Supplementary Submission No 49b

INQUIRY INTO ROAD TRANSPORT AMENDMENT (MEDICINAL CANNABIS-EXEMPTIONS FROM OFFENCES) BILL 2021

Name: Dr Michael White

Date Received: 16 June 2022

Michael White

Inquiry into Road Transport Amendment (Medicinal Cannabis-Exemption from Offences) Bill 2021
Updated version of Submission No. 49

Evaluation of roadside drug testing in Victoria

Researchers from the Monash University Accident Research Centre (MUARC), funded by an agency of the Victorian government (a point not to be overlooked), have recently conducted a second evaluation of an Australian roadside drug testing (RDT) program, and have again found it to be remarkably successful.

The purpose of this briefing paper is to investigate the credibility of the second evaluation, which relates to RDT in Victoria. My critique is attached. It refers to two published MUARC descriptions of the evaluation (Newstead *et al*., 2020; Cameron *et al.,* 2022).

I have already investigated the credibility of the first MUARC evaluation (Cameron, 2013) and found it to be seriously defective. My critique of that evaluation is provided here at Appendix A.

One reason to be sceptical about the outcomes of both evaluations is that they assume that THC-positive driving presents a major risk of crashing. But that assumption is not compatible with the conclusion of a recent epidemiological review (White & Burns, 2021).

My critique shows that the second MUARC evaluation suffers from a number of serious defects.

References

Cameron, M. (2013). *Random drug testing in Australia, analogies with RBT, and likely effects with increased intensity levels.* Paper presented at the Twentieth International Council on Alcohol, Drugs and Traffic Safety (ICADTS) Conference (T13), Brisbane, Australia, August 2013.

Cameron, M., Newstead, S., Clark, B., & Thompson, L. (2022). Evaluation of an increase in roadside drug testing in Victoria based on models of the crash effects of random and targeted roadside tests. *Journal of Safety Research, 33* (2), 17-32.

Newstead, S., Cameron, M., Thompson, L., & Clark, B. (2020). *Evaluation of the roadside drug testing expansion and roadside alcohol testing enforcement programs in Victoria* (Report number 355). Melbourne, Victoria: Monash University Accident Research Centre (MUARC).

White, M. A., & Burns, N. R. (2021). The risk of being culpable for or involved in a road crash after using cannabis: A systematic review and meta-analyses. *Drug Science, Policy and Law*, 7, 1-20.

Evaluation of roadside drug testing in Victoria: Critique of findings as reported by Newstead *et al*. (2020) and Cameron *et al*. (2022)

Dr Michael White
Adjunct Senior Fellow
School of Psychology, University of Adelaide, South Australia

ResearchGate: https://www.researchgate.net/profile/Michael_White24

Introduction

In 2004, the Australian state of Victoria was the first jurisdiction in the world to introduce stand-alone zero-tolerance *per se* drug-presence driving offences for three illegal psychoactive drugs (cannabis, methamphetamine and ecstasy), along with a large-scale program of random roadside drug testing (RDT) (Boorman & Owens, 2009; Moxham-Hall & Hughes, 2020; O'Halloran, 2010). The introduction of RDT was contrary to the advice from a national Austroads Working Group on Drugs and Driving that there was not a sufficient evidence-base to justify it (Potter, 2000). Nevertheless, the other Australian jurisdictions soon followed Victoria's lead, with none wanting to appear 'softer on drugs' than any other. So, by 2011, RDT had been established across Australia. However, the sufficiency of the evidence-base has continued to be questioned, especially in relation to cannabis (Hall, 2012; Hall & Homel, 2007; McDonald, 2009; Prichard *et al*., 2010; Quilter & McNamara, 2017; Roth, 2015). One reason to be sceptical about the success of Australia's RDT programs is that one of the two main targeted drugs, cannabis, either presents no crash risk at all, or only a very minor risk (White & Burns, 2020).

In contrast with the general level of scepticism about the necessity for, and success of, Australia's RDT programs, researchers from the Monash University Accident Research Centre (MUARC), funded by agencies of the Victorian government, have conducted two evaluations of Victoria's RDT program, and twice found it to be remarkably successful. The purpose of this briefing paper is to investigate the credibility of the two MUARC evaluations. In fact, I have already investigated the credibility of first (Cameron, 2013) and found it to be seriously defective. My critique of that evaluation is provided here at Appendix A. So, the first evaluation is only briefly revisited in this current briefing paper, which focuses on a critique of the second evaluation (Newstead *et al*., 2020; Cameron *et al.,* 2022).

Background

The timeline of an econometric model should normally involve three phases: (1) development, (2) verification and (3) application. Models that are developed from a single dataset can be idiosyncratic, such that they describe only the dataset they were derived from, and are incapable of being generalised to other datasets. For that reason, a model should be verified against a separate dataset from that used in its development, before being applied. In other words, unverified models should not be employed to estimate program outcomes; but if they are employed, the results should be viewed sceptically.

Researchers from MUARC have developed two very different econometric models to describe the relationship between levels of RDT and the *prevalence of drugs* in injured and killed drivers.

Without being verified, both models were then employed to estimate *crash savings*. As noted above, the first model was described by Cameron (2013), while the second was described in some detail by Newstead *et al.* (2020) and then summarised by Cameron *et al*. (2022).

Model 1: Cameron (2013)

Let us consider the timeline of the first MUARC RDT-evaluation model (Cameron, 2013). It was developed using a Victorian dataset for years from 2005 to 2009. It has never been verified. It was first applied in relation to Western Australia's (WA) RDT program, where Cameron concluded that the program had been very successful up to the year 2013, and that it would save many more lives and injuries if it were substantially expanded.

As I observed in my critique of Cameron (2013), which is provided here at Appendix A, this first MUARC model was developed from unrepresentative Victorian data; and, in its application to the situation in WA, involved three inappropriate assumptions that considerably over-estimated the crash-reduction benefits of the program.

The first MUARC model was next applied in the context of Cameron being invited in 2016 by the New Zealand (NZ) government to evaluate the likely success of the proposed introduction of an Australian-type RDT program. Working with the help of Dr Helen Yan Huang, an economist from the NZ Ministry of Transport, Cameron applied the model to NZ data, and concluded that the introduction of RDT would be remarkably successful. While Cameron's advice was originally accepted by the NZ authorities, they eventually came to recognise that his model was developed from cherry-picked data, and that the main assumption in its application (that drug presence in crashes equates to drug causality for those crashes) was false. So, they rejected the model, and developed and applied their own model (NZ Ministry of Transport, 2020).

Cameron and the other MUARC researchers now understand that the first MUARC model was never fit-for-purpose, and have abandoned it; but without abandoning at least one of the three untenable assumptions employed in its application (see below).

Model 2: Newstead et al. (2020) and Cameron et al. (2022)

The second MUARC RDT-evaluation model is described in Newstead *et al's* (2020) broad-ranging 135-page MUARC report. Conveniently, the main drug-related findings have been summarised in a 16-page journal article by Cameron *et al.* (2022).

This critique will show that Model 2 suffers from the following serious defects (which are explained and discussed below). The model:

1. is un-replicated, and therefore of dubious validity
2. pretends that there is no disconfirming elephant in the room in regard to trends
3. uses an incoherent independent variable in the methamphetamine sub-models
4. is based on low-quality data, from which spurious relationships might be expected
5. makes unsupportable, self-serving assumptions in its application to crash reductions

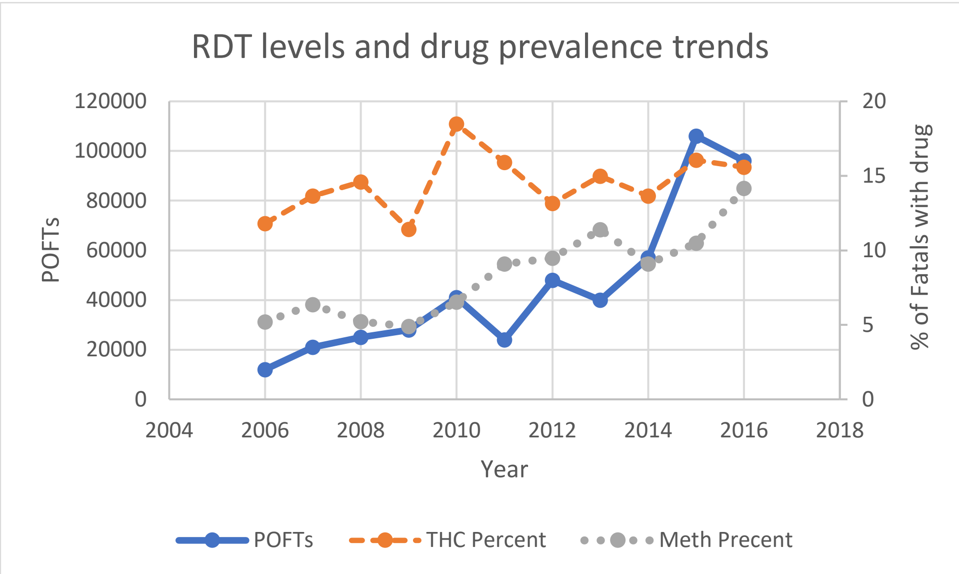
Defect 1: Model 2 is un-replicated, and therefore of dubious validity

Econometric modelling is a tricky business. Before a model can be confidently applied, it must be replicated against data that is independent of the data used in its development. In the words of Bollen *et al.* (2015, p. 2): "Scientific knowledge is cumulative. The production of each empirical finding should be viewed more as a promissory note than a final conclusion. If a scientific finding cannot be independently verified, then it cannot be regarded as an empirical fact. And if a literature contains illusory evidence rather than real findings, the efficiency of the scientific process can be compromised." We have seen how the first MUARC model is now recognised as invalid, and has been abandoned by the MUARC researchers. However, they are now spruiking a second un-replicated model. In my opinion, it is very unlikely that the second model would be replicated if re-developed from an independent set of comparable data (such as could presumably be obtained fairly easily from New South Wales or another Australian state). The MUARC researchers have been indifferent to replication. Their first model would not have survived a genuine attempt at replication, and it seems likely that the second would also fail. It follows that the second model should be considered to be exploratory, and its application to crash savings should be viewed sceptically.

Defect 2: Model 2 pretends that there is no disconfirming elephant in the room in regard to trends

The second MUARC model spectacularly fails the pub test. But that fact is glossed over by the MUARC researchers, who hope that nobody will notice the elephant in the room. Let me explain.

Figure 1. The elephant in the room: RDT levels (POFTs) and percentages of driver fatalities with either THC or methamphetamine for the years from 2006 to 2016 in Victoria



The first MUARC model (Cameron, 2013) was based on Victorian annual trends in RDT enforcement levels and the prevalence of impairing drugs in driver fatalities **for the 6-year period from 2005 to 2009**. It predicted that drug prevalences in road crash fatalities would fall over the following years as a consequence of increasing levels of RDT enforcement.

But comparable results for the **11-year period from 2006 to 2016** (Cameron *et al.,* 2022, Figures 1 and 4; re-plotted here in Figure 1) clearly show the opposite result: *both* the level of RDT enforcement *and* the proportion fatally injured drivers who test positive for methamphetamine steadily and substantially increased in Victoria. The changes for THC were similar but less pronounced. Those results clearly disconfirm Model 1's predictions, and are the main reason that the MUARC researchers have abandoned it.

The second MUARC model (Newstead *et al.*, 2020; Cameron *et al*., 2022) is based on data for the **seven years from 2010 to 2016**, which is summarised in Cameron *et al's* (2022) Figures 1 and 4, and included here in Figure 1. The fact that *increasing* levels of RDT enforcement are associated with *increasing* prevalences of proscribed drugs in fatally injured drivers was an insurmountable hurdle for Model 1 (as noted above), but is also a hurdle for Model 2. The two challenges facing the MUARC researchers in their development of Model 2 were therefore: (1) how to distract the reader's attention from the embarrassing trend information (the elephant in the room), and (2) how to develop a model that flies in the face of that reality.

The obvious 'pub test' for a successful RDT program is that it reduces the level of drug-positive driving. The first MUARC model embodied that test. So, any thoughtful reader of Newstead *et al.* (2020) or Cameron *et al.* (2022) would naturally be confused by the fact that the MUARC researchers are now trying to provide a positive evaluation of a program that has so obviously, at least at first sight, been a complete failure. One might expect that the MUARC researchers would address that predictable confusion; but they fail to do so. They presumably hope that their readers will not notice the elephant in the room, and that the best policy is therefore to pretend that nothing is problematic.

So, how did the MUARC researchers circumvent the embarrassing year-to-year trend relationships in their second model? The answer is by de-trending the data and thereby isolating the model from the trends. In other words, they basically pretended that any trend relationships between RDT levels and crashed-driver drug prevalences had nothing to do with the level of RDT enforcement. In the words of Cameron *et al.* (2022, p. 22): "'Year of Crash' was also included in the model to control for possible changes in driver drug use prevalence (unrelated to enforcement) over time". Here, the euphemistic term 'possible changes' describes the very real trend information that would have destroyed the model if it had been appropriately included. In this masterly piece of statistical legerdemain, the MUARC researchers have arbitrarily declared that changes in crashed-driver drug prevalences *within* a single year can be attributed to the effects of RDT enforcement, while changes *from year to year* cannot.

Defect 3: Model 2 uses an incoherent independent variable in the methamphetamine sub-models

The term 'Model 2' actually refers to four sub-models of the effects of RDT-enforcement on proscribed drug prevalences in crashed drivers. Each sub-model is based on only 28 data points, which describe the circumstances in 4 Victorian police regions (North-West Metro, Southern Metro, Eastern and Western) for each of the 7 years from 2010 to 2016. There are two different **independent** (RDT-enforcement) variables. One is the total number of Preliminary Oral Fluid Tests (POFTS) conducted. These tests screen for the presence of methamphetamine and cannabis. This variable was (appropriately) interpreted as a measure of *general* deterrence. The other independent variable is a measure of the 'hit rate' - which is the percentage of POFTs that are followed by confirmatory Oral Fluid Tests (OFTs). This variable was (inappropriately - see below) interpreted as a measure of *specific* deterrence. There are four **dependent** variables (crashed-driver drug-prevalences), as defined by the prevalence of either THC or methamphetamine, in either fatalities or serious injuries. After discarding a number of non-significant model relationships, the MUARC researchers settled on these four sub-models:

- Number of POFTs vs THC prevalence in fatalities
- Number of POFTs vs THC prevalence in serious injuries
- Hit rate (OFT/POFT%) vs methamphetamine prevalence in fatalities
- Hit rate (OFT/POFT%) vs methamphetamine prevalence in serious injuries

The selection the two non-obvious sub-models for methamphetamine-positive crashes was based on the statistical significance of the relationships described by the models. Models that are based on significance-chasing are unlikely to be verified when tested against independent datasets.

Let us now consider the bizarre nature of the independent (RDT-enforcement) variable for the two methamphetamine sub-models. This variable can be described as the enforcement 'hit rate'. In recent years, the hit rate has been in the vicinity of 2% for random bus-based enforcement and 16% for targeted car-based enforcement. From first principles, one might expect that the total number of screening tests (POFTs) would be the best (general) deterrent for methamphetamine-positive driving (as assumed in the first MUARC model). But that relationship was found by Newstead *et al.* (2020) to be non-significant. So, perhaps methamphetamine-positive drivers are a bit stupid, and really need to experience the pain themselves (specific deterrence). In that case, the obvious independent variable is the total number of OFTs. In other words, higher levels of specific deterrence should be the best way of deterring methamphetamine-positive drivers. Presumably, this relationship was tested for statistical significance, but failed to deliver. So, the MUARC researchers went hunting elsewhere for an effective enforcement variable, and found it in the hit rate: (OFT/POFT%).

There are three things to note about the hit rate. The first is that it is a bizarre measure of specific deterrence. According to Cameron *et al.* (2022, Table 3), each percentage point increase in the hit rate will reduce the number of methamphetamine-positive fatalities by 21% and the number of methamphetamine-positive serious injuries by 6.6%. So, the police officer in charge of one of the Victorian police regions could simply ban the use of random drug testing at bus-based stations, *without* increasing the level of car-based targeted testing, and thereby increase the hit rate from somewhere around 2% to about 16%, and thereby completely eliminate methamphetamine-related fatalities, and almost eliminate all methamphetamine-related serious injuries. This do-less-than-nothing scenario, which is advised by the two methamphetamine sub-models, is obviously ridiculous. The two sub-models are simply incoherent.

The second thing to note is that promoting the relevance of the hit rate in RDT enforcement will be welcomed by the police. They have never been comfortable with general deterrence. They have always wanted to chase the bad guys, and now have a scientific justification for doing so. By using stereotypical profiles, as described by Anderson *et al.* (2021), the police will be able to improve their hit rate in accordance with the advice inherent in the second MUARC model. Young tradespeople with tattoos who are on P-plates can anticipate increased levels of police harassment.

The third thing to note is that concentrating on the hit rate flies in the face of accepted road-safety enforcement practice, which is based on the theory of general deterrence, as expounded by Homel (1988), and others, and which is considered to be responsible for the remarkable progress in Australia in reducing the prevalence of drink driving through the introduction of random roadside breath testing (Henstridge, Homel & McKay, 1997). It would be a pity to reject that wisdom on the basis of an unverified model. Having said that, it is possible that the police *should* invest more in specific deterrence. The point being made here is that any such radical re-direction of police resources should not seek its justification in Newstead *et al's* (2020) unverified and implausible model.

Defect 4: Model 2 is based on low-quality data, from which spurious relationships might be expected

Fifty years ago, the president of the Royal Economic Society, observed that: "… running regressions between time-series is only likely to deceive" (Phelps Brown, 1972). Variables that are repeatedly measured across the years can co-vary for obscure reasons. While the four MUARC sub-models do not actually involve time-series analyses, they do involve time-series data: the 28 data-points for each sub-model are measured over a 7-year period (with the 4 data-points for each year representing the four police regions). It is likely that the sub-models developed from that data incorporate inscrutable co-variances, and thereby give rise to spurious findings.

Another reason to treat the two serious-injury sub-models with suspicion is that the crashed-driver drug-prevalence data is mostly missing. The sampled (drug-tested) drivers comprise only about one-third of the seriously-injured driver population. The sampling is unlikely to be random and representative. So, the two sub-models that have serious-injury outcomes are built on shaky foundations. It is possible that any biases involved in selective sampling could have varied in strength from year to year over the study period. The finding of spurious relationships for the two serious-injury sub-models should therefore not be surprising.

Defect 5: Model 2 makes unsupportable, self-serving assumptions in its application to crash reductions

If one assumes that the second MUARC model is scientifically rigorous (which I do not), then the next thing to consider is how it has been *applied* to the prediction of crash savings from increased levels of RDT enforcement. The application of both the first and second MUARC models has involved making assumptions to help translate the modelled changes in *crashed-driver drug prevalences* (as determined in the developmental phase) into *crash savings* (as predicted in the application phase). Cameron (2013) employed three translational assumptions in relation to the first MUARC model:

1. the equivalence assumption
2. the all impairing-drugs assumption
3. the co-use of alcohol assumption

I have shown (see Appendix A) that all three assumptions in the first MUARC model mis-represent the real world in ways that grossly exaggerate predicted crash savings.

The first translational assumption is that drug prevalence in crashed drivers is equivalent to drug causation of those crashes. For example, consider 100 THC-positive crashed drivers; Assumption 1 claims that all 100 of those crashes were caused by the drivers' use of cannabis. I discuss this assumption, which is scientific nonsense, in Appendix A.

The MUARC researchers refer to the second MUARC model as 'TERAM' (for Traffic Enforcement Resource Allocation Model). Despite TERAM's central role in the estimation of crash savings, Cameron *et al.* (2022) decided to say almost nothing about its application to crash savings in their article in the *Journal of Road Safety*. The assumptions incorporated into TERAM's prediction of crash savings from increased levels of RDT enforcement are simply not identified. The editors of the journal were seriously negligent in allowing that critical information to slip below the radar. However, the authors would presumably have been happy to avoid any scrutiny of their felicitous assumptions. Instead of identifying the assumptions, Cameron *et al.* (2022, p. 26) direct the reader to Section 3.1.4 of Newstead *et al's* (2020) lengthy MUARC report, for "further details of the results of the modelling analysis".

So, let's play hide and seek. The obvious place to start is Section 3.1.4 of Newstead *et al*. (2020). However, Section 3.1.4 covers only the *development* of TERAM, not its *application* to crash savings. The application of TERAM is discussed later, in Section 6, which deals with the *Impact of TAC-funded roadside drug testing increases on road trauma*. So, Section 6 is where the reader might expect to be informed about the assumptions used in the prediction of crash savings. But Section 6 is also mostly silent on that topic. However, there is a cryptic clue to the deployment of Assumption 1 in the following text (p. 77):

> The increase in the total number of POFTs (random or targeted) drives the savings in fatal and hospitalisation crashes due to reductions in THC presence in fatally injured and seriously injured vehicle controllers. The increase in the detection rate from all testing operations (random or targeted) drives the savings in crashes due to reductions in Methamphetamine (Meth) presence in the vehicle controller casualties.

If you blinked, you would miss it, but the involvement of Assumption 1 is subtly embedded in this text, where the very real distinction between the prevalence of drugs in crashed drivers and the causal role of the drugs in the crashes is glossed over.

The second assumption in the first MUARC model is that RDT would not only reduce the prevalence of the *targeted proscribed illegal* drugs in drivers, but would also reduce the prevalence of *non-targeted medical* psychoactive drugs such as benzodiazepines (for anxiety) and opioids (for pain). However, it seems unlikely that this outlandish idea has survived for long enough to be incorporated into the second MUARC model.

The third assumption in the first MUARC model relates to the crashed driver's co-use of alcohol with either cannabis or methamphetamine. In such instances, Assumption 3 attributes the cause of the crash to the illegal drug rather than to the alcohol. This again, is scientific nonsense, as I have discussed in Appendix A. While this assumption is most probably deployed in the second MUARC model, I could not find explicit mention of it in either Newstead *et al*. (2020) or Cameron *et al.* (2022).

References

Anderson, L., Love, S., Freeman, J., & Davey, J. (2021). Hit and miss: A comparison of targeted and randomised roadside drug testing (RDT). *Policing: An International Journal, 44* (6), 1154-1167.

Bollen, K., Cacioppo, J. T., Kaplan, R. M., Krosnick, J. A., & Olds, J. L. (2015). *Social, behavioral and economic sciences perspectives on robust sand reliable science.* Alexandria, Virginia: National Science Foundation.

Boorman, M., & Owens, K. (2009). The Victorian legislative framework for the random testing of drivers at the roadside for the presence of illicit drugs: An evaluation of the characteristics of drivers detected from 2004 to 2006. *Traffic Injury Prevention, 10*, 16-22.

Cameron, M. (2013). *Random drug testing in Australia, analogies with RBT, and likely effects with increased intensity levels.* Paper presented at the Twentieth International Council on Alcohol, Drugs and Traffic Safety (ICADTS) Conference (T13), Brisbane, Australia, August 2013.

Cameron, M., Newstead, S., Clark, B., & Thompson, L. (2022). Evaluation of an increase in roadside drug testing in Victoria based on models of the crash effects of random and targeted roadside tests. *Journal of Safety Research, 33* (2), 17-32.

Hall, W. (2012). Driving while under the influence of cannabis: It is currently unclear whether roadside drug resting reduces cannabis impaired driving. *British Medical Journal, 344:e595*, 1-2.

Hall, W., & Homel, R. (2007). Reducing cannabis-impaired driving: Is there sufficient evidence for drug testing of drivers? *Addiction, 102* (12), 1918-1919.

Henstridge, J., Homel, R, & McKay, P. (1997). *The long-term effects of random breath testing in four Australian states: A time-series analysis* (Report Number 162). Canberra, Australia: Federal Office of Roads Safety.

Homel, R. (1988). *Policing and punishing the drinking driver: A study of general and specific deterrence*. New York: Springer-Verlag.

McDonald, D. (2009). The policy context of roadside drug testing. *Journal of the Australasian College of Road Safety, 20* (1), 37-43.

Moxham-Hall, V., & Hughes, C. (2020). *Drug driving laws in Australia: What are they and why do they matter?* (Drug Policy Modelling Program (DPMP) Bulletin Number 24). Sydney, New South Wales, Australia: University of NSW Social Policy Research Centre.

Newstead, S., Cameron, M., Thompson, L., & Clark, B. (2020). *Evaluation of the roadside drug testing expansion and roadside alcohol testing enforcement programs in Victoria* (Report number 355). Melbourne, Victoria: Monash University Accident Research Centre (MUARC).

New Zealand Ministry of Transport. (April, 2020). *Enhanced testing regime for drug-impaired driving: Cost-benefit analysis*. Wellington: Ministry of Transport. Retrieved from: www.transport.govt.nz.

O'Halloran, S. J. (2010). *Forensic science, expertise and evidence at the boundary of legislation, policy and politics: A case study of legislative, political and policy responses to the effects of drugs on road safety in Victoria and other Australian states* (Doctoral thesis, University of Western Australia, Perth, Western Australia).

Phelps-Brown, E. H. (1972). The underdevelopment of economics. *The Economic Journal, 82*, 1-10.

Potter, J. (On behalf of the members of the Austroads Working Group on Drugs and Driving). (2000). *Drugs and driving in Australia* (Report Number AP-R172). Sydney, Australia: Austroads.

Prichard, J., Matthews, A., Bruno, R., Rayment, K., & James, H. (2010). Detouring civil liberties? Drug-driving laws in Australia. *Griffith Law Review, 19* (2), 330-349.

Quilter, J., & McNamara, L. (2017). 'Zero tolerance' drug-driving laws in Australia: A gap between rationale and form. *International Journal for Crime, Justice and Social Democracy, 6* (3), 47-71.

Roth, A. (2015). The uneasy case for marijuana as chemical impairment under a science-based jurisprudence of dangerousness. *California Law Review, 103* (4), 840-918.

White, M. A., & Burns, N. R. (2021). The risk of being culpable for or involved in a road crash after using cannabis: A systematic review and meta-analyses. *Drug Science, Policy and Law*, 7, 1-20.

Draft: 9 June 2022

APPENDIX A: Critique of Cameron (2013)

> A chemist, a physicist and an economist are marooned on a desert island without food. Suddenly they discover a cache of canned goods, but there is no can-opener. The chemist begins looking about for chemicals in their natural state so he can make up a solution that will dissolve the tops of the cans. The physicist picks up a rock and begins calculating what angle, what force, what velocity he will need to strike the can with the rock in order to force it open. The economist merely picks up a can and says, "Let us assume we have a can-opener". (Dundes, 1977, p. 775)

Introduction

Professor Max Cameron from the Monash University Accident Research Centre (MUARC) presented a paper in Brisbane at the 2013 conference of the International Council on Alcohol, Drugs and Traffic Safety (ICADTS) in which he provided an economic assessment of costs and benefits (CBA) of Western Australia's roadside drug testing (RDT) program. Cameron concluded that the program had been very successful up to 2013 and that it should be substantially expanded. He used four assumptions in reaching those conclusions:

1. the downward-slope assumption
2. the equivalence assumption
3. the all impairing-drugs assumption
4. the co-use of alcohol assumption

These four assumptions are scrutinised in this critique. The first is used in the *development* of the model that describes how an increased level of RDT enforcement leads to reduced drug prevalences in crashed drivers. The other three are used in the *application* of the model to the prediction of crash reductions.

Assumption 1: The downward-slope assumption

Cameron's 2013 evaluation of the effectiveness of RDT in Western Australia was founded on the fact that there was an inverse relationship between the levels of RDT in Victoria for the five years from 2005 to 2009 (as measured by the annual number of roadside oral-fluid tests [ROFTs] for proscribed illegal drugs) and the prevalence of impairing (both legal and illegal) drugs in vehicle operators (drivers and riders) who were killed in Victorian road crashes over the same period. He describes this relationship in terms of a negative exponential function. There is a sense in which his positive evaluation of the Western Australian RDT program had already been achieved once the downward slope had been established, because the rest of Cameron's evaluation involved little more than inserting parametric values into the function.

Cameron could have argued from first principles for a downward slope, as was done in a recent cost-benefit analysis (CBA) of a proposed RDT program for New Zealand (Ministry of Transport, 2020). However, he apparently believed that his RDT evaluation would be more credible if the downward slope were derived empirically. In other words, he wanted his evaluation to appear to be ‘scientific’ rather than merely exploratory. He did not want it to appear that he was simply assuming the truth of a desired outcome (as was done in the New Zealand CBA).

However, finding an empirical basis for the downward slope was not an easy task. Over the years from 2004 (when RDT was introduced into Victoria) to 2013 (the date of Cameron’s paper) the level of RDT was steadily increasing across Australia (e.g., Newstead et al., 2020, Figure 2; Rowden et al., 2011, Table 1; Thompson, 2012). At the same time, the proportion of road-crash victims who tested positive for illegal drugs was also generally increasing, and especially for methamphetamine (e.g., Baldock & Lindsay, 2020; Centre for Road Safety, 2017, Section 1.1.2; Davey et al., 2020; Newstead et al, 2020, Figures 6 & 7; Schumann et al., 2021). So, for most of the empirical functions that Cameron might have uncovered, the slope would not have been in the ‘right’ direction. Nevertheless, Cameron discovered that if he selected data from Victoria, for the five years from 2005 to 2009, he would obtain the felicitous downward slope. If he had included Victorian data for four additional years (i.e., from 2005 to 2013), he would have found an infelicitous upward slope (White, 2017), and his positive evaluation would have collapsed. And if he had used any five years of data from any other state he would also, most probably, have found the ‘wrong’ slope.

Despite creating the appearance of an empirical basis for his evaluation, Cameron had effectively based it on the speculative existence of a downward slope. But the real slope might actually *not* be downward. It is perfectly feasible that RDT is ineffective (and the ‘slope’, all other things being equal, is therefore horizontal). It is feasible that the tight analogy with roadside breath testing for alcohol (RBT), as proffered by Cameron (2013), simply does not hold. It is possible that illegal drug users, who are already breaking the law, are not deterred by RDT.

While Cameron’s pseudo-empirical downward-slope guaranteed the success of his Western Australian RDT evaluation, the *extent* of that success, in terms of lives saved and costs reduced, depended critically on three other assumptions. While the specific shape of the downward-slope function as identified by Cameron could conceivably be more-or-less correct, it will be shown that his three other assumptions make extravagant claims. As a consequence, Cameron has considerably over-estimated the potential success of Western Australia’s RDT program.

Assumption 2: The equivalence assumption

The second of Cameron’s (2013) assumptions, in a nutshell, is that *prevalence* is equivalent to *causality*. More specifically, the assumption is that presence of an impairing drug in a fatally injured vehicle operator *always* indicates that the drug caused the crash. Consequently, a reduction in prevalence is equivalent to a reduction in the number of fatal crashes. The evidence that Cameron (2013) makes this extraordinary assumption can be seen in his Table 1, where he describes the numbers in the third column as “driver fatalities saved”, when what he actually provides is “driver fatalities who are now drug-free”. The assumption is obviously false: The prevalence of an impairing drug in crashed vehicle operators is clearly not equivalent to the drug’s level of causality for the crashes. In other words, the fact that a drug is present in x% of fatally injured vehicle operators does not mean that the drug caused all x% of those crashes. The equivalence assumption might hold true for very high levels of alcohol, but, as discussed below, it does not hold for the psychoactive drugs of interest (cannabis, methamphetamine, ecstasy and some medical drugs).

The relationship between the prevalence of a drug in crashed drivers and the causal role of the drug in those crashes is mediated by the drug-crash odds ratio (OR). I have described this well-known relationship in an unpublished working paper (White, 2018). White and Burns (2021) argue that the cannabis-crash OR could be as low as 1.00, in which case *none* of the crashes involving THC-positive drivers would have been caused by cannabis. However, White and Burns admit that most researchers in the field would probably still estimate the cannabis-crash OR at up to 1.50, in which case (by reference to White, 2018) one third (33.3%) of the crashes involving THC-positive drivers would have been caused by cannabis. That is still far less than the 100% assumed by Cameron.

Drummer et al. (2004) investigated the role of psychoactive drugs in fatal Australian road crashes. They estimated that the OR for *any* psychoactive drug (apart from alcohol) was 1.80, in which case (by reference to White, 2018) only 44% of the fatal crashes in their study involving drug-positive drivers would have been caused by those drugs. By ignoring the fact that prevalence is not equivalent to causation, Cameron (2013) has over-estimated the benefits to be obtained from RDT by at least a factor of two. So, to account for Cameron's failure in relation to Assumption 2, any benefits found for the Western Australian RDT program would have to be at least halved.

Evaluators, and especially economic-evaluators, have an ethical obligation to clearly state all of their assumptions. Cameron (2013) failed to do so. While his first assumption was described clearly, his second (as discussed above) and the following two would probably not have been noticed by many of his intended audience (including policy-makers).

Assumption 3: The all impairing-drugs assumption

There are different types of psychoactive drugs. The set of all impairing drugs includes the set of proscribed illegal drugs (cannabis, methamphetamine and ecstasy) as well as the set of prescribed medical drugs, such as benzodiazepines (for anxiety) and opioids (for pain).

In the first four columns of Table 2, Cameron (2013) shows how an increase in the level of RDT in Western Australia would, in accordance with the downward-sloping function *for all impairing drugs*, reduce the number of "driver fatalities with impairing drugs". It is important to note that the drivers being saved by the increasing levels of RDT are all of those who would otherwise have used *any* impairing drugs, and not just those who had used the illegal drugs that are targeted in the RDT program. In other words, Cameron has assumed that the RDT program will not only deter driving after using the targeted illegal drugs, but it will also deter driving after taking non-targeted prescribed psychoactive drugs for anxiety, pain and other conditions. That assumption does not pass the pub test. There is no evidence for it in the relevant literature. The only supporting evidence comes Cameron's five years of carefully selected Victorian data.

To summarise: the 'impairing drugs assumption' actually comprises two separate assumptions. The first is that, as for the proscribed illegal drugs, where a prescribed psychoactive drug is present in a crashed driver, the crash was always caused by the drug. The second is that, as well as being effective in reducing the prevalence of proscribed illegal drugs in drivers, RDT operations are also effective in reducing the prevalence of prescribed psychoactive drugs.

From Cameron's (2013) Figure 2, it can be seen that prescribed medical drugs comprise about 40% of all impairing drugs. If we assume that Cameron's third assumption is wrong, and that

RDT operations do not deter driving after taking psychoactive medications, then Cameron's RDT evaluation will have over-estimated the number of fatalities saved by about 40%.

Assumption 4: The co-use of alcohol assumption

On page 6 of his 2013 paper, Cameron says that he will evaluate the success of the Western Australian RDT program in terms of the saved lives of "all victims" involved in crashes where the killed drivers "had impairing drugs in their bloodstream". He failed to make it explicit that the killed drivers were of two types: those who had used drugs alone, and those who had co-used drugs with alcohol.

Now, it is very well known that, where alcohol is involved in fatal crashes, the BACs are likely to be very high, such that the corresponding alcohol-crash ORs are also very high. For example, Drummer et al. (2003, p. 157) reported a median BAC of 0.17 for Australian road-crash fatalities. And most of the BACs were above 0.10 (see Drummer et al's Figure 1). In other words, if alcohol is involved in a fatal crash, it will usually be at such a high BAC that the alcohol alone would have caused the crash. Again, Cameron's attribution of 100% causality to the drugs involved in fatal crashes where the driver was a co-user of alcohol does not pass the pub test. It is perhaps not surprising that Cameron failed to state the assumption explicitly.

For the purpose of this investigation, it will be conservatively assumed that, where a driver in a fatal crash is a co-user of alcohol and another psychoactive drug, that it is the alcohol rather than the other drug that caused the crash for 80% of the crashes. In Drummer et al's (2003, pp 157-158) study of fatal Australian crashes, 23.5% of the drivers were positive for impairing drugs (other than alcohol). Of those drivers, about 40% were also positive for alcohol. In making the incorrect assumption that co-use crashes can always be attributed to the drug involved, Cameron has therefore over-estimated the lives saved from RDT by 32% (80% of 40%). For the purpose of this investigation, that figure will be taken to be 30%.

Summary

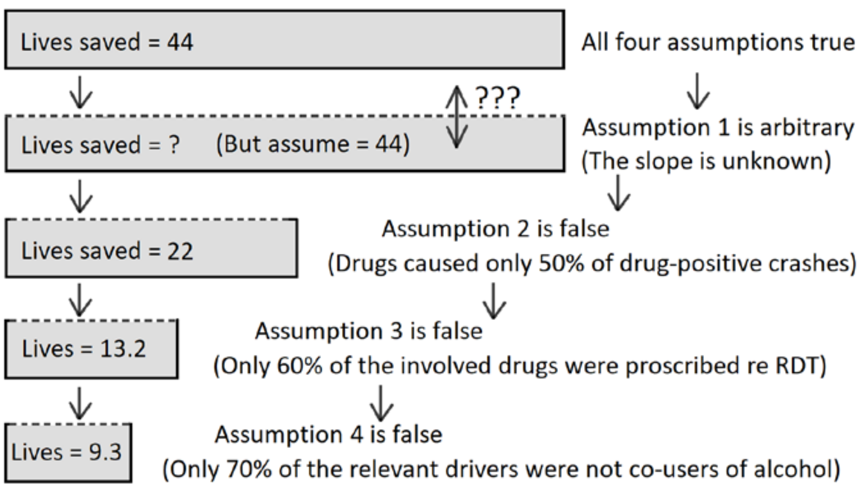


Figure *1: Accounting for the falsity of the assumptions in Cameron's (2013) evaluation of lives saved from an extension of Western Australia's RDT program*

Figure 1 summarises the results of the above scrutiny of the four assumptions that are embedded in Cameron's (2013) estimation of the likely success of expanding Western Australia's RDT program. The greatest additional (above base-level) saving of lives would occur if all four assumptions were true. Assuming their truth, the maximum number of lives saved is set by the level of RDT, in accordance with the shape of the downward-sloping function. That number is represented by the size of the rectangle at the top of Figure 1. According to Cameron's Table 1, forty-four additional lives would be saved if the level of RTD were increased from 0.54% to 12. 5% of all licenced drivers being tested at RDT sites per year.

Assumption 1 asserts that the 'seeding function' (on which the evaluation is grounded) has a downward slope of a particular shape. However, that function was carefully selected. The slope is probably too steep, resulting in an over-estimate of the number of lives saved. The arbitrariness of Assumption 1 is depicted by the unknown location of the dotted top side of the rectangle. So, the maximum number of lives saved, if Assumptions 2. 3 and 4 were true, is actually unknown. Assumptions 2, 3 and 4 all provide over-estimates the number of lives saved, to the extents indicated by the diminishing lengths of the rectangular boxes. Overall, Cameron's evaluation of the likely benefits from Western Australia's RDT program, if Assumption 1 were true, has produced at least a four-fold greater benefit than is realistic: the number of lives saved would be less than 10, rather than the 44 claimed.

Discussion

Cameron's (2013) evaluation of Western Australia's RDT program found that it had been very successful up to 2013, and that it would be even more successful if it were substantially expanded. The number of roadside drug tests in Western Australia more than tripled in the two years following his evaluation (BITRE, 2022), and it is very likely that his enthusiastic endorsement of the RDT program facilitated its expansion.

Cameron's (2013) evaluation of RDT in Western Australia was based on four assumptions. In this critique, I conclude that his foundational assumption is arbitrary, while his three supporting assumptions have the effect of grossly over-estimating any benefits of the program.

Cameron's (2013) approach to the evaluation of RDT has been influential beyond Western Australia. For example, he was invited by the New Zealand government in 2016 to provide advice on the possible introduction of an Australian-style RDT program. He applied his evaluation methodology to the New Zealand crash data, and concluded that the New Zealand program would be very successful. The New Zealand government has since introduced an RDT program, and it seems likely that Cameron's involvement was facilitative. However, as an aside, it should be noted that, while the New Zealand authorities at first accepted Cameron's evaluation methodology, they eventually came to understand that its assumptions were not valid, and they developed their own alternative approach (which also grossly over-estimated the potential benefits from the program - but that is a different story for another day).

In their recent economic evaluation of Victoria's RDT program, Stuart Newstead et al. (2020) conclude that the program so far has been very successful, and that it should be substantially expanded. Cameron was one of four members of Newstead et al's evaluation team, and had a strong influence on the design of the data analysis, including through the re-deployment of his fanciful assumptions.

Cameron's work has not passed any journal-level peer-review process, and his discredited analyses should not be allowed to provide further direction to policy-makers who are trying to understand the costs and benefits of Australia's RDT programs.

References

Baldock, M., & Lindsay, T. (2020). Illicit drugs are now more common than alcohol among South Australian crash-involved drivers and riders. *Traffic Injury Prevention, 21* (1), 1-6.

BITRE (Bureau of Infrastructure and Transport Research Economics). *RDT data available at:* https://data.gov.au/dataset/ds-dga-2b299428-c626-4446-88ef-54930b8a5e9b/details?q= (Accessed 25 April 2022)

Cameron, M. (2013). *Random drug testing in Australia, analogies with RBT, and likely effects with increased intensity levels.* Paper presented at the Twentieth International Council on Alcohol, Drugs and Traffic Safety (ICADTS) Conference (T13), Brisbane, Australia, August 2013.

Centre for Road Safety. (2017). *Drug driving trauma trends report*. Sydney, New South Wales: NSW Government, Transport for NSW.

Davey, J. D., Armstrong, K. A., Freeman, J. E., & Parkes, A. (2020). Alcohol and illicit substances associated with fatal crashes in Queensland: An examination of the 2011 to 2015 Coroner's findings. *Forensic Science International, 312*, Article 110190.

Drummer, O. H., Gerostamoulos, D., Batziris, H., Chu, M., Caplehorn, J. R. M., Robertson, M. D., & Swann, P. (2003). The incidence of drugs in drivers killed in Australian road traffic crashes. *Forensic Science International, 134*, 154-162.

Drummer, O. H., Gerostamoulos, D., Batziris, H., Chu, M., Caplehorn, J. R. M., Robertson, M. D., & Swann, P. (2004). The involvement of drugs in drivers of motor vehicles killed in Australian road traffic crashes. *Accident Analysis and Prevention, 36*, 239-248.

Dundes, A. (22-29 December, 1977). Science in folklore? Folklore in Science? *New Scientist, 76* (1083), 774-776.

Newstead, S., Cameron, M., Thompson, L., & Clark, B. (2020). *Evaluation of the roadside drug testing expansion and roadside alcohol testing enforcement programs in Victoria* (Report number 355). Melbourne, Victoria: Monash University Accident Research Centre (MUARC).

New Zealand Ministry of Transport. (April, 2020). *Enhanced testing regime for drug-impaired driving: Cost-benefit analysis*. Wellington: Ministry of Transport. Retrieved from: www.transport.govt.nz.

Rowden, P., Mazurski, E., Withaneachi, D., & Stevens, M. (2011). *Roadside drug testing in New South Wales*. Paper presented at the Australasian Road Safety Research, Policing and Education Conference, November 2011, Perth, Western Australia.

Schumann, J., Perkins, M., Dietze, P., Nambiar, D., Mitra, B., Gerostamoulos, D., Drummer, O. H., Cameron, P., Smith, K., & Beck, B. (2021). The prevalence of alcohol and other drugs in fatal crashes in Victoria, Australia. *Accident Analysis and Prevention, 153*, Article 105905.

Thompson, P. (2012). *Changing trends of drug driving detections in South Australia*. Paper presented at the Australasian Road Safety Research, Policing and Education Conference, 4-6 October 2012, Wellington, New Zealand.

White, M. (2017). *Investigation of an evaluation of Western Australia's roadside drug testing program*. Unpublished paper. Available from the author at: mawhite8@internode.on.net

White, M. (2018). *Converting an agent-outcome relative risk (RR) to the probability that the agent caused the outcome (probability of causation 'PC') with a worked example*. Unpublished paper. Available from the author at: mawhite8@internode.on.net

White, M. A., & Burns, N. R. (2021). The risk of being culpable for or involved in a road crash after using cannabis: A systematic review and meta-analyses. *Drug Science, Policy and Law*, 7, 1-20.