) program (Quilter and McNamara, 2017). The other Australian jurisdictions all followed suit within a few years. The fairness of these programs needs to be scrutinised in relation to the jurisprudential principle established by the English judge William Blackstone over 250 years ago, which is that 'It is better that ten guilty persons escape than that one innocent suffers' (Blackstone, 1765). This principle still holds today, although the exact value of the ratio has been debated (Volokh, 1997). In the case of cannabis-presence driving penalties arising from RDT operations, the *fundamental* offence is driving while impaired, such that Blackstone's principle becomes 'It is better that ten impaired drivers escape penalty than that one unimpaired driver is penalised'. To apply a version of Blackstone's principle to Australia's RDT programs, it is necessary to estimate the proportion of unimpaired drivers who are penalised for a cannabis-presence driving offence - which we attempt to do in the following section.

Estimating the proportion of apprehended THC-positive drivers who are unimpaired

In this section we attempt to estimate proportion of apprehended THC-positive drivers who are unimpaired in the context of Australia's RDT programs, where oral fluid is the usual matrix for toxicological analysis, and cannabis is usually consumed in the form of smoked marijuana.

Estimating the proportion of drivers who are unimpaired soon after using cannabis

It has been proposed that *regular* users of cannabis are likely to be unimpaired, even soon after using, because of

the development of tolerance (Colizzi and Bhattacharyya, 2018). And it might therefore be thought that an estimate of the proportion of drivers who are unimpaired soon after using could be based on that proposition. However, as Ramaekers et al. (2016) have shown, the development of tolerance by regular users cannot be taken as a guarantee that they are unimpaired soon after using. Further to that point: a recently published study by Marcotte et al. (2022) found that when regular users were able to smoke cannabis cigarettes *ad libitum,* they absorbed sufficient THC to achieve the same level of intoxication as occasional users, and were equally impaired. So, it seems that the development of tolerance by regular users is not relevant to estimating the proportion of drivers who are unimpaired soon after using cannabis.

Three studies are known to the authors where attempts were made to *directly* estimate the proportion of cannabis users who are unimpaired by their recent use. The first estimate is from a driving-simulator study by Arkell et al. (2021) that involved 14 infrequent cannabis users, and had SDLP as its main outcome measure. The subjects were required to take a sufficient dose of cannabis, through controlled vaporization, to achieve a level of blood-THC that is above the conventional higher-level *per se* limit of 5 ng/ml. The researchers' definition of impairment was an increase in SDLP for subjects of more than 2.00 cm relative to their own placebo performance. (The researchers cited evidence that a 2.00 cm increase indicated a level of impairment equivalent to that at a BAC of 0.05). They found that 46% of their subjects were unimpaired 30 min after using cannabis.

The second estimate is from a driving-simulator study by Marcotte et al. (2022) that involved 191 cannabis users representing a broad range of use intensity. The experiment had a composite measure of driver performance as its main outcome. The subjects were randomly allocated to a THC group (n = 128) or a placebo group (n = 63). Subjects in the THC group were required to smoke cannabis cigarettes *ad libitum* to achieve their usual level of intoxication. Subjects in the placebo group smoked THC-free placebo cigarettes. Impairment for the THC-group subjects was defined in terms of placebo-group performance: subjects in the THC group were considered to be impaired if they performed worse than 85% of the subjects in the placebo group. The researchers found that 54% of THC-group subjects were unimpaired 30 min after smoking.

The third estimate is from a laboratory study by Gilman et al. (2022) that involved 169 regular users of cannabis. On one day they were dosed with synthetic THC (dronabinol) capsules, and on another, they were dosed with identical appearing placebo capsules. All participants acted as their own controls. The dose of synthetic THC 'was determined by taking a history of participants' usual use pattern, and estimating the dose when used recreationally' (p. 2). The researchers were very aware of the difficulty of establishing whether or not a participant was impaired after using cannabis. They operationalized 'ground truth impairment' in terms of both 'clinical consensus ratings' (CCRs) that involved two expert raters and an algorithm involving heart rate and self-rated intoxication. The CCRs were based on many sources of information, including the results of 'extended field sobriety tests' conducted about two hours after dosing by Drug Recognition Experts (DREs), and performance on a short-term memory task that was conducted about 100 min after dosing. The researchers concluded that '… only approximately half of the participants achieved such significant intoxication that we were confident that performance in such activities as driving would be impaired' (p. 7).

So, if drivers were to be apprehended in RDT operations soon after using cannabis, it seems that the proportion who would be unimpaired is about 50%. However, that figure fails to take into account that, for the 50% of drivers who *are* impaired soon after use, there could be a mis-match between the durations of their impairment window and their THC-positive window. An attempt is now made to estimate the extent of the mis-match.

The duration of the cannabis-impairment window

Some reviewers of the cannabis-impairment literature have provided estimates of the impairment window for smoked marijuana. For example, Jan Ramaekers, who is the current president of the International Council on Alcohol, Drugs and Traffic Safety, concluded in a 2018 overview paper that 'Performance impairments are maximal during the first hour after smoking and decline over 2 to 4 h after cannabis use' (p. 1433). The same estimate was provided by James Hedlund in a 2017 review that was conducted for the U.S. Governors Highway Safety Association, where he said that impairment from smoking marijuana 'lasts for 2 to 4 h' (p. 13). From those comments it seems that, where there is some degree of impairment from using cannabis, the impairment window is likely to be about 4 h. However, a recently published systematic review (McCartney et al., 2021a) provides longer estimates. The reviewers conclude that the impairment window is about 5 h for lower doses of THC and 7 h for higher doses. Perhaps there is no real inconsistency. The McCartney et al. review demonstrated that, for those tasks where impairments were found, they were strongest soon after using cannabis, and became progressively weaker over time. It is likely that no consequential impairments are found more than 4 h after using cannabis. For our current purpose, we will take the duration of the cannabis impairment window to be 4 h for smoked marijuana. (The impairment windows for other forms of cannabis consumption may be different).

The duration of the THC-positive-window

Three reviews have recently been published of laboratory studies that measured THC detection windows for oral fluid.

In their 2018 review, Huestis and Smith advised that 'Finding 2 ng/ml of THC in oral fluid (which is the U.S. Substance Abuse Mental Health Services Administration's (SAMHSA) proposed confirmation THC cut-off) is generally considered to be a marker of cannabis intake within the past 24 h' (p. 163). In their 2020 review, Karschner et al. noted that, in one study, 'THC was still detected in oral fluid from frequent smokers at their 72-h discharge [from the experiment], not only at the limit-of-quantification (0.2 ng/ml), but also … at the cut-off for SAMHSA (2.0 ng/ml)' (pp. 908–909). And in their 2019 review, Arnold et al. reported that, for smoked marijuana, the median 'last detection times' from the reviewed studies for THC in oral fluid ranged from 6 h to over 30 h. These three reviews show that THC can often be detected in oral fluid for up to 24 h or longer after using cannabis. However, the relevance of that conclusion to the conduct of Australia's RDT programs is difficult to assess, for the reasons discussed below.

The window for the detection of THC in oral fluid in the context of Australia's RDT programs depends on two factors that were not considered in the three reviews: police use of insensitive roadside oral-fluid screening devices, and the possible employment by enforcement agencies of confirmatory (evidentiary) oral-fluid THC cut-offs that are well above the limits-of-detection of the confirmatory equipment. The relevance of those factors to the effective duration of the THC detection window should not be under-estimated. De Castro et al. (2014, Figure 3), for example, have shown that the use of higher cut-off THC concentrations greatly reduces the effective duration of the window. Using evidentiary-standard oral-fluid analyses, they found that, at 6-h after smoking marijuana, only 9% of the participants in their experiment tested positive to THC when the cut-off was set at 25 ng/ml, compared with 73% positive at 5 ng/ml, and 100% positive at 1 ng/ml. A recent study of the drug screening devices used in Australia's RDT programs (Arkell et al., 2019a) concluded 'Overall, our data confirm that oral-fluid THC is a good indicator of *very recent* cannabis use' (p. 1492; emphasis added). And, by 'very recent use', they meant that the device was so *insensitive* (i.e. had such a high THC cut-off) that it could only reliably detect THC within about 3 h of using cannabis. Arkell et al. (p. 1492) also mentioned that other recent studies of Australia's screening device had reached much the same conclusion as to its insensitivity to all but the very recent use of cannabis. It should be noted that Arkell et al.'*s* finding seems incompatible with anecdotal accounts of THC detection windows of many days' duration from Australian court cases (Ricketts, 2018). Similarly, the 3-h window seems incompatible with the content of some Australian road-safety television media campaigns where the police who conduct the RDT programs claim that they can detect THC-positive drivers for up to 24 h after their last use of cannabis.

The second factor to be considered in relation to the duration of the THC detection window in the context of Australia's RDT programs is the possible use in some Australian jurisdictions of high confirmatory cut-offs for THC. While the oral-fluid confirmatory analyses have a limit-of-detection of 1 ng/ml or less, it seems likely that some enforcement agencies raise the confirmatory cut-offs to much higher levels, so as to not penalise drivers who used cannabis many hours prior to being apprehended. It can be seen that there is much uncertainty around the duration of the THC detection window in the context of Australia's RDT programs. The situation is not helped by the lack of information provided by the police on the THC cut-offs for their screening and confirmatory tests. Nevertheless, for the purpose of this review it is assumed that the duration of the THC detection window is only 4 h (despite the apparently contradictory evidence from some Australian court cases and road-safety advertising campaigns).

Comparing the durations of the cannabis-impairment-window and the THC-positive-window

If our estimate of the cannabis-impairment window had been shorter than our estimate of the THC-detection window, it would have been necessary to proportionately increase our estimate of the percentage of apprehended THC-positive drivers who are unimpaired. However, from the discussion above, it seems plausible that the durations are much the same (about 4 h). So, we cannot adjust our estimate of the proportion of apprehended THC-positive drivers who are unimpaired, which remains at 50%. However, that figure is based on the very conservative estimate of 4 h for the THC-detection window. If the detection window was actually 24 h, as claimed by some Australian police forces, then the proportion of THC-positive drivers who are not impaired when apprehended at RDT stations would be over 90%.

Given that at least half of the apprehended THC-positive drivers in Australia are unimpaired, the justice of Australia's cannabis-presence driving offences must be questioned. Blackstone would presumably have been appalled by such a high level of 'suffering by the innocent'.

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