ADDICTION

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REVIEW

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# The effects of cannabis intoxication on motor vehicle collision revisited and revised

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## ABSTRACT

**Aims** To determine whether and to what extent acute cannabis intoxication increases motor vehicle crash risk. **Design** Study 1 replicates two published meta-analyses, correcting for methodological shortcomings. Study 2 is an updated meta-analysis using 28 estimates from 21 observational studies. These included studies from three earlier reviews, supplemented by results from a structured search in Web of Science and Google Scholar, and by the personal libraries of the research team. Risk estimates were combined using random-effects models and meta-regression techniques. **Setting** Study 1 replicates the analysis of Asbridge *et al.*, based on nine studies from five countries, published 1982–2007; and Li *et al.*, based on nine studies from six countries, published 2001–10. Study 2 involves studies from 13 countries published in the period 1982–2015. **Participants** In study 1, total counts extracted totalled 50 877 (27 967 cases, 22 910 controls) for Asbridge *et al.* and 93 229 (4236 cases and 88 993 controls) for Li *et al.* Study 2 used confounder-adjusted estimates where available (combined sample size of 222 511) and crude counts from the remainder (17 228 total counts), giving a combined sample count of 239 739. **Measurements** Odds ratios (OR) were used from case–control studies and adjusted OR analogues from culpability studies. The impact of the substantial variation in confounder adjustment was explored in subsample analyses. **Findings** Study 1 substantially revises previous risk estimates downwards, with both the originally reported point estimates lying outside the revised confidence interval. Revised estimates were similar to those of study 2, which found cannabis-impaired driving associated with a statistically significant risk increase of low-to-moderate magnitude [random-effects model OR 1.36 (1.15–1.61), meta-regression OR 1.22 (1.1–1.36)]. Subsample analyses found higher OR estimates for case–control studies, low study quality, limited control of confounders, medium-quality use data and not controlling for alcohol intoxication. **Conclusions** Acute cannabis intoxication is associated with a statistically significant increase in motor vehicle crash risk. The increase is of low to medium magnitude. Remaining selection effects in the studies used may limit causal interpretation of the pooled estimates.

**Keywords** Cannabis, case-control, culpability, driving, DUI, driving under the influence, impairment, marijuana, meta-analysis.

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## INTRODUCTION

The raised traffic crash risks of cannabis-impaired drivers has received increasing attention from researchers and policymakers following legislative changes for medical and recreational cannabis, particularly in US states. Researchers have highlighted cannabis-impaired driving as one of the three 'primary reasons for concern about legalized cannabis' [1], and the current consensus, as summarized in a recent narrative review of cannabis research, holds that the evidence 'suggests strongly' that cannabis-impaired driving increases the crash risk two to three times [2].

The evidence for this claim includes both laboratory and epidemiological research. Experimental studies find evidence of dose-related impairment on a number of driving-relevant abilities, with a typical duration of 3–4 hours following intake through smoking [3,4], but also find that cannabis users tend to be aware of, and to some extent compensate for, these impairments when driving. Overall, the external validity of these studies remains unclear, necessitating the use of observational epidemiological studies to assess the net traffic risk of cannabis intoxication. This requires the use of meta-analytical techniques that

pool evidence from different studies, as individual studies tend to be small and find strongly differing risk estimates. The most widely referenced meta-reviews are both from 2012, and report summary odds ratios (OR) from pooled studies of 1.92 and 2.66 [5,6], each based on a set of nine estimates.

The present meta-review was motivated by two concerns: first, we suspected that methodological issues, particularly relating to established confounders, were addressed insufficiently in the earlier meta-analyses. For this reason, we replicated the analyses of Asbridge *et al.* [5] and Li *et al.* [6], correcting for a set of identified methodological issues (study 1). Secondly, the evidence base has grown rapidly in recent years as a result of increased research attention. To address this, we performed an updated and more comprehensive meta-analysis that identified a total of 28 estimates from 21 studies (study 2).

## STUDY 1: A CRITIQUE AND RE-ANALYSIS OF PREVIOUS META-ANALYSES

### Study selection and comparability

Meta-analyses are meaningful to the extent that the underlying studies can yield comparable estimates of the effect of interest.

In Li *et al.*'s [6], study selection criteria are unclear and hard to rationalize. The pooled studies report qualitatively different types of associations: self-reported crashes in some past period for cannabis ever-users versus never-users [7,8], self-reported crashes in a past period for those with self-reported intoxicated driving episodes in the past versus those without [9,10] and acute intoxication among crash-involved and other motorists [11–15]. In addition to the difference in outcome measures, this means that habitual cannabis users who do not drive while intoxicated are placed in the exposed counts extracted from some studies and the control counts extracted from others. The review does not discuss how these differences affect the interpretation of the pooled estimate.

The study selection criteria in Asbridge *et al.* [5] are stated clearly: studies on the association between acute intoxication and traffic crashes resulting in serious injuries or fatalities. To avoid confounding from alcohol, counts are extracted from 'no-alcohol' subsamples of the underlying studies.

While the research question in the studies pooled by Asbridge *et al.* are similar, the studies pooled use two distinct methodological approaches that yield incompatible estimators: culpability studies and case–control studies. A case–control study compares the ratio of intoxicated to non-intoxicated drivers among those involved in crashes to the same ratio among those not involved in crashes. This estimates the increased crash risk associated with acute use. A culpability study uses data on crash-involved drivers only, and compares the ratio of intoxicated to non-intoxicated drivers among those judged culpable for their crash to the same ratio among those not judged culpable. While this is interpreted commonly as an estimate of the increase in crash risk, it is actually an estimate of the increased risk of culpable accidents associated with acute use, which will necessarily be higher than the overall increase in crash risk.

To see this, note that the identifying assumption of culpability studies is: 'that drivers found non-culpable after a car crash represent a random sample of the general driving population' [16]. By this assumption, the denominator in the OR estimator of case–control studies can be proxied by the intoxication odds of the non-culpable drivers, yielding the estimator:

$$\hat{\theta}_A = \frac{(culp_+ + nonculp_+)/(culp_- + nonculp_-)}{nonculp_+/nonculp_-}$$

This estimator can be compared to the one used in culpability studies.

$$\hat{\theta}_B = \frac{culp_+/culp_-}{nonculp_+/nonculp_-}$$

A simple simulation exercise comparing these two estimators shows that the traditional culpability estimator is biased upwards relative to the underlying true value (see Fig. 1). The explanation lies in the identifying assumption of culpability studies, which requires that non-culpable crashes were random and not due to intoxication. Consequently, an intoxicated driver would have the same number of non-culpable, but a raised number of culpable accidents, and the risk increase has to be multiplied by the baseline culpability share.[1] Actual baseline culpability rates in the culpability studies used in Asbridge *et al.* ranged from 42 to 76%.

### Data extraction

We re-extracted counts from the individual studies used in Asbridge *et al.* and Li *et al.* In some cases, the extracted numbers could not be inferred from or control-checked using the underlying studies, and the original study authors were contacted directly.

For Asbridge *et al.*, this resulted in substantial changes ($> 10\%$) of extracted OR estimates for four of the nine studies used, all of which were adjusted downwards. For Blows *et al.* [12], Asbridge *et al.* misinterpreted a published table. Accurate counts of cannabis-only and non-intoxicated case and controls had to be requested from the authors of the underlying study, yielding an OR estimate of 2.6 rather

[1]For adjusted ORs controlling for known confounders, we use $\theta_A = \theta_B \cdot culp + (1 - culp)$, where culp is the baseline culpability rate in non-intoxicated drivers. This correction fails to reduce the estimator's standard error appropriately, but is sufficient for our purposes.

 

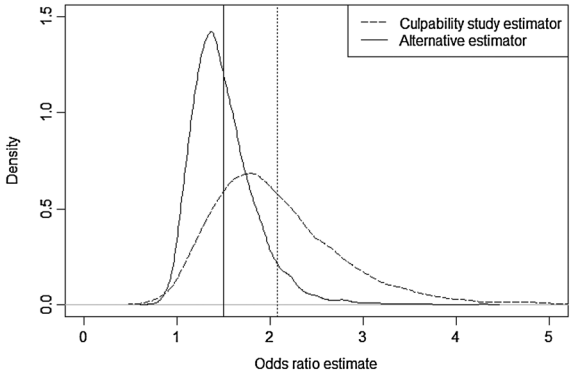


**Figure 1** Compared distributions of estimators from simulated culpability studies. Solid line shows underlying effect, dashed lines show mean value of estimators (line for Alternative estimator mean indistinguishable from solid due to overlap). Assumptions: driving under the influence of cannabis (DUIC) and non-DUIC drivers have the same risk of non-culpable crash, non-DUIC drivers have 50% culpability rate, DUIC doubles the risk of culpable crash. Each simulated study had 500 participants drawn from a multinomial distribution; figure is based on 100 000 simulations. Laplace correction applied to both estimators

than the 7.2 extracted by Asbridge. For Terhune [17], Asbridge *et al.* included the 'partly culpable' as culpable —while excluding the same group when extracting counts from Drummer [18]. Excluding them in both cases reduced the Terhune OR from 4.4 to 1.7.[2] The remaining two substantial adjustments were smaller, causing a roughly 20% reduction in the extracted OR [19,20]. In addition, the re-extracted counts caused the total counts extracted from studies to change substantially (> 10%) for three studies, with two total counts declining [12,17] and one increasing [20].

In Li *et al.*, substantial (> 10%) changes in ORs were found only for one study [7], where the correct OR was 1.4 rather than the 2.4 extracted by Li *et al.* No studies saw substantial changes in total counts after re-extraction of data.

With the revised counts, the studies used in Asbridge et al. involved a total of 50 877 (27 967 cases and 22 910 controls). The studies used in Li et al. involved a total of 93 229 counts (4 236 cases and 88 993 controls).

### Sparse data bias in ORs

The OR estimator has a known upward bias when cells have small counts [21–23]. A resampling analysis examined the impact of this for the two meta-reviews. From each study, the (corrected) counts were taken to characterize accurately the underlying case and control populations, and new case and control samples were drawn 10 000 times from this population, each with the same sample size as the original study. Taking the mean of all finite estimates, the results indicate that sparse data bias is an issue with a third of the studies used in Asbridge *et al.* and one in Li *et al.* (Fig. 2): samples of the size used in these studies would be expected to overestimate the underlying risks. Repeating the exercise with Laplace correction of the resampled OR estimates, i.e. adding 1 to each cell count and rescaling all cells proportionately to keep total sample size fixed, largely removed the bias.

### Known and observable confounders

Using cannabis and driving under the influence are behaviours that are more common among young adults and males, groups with higher crash risks irrespective of use. Typically, estimated ORs decline substantially after adjustments for such factors. Despite this, both meta-reviews used case and control counts from individual studies rather than adjusted estimates, although Asbridge *et al.* avoid confounding from alcohol by using 'no-alcohol' subsamples. The choice not to use adjusted risks raises risk estimates systematically and substantially, as can be seen by plotting the counts-based ORs against the adjusted estimates from the underlying studies where both are available (Fig. 3).

### Overall impact of methodological issues on pooled estimates

Correcting for the methodological issues noted revises the pooled estimates downwards substantially and systematically (Fig. 4).[3] All pooled analyses use the DerSimonian–Laird random-effects estimate, using the metafor R package [24]. Note that the originally reported pooled estimates of both meta-analyses are outside the 95% confidence interval (CI) bands of the revised pooled estimates. For Li *et al.*, however, the main shortcoming remains the lack of clear study selection criteria which gives the resulting pooled estimate no meaningful interpretation.

## STUDY 2: AN UPDATED META-ANALYSIS OF CANNABIS INTOXICATION AND TRAFFIC RISKS

### Sources

We aimed to include studies using case–control or culpability methods to assess the effects of acute cannabis intoxication on the risks of traffic crashes involving motor vehicles. Studies from before 2011 were identified by pooling the studies identified in Asbridge *et al.*, Li *et al.* and the cannabis-related studies included in a broader overview of studies on crashes and drugs [25]. Studies published since 2011 were identified using a structured search in Google

[2]Including the contributory cases would have reduced the OR from Drummer from 3.25 to 3.
[3]For the pooled estimates based on adjusted ORs sparse data bias could not be corrected for.

 

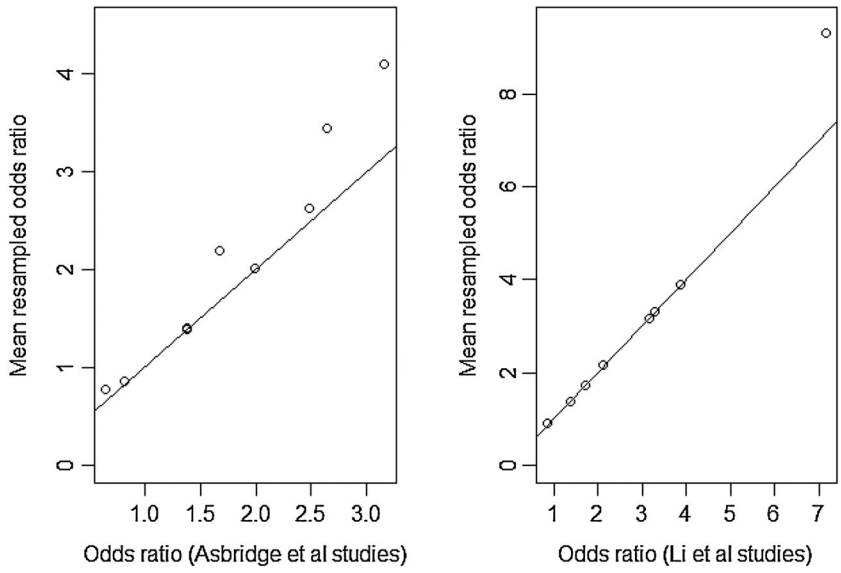


**Figure 2** Indications of sparse data bias in studies used by Asbridge *et al.* and Li *et al.* The plots compare the odds ratios calculated from (corrected) counts with means of 10 000 simulated studies of the same size, resampled from the same case and control counts

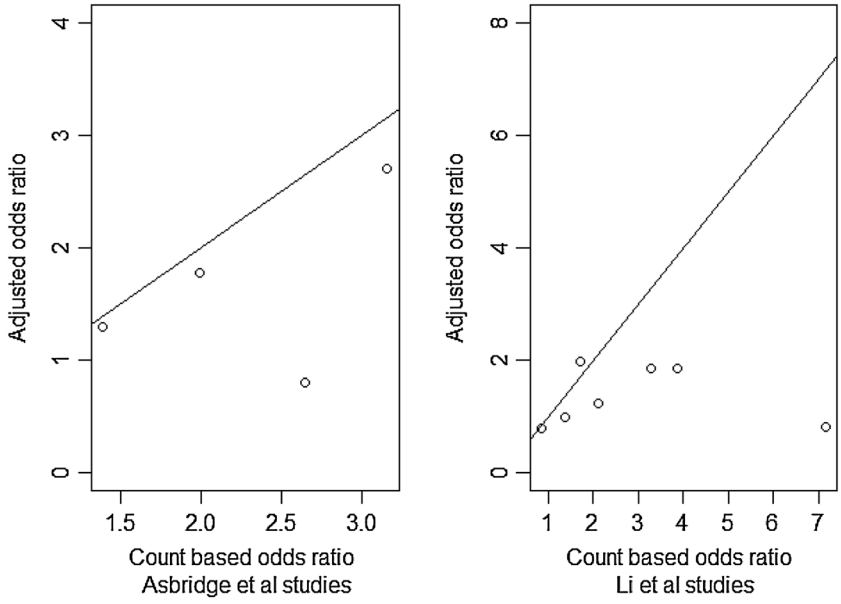


**Figure 3** Crude odds based on corrected counts versus adjusted odds reported in the original studies (for studies where confounder adjusted estimates were reported). Note: for Gerberich [8], used in Li *et al.*, adjusted estimates were reported for men [odds ratio (OR) = 1.96] and women (OR = 1.23) separately. The estimate for men was chosen as they were 72% of the current users in the sample

Scholar and Web of Science.[4] The database was supplemented by reviews of the authors' personal research libraries. In cases with substantial overlap in the data employed, studies reporting estimates after adjustment for relevant confounders and studies with larger sample sizes were preferred (see Supporting information).

### Data collection

One author (R.E.) extracted data from the recent and additional studies, adding these to a database containing extracted data from the studies used in the three earlier meta-reviews. Extracted information included

[4](cannabis OR marihuana OR marijuana OR hash OR THC OR cannabinoids OR hashish OR ganja OR hemp OR pot) AND (car OR automobile OR vehicle OR traffic OR road) AND (accident* OR crash* OR collision* OR collide OR injury OR fatal*). The first 500 search results, 5 April 2015, were assessed.

 

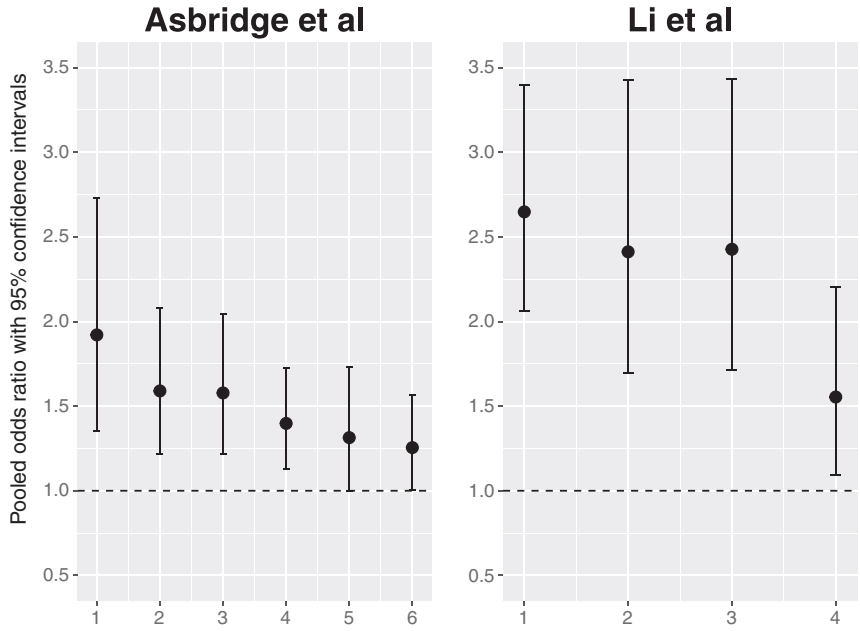


**Figure 4** Pooled estimates after adjustments. Left panel displays results for Asbridge *et al.* (1: reported counts, 2: corrected counts, 3: as 2 + Laplace correction, 4: as 3 + adjusted culpability estimator, 5: adjusted estimates from underlying studies, 6: as 5 + adjusted culpability estimator). Right panel displays results for Li *et al.* (1: reported counts, 2: corrected counts, 3: as 2 + Laplace correction, 4: adjusted estimates from underlying studies)

study details (author, year, country, study design), measure of cannabis exposure and multivariate associations between cannabis and outcomes. For studies without adjusted estimates, subsamples free of alcohol and other drugs were inferred when possible. The other author (O.R.) checked each result [(OR and standard errors (SE)] against the original studies, documenting the locations of the information as well as any inferences required (see Supporting information).

### Quality assessment for individual studies

Study quality was assessed in terms of four criteria [25]:

1 The quality of the information regarding the use of cannabis while driving;
2 Specification of crash severity;
3 Control for potential confounding factors; and
4 Test of the presence of a dose–response pattern in the relationship between the dose taken of cannabis and the increase in crash risk.

Table 1 explains how the various characteristics of study quality were defined and measured.

Laboratory analyses of blood samples for all subjects included in a study was rated as providing the best information on acute intoxication while driving. The second best indicator is saliva. Urine is a less informative indicator, as inactive metabolites of cannabis can be detected in samples of urine a long time after the substance became inactive. Prescription data, which would be more relevant for medicinal drugs than for cannabis, and self-reported use, were regarded as the least reliable data on the use of cannabis.

Several illicit drugs are known to increase the risk of serious crashes more than the risk of less serious crashes. To test whether there is such a severity gradient, a study should estimate crash risk for at least two levels of severity. Evidence regarding a severity gradient based on different studies is less conclusive, as different studies may differ in many ways that influence estimates of crash risk.

Nine potential confounding factors have been listed; in addition to these, a study may earn a bonus if it controls for more confounding factors.

Finally, testing for a dose–response pattern, and confirming its existence, is essential if one wants to support causal inferences, i.e. claims that cannabis intoxication is causally, not merely statistically, related to crash occurrence.

Points have been assigned to the various characteristics in Table 1. The use of formal quality scoring is controversial in meta-analysis [26,27]. One may, however, use the study characteristics listed in Table 1 as a screening device, without applying a formal quality scoring.

### Synthesis of results

Driving under the influence of cannabis is a behaviour that tends to be concentrated in subpopulations with raised risks of crashes irrespective of cannabis use. It is associated statistically with being a young adult, male and holding 'high-risk' attitudes towards driving and traffic as reflected in, e.g. higher rates of driving under the influence of alcohol [28–31]. For this reason, the adjusted estimates of risk and their associated standard errors were always employed when available.

 

**Table 1** Quantitative assessment of study quality (from [25]).

| *Study characteristic* | *Scores assigned* | *Maximum possible score* |
|---|---|---|
| Measure of drug use | 5 = laboratory analysis of blood samples for all subjects (cases and controls); 4 = laboratory analysis of samples of saliva or mix of blood and saliva; 3 = laboratory analysis of samples of urine or mix of urine and other body fluids; 2 = prescriptions; 1 = self-report | 5 (25% of total score) |
| Specification of crash severity | 2 = at least two levels of crash or injury severity included in the same study; 1 = crashes at a specific level of severity (fatal, injury, property damage) included; 0 = a mix of injury crashes and property damage crashes included | 2 (10% of total score) |
| Control for confounding factors | 9 = if all the following potentially confounding factors are controlled for: age, gender, km driven, drug use history, dose of drug, use of other drugs, use of alcohol, health status (comorbidity), place of residence<br>2 = additional points if multiple other potentially confounding factors are controlled for<br>1 = additional point if one other potentially confounding factor is controlled for | 11 (55% of total score) |
| Test of dose–response | 2 = tested and found; 1 = tested but not found; 0 = not tested or not relevant | 2 (10% of total score) |
| | Scoring of studies<br>Points counted and divided by maximum possible score (20 = 5 + 2 + 11 + 2). Expressed as relative score, e.g. 12/20 = 0.60 | |

Subgroup analyses were performed to assess differences associated with study design (culpability versus case control), study quality, crash severity, the measure of cannabis intoxication used and whether or not individual studies adjusted for simultaneous alcohol intoxication.

Meta-analyses were performed using two approaches: random-effects modelling using the DerSimonian–Laird estimator in the Metafor R package [24]. Given the presence of small sample (and possibly publication) bias, we also employed a weighted least-squares meta-regression technique [precision-effect estimate with standard errors (PEESE)] that has been shown to handle such biases better than random-effects models [32,33]. In addition, trim-and-fill methods were employed to test and correct for publication bias [34–36].

## RESULTS

### Study selection

The primary criterion for study inclusion was the quality of the information given about cannabis use, in particular if, based on the information given, there was reason to believe that cannabis had been used while driving or recently enough before driving for effects to persist.

The earlier reviews considered nine [5], nine [6] and 42 [25] estimates relating to cannabis and traffic crashes. New and additional studies found increased this to a total of 74 risk estimates from 46 studies, of which 28 estimates from 21 studies (Table 2) fitted our specified study selection criteria. The combined sample size for studies reporting confounder-adjusted estimates was 222 511, with an additional total of 17 228 counts from the remaining count-based studies.

### Characteristics of included studies

The studies included (Table 2) were published between 1982 and 2015. A case–control or culpability design was used in all studies. Nearly all studies estimated the risk of injury accidents or fatal accidents. All studies used the OR as estimator of risk. Use of cannabis was determined by laboratory analysis of body fluids in all studies except one. Studies differed greatly in terms of which potentially confounding variables they controlled for. A minority of studies tried to find a dose–response relationship between the amount of cannabis taken and the size of the change in crash risk.

### Risk of bias assessment

The quality of information regarding cannabis use is known to be an issue, in that poor quality (e.g. measuring inactive metabolites in urine) will produce a bias towards zero effect (OR = 1). This risk can be assessed using our quality scores: included studies scored an average of 4.11 on the five-point scale for quality of information about cannabis use. Studies not included scored an average of 2.16.

A recent analysis of cannabis-and-traffic studies from a cross-national project highlighted the issues of selection bias, small sample sizes and cell counts [38]. Small sample bias is likely to be an issue for several studies, given the typically low count of positive cases and controls, inflating estimates upwards. Selection bias (non-random sampling) in case–control studies are due to non-response rates, particularly from controls stopped in traffic-side stops. Non-response is likely to be more common for potential controls influenced by drugs or alcohol, inflating the OR [39]. In

 

**Table 2** Studies included in the meta-analysis.

| *Reference* | *Authors* | *Year* | *Country* | *Design* | *Accident severity* | *Estimator of risk (OR)* | *Measure of drug use* | *Confounders controlled (see Table 1)* | *Dose–response assessed* | *Dose–response found* |
|---|---|---|---|---|---|---|---|---|---|---|
| [17,37] | Terhune | 1983 | United States | Culpability | Mostly PDO | OR | Lab analysis | Alcohol, other drug use | No | No |
| [41] | Williams *et al.* | 1985 | United States | Culpability | Fatal | OR | Lab analysis | Alcohol, other drug use | No | No |
| [19] | Terhune *et al.* | 1992 | United States | Culpability | Fatal | OR | Lab analysis | Alcohol, other drug use | No | No |
| [42] | Longo *et al.* | 2000 | Australia | Culpability | Injury | OR | Lab analysis | Alcohol, other drug use | Yes | Yes |
| [43] | Lowenstein | 2001 | United States | Culpability | Injury | OR | Lab analysis | Alcohol, other drug use | No | No |
| [14] | Mura *et al.* | 2003 | France | Case–control | Injury | OR | Lab analysis | Age, gender | No | No |
| [11] | Brault *et al.* | 2004 | Canada | Case–control | Fatal | OR | Lab analysis | Age, gender, time of day | No | No |
| [18] | Drummer *et al.* | 2004 | Australia | Culpability | Fatal | OR | Lab analysis | Age, gender, other drug use, alcohol, type of accident, place of residence | Yes | Yes |
| [44] | Assum | 2005 | Norway | Case–control | Mostly fatal | OR | Lab analysis | Region | No | No |
| [12] | Blows *et al.* | 2005 | New Zealand | Case–control | Injury | OR | Self-report | Age, gender, ethnicity, education, alcohol, km driven, speed, time of day | No | No |
| [20] | Laumon *et al.* | 2005 | France | Culpability | Fatal | OR | Lab analysis | Age, alcohol, time of day | Yes | Yes |
| [45] | Mathijssen | 2005 | Netherlands | Case–control | Injury | OR | Lab analysis | Alcohol, other drug use | No | No |
| [15] | Woratanarat *et al.* | 2009 | Thailand | Case–control | Injury | OR | Lab analysis | None | No | No |
| [46] | Kuypers *et al.* | 2012 | Belgium | Case–control | Serious injury | OR | Lab analysis | Age gender, time of day | Yes | No |
| [47] | Hels *et al.* | 2011 | Denmark, Italy, Netherlands | Case–control | Serious injury | OR | Lab analysis | Age, gender | No | No |
| [47] | Hels *et al.* | 2011 | Lithuania | Case–control | Serious injury | OR | Lab analysis | None | No | No |
| [47] | Hels *et al.* | 2011 | Norway | Case–control | Fatality | OR | Lab analysis | Age, gender | No | No |
| [47] | Hels *et al.* | 2011 | Portugal | Case control | Fatality | OR | Lab analysis | None | No | No |
| [48] | Gjerde *et al.* | 2013 | Norway | Case–control | Fatal | OR | Lab analysis | Age, gender, region, season, time-period, road type | No | No |
| [49] | Li *et al.* | 2013 | United States | Case–control | Fatal | OR | Lab analysis | None | No | No |
| [50] | Poulsen *et al.* | 2014 | New Zealand | Culpability | Fatal | OR | Lab analysis | Age, gender, alcohol, other drug use, licence | Yes | No |

(*Continues*)

**Table 2.** (Continued)

| *Reference* | *Authors* | *Year* | *Country* | *Design* | *Accident severity* | *Estimator of risk (OR)* | *Measure of drug use* | *Confounders controlled (see Table 1)* | *Dose–response assessed* | *Dose–response found* |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | status, vehicle type, road class, crash type | | |
| [51] | Romano *et al.* | 2014 | United States | Case–control | Fatal | OR | Lab analysis | Alcohol, other drug use | No | No |
| [52] | Compton *et al.* | 2015 | United States | Case–control | Mostly PDO | OR | Lab analysis | Age, gender, ethnicity, alcohol | No | No |
| [53] | Dubois *et al.* | 2015 | United States | Culpability | Fatal | OR | Lab analysis | Age, gender, alcohol, other drug use, driving history | No | No |

PDO = property damage only.

culpability studies using administrative data registers, toxicological testing may be conducted more often when the driver is seen as culpable and/or suspected of intoxication. This can result in both an upwards or downwards bias of estimates depending on the details. A related issue present in some studies (e.g. Mura [14], as pointed out in Baldock [40]) is the use of control groups not drawn from non-crash-involved drivers, raising issues of comparability.

### Primary and secondary analyses

The primary analysis is shown in Fig. 5. For culpability studies, point estimates were adjusted using their baseline culpability rates; for studies based on crude counts, Laplace correction was applied.

A subsample analysis (Table 3) was conducted by splitting the studies according to type (case–control versus culpability), study quality (low versus medium versus high), control for confounders (limited versus high), use data quality (low versus medium versus high), control for alcohol (no versus yes) and crash severity (fatalities involved versus not). Pooled risks and confidence intervals were calculated within each subsample using both random effects and a PEESE meta-regression.

### Publication bias

Overall, the PEESE meta-regression technique finds no indication of publication bias in the 28 estimates ($P = 0.52$, see Table 3), indicating a best estimate for the underlying effect of 1.23. The trim-and-fill method indicated a weak publication bias, with a trimmed mean summary estimate of risk of 1.23, in effect similar to that from the meta-regression model.

## DISCUSSION

The replication of Asbridge *et al.* [5] and Li *et al.* [6] in study 1 indicates that their published pooled estimates substantially overestimated the effect of acute cannabis intoxication on crash risk, and that the pooled estimate presented by Li *et al.* is hard to interpret, given the qualitatively different estimates pooled. The revised estimate from the studies used in Asbridge *et al.* was in line with the results from the expanded meta-analysis in study 2, lying between the pooled odds from the mixed-effects model of 1.36 (CI = 1.15–1.61) and the pooled odds from the PEESE meta-regression of 1.22 (meta-regression model, CI = 1.1–1.36).

The effect sizes found represent an average risk increase for those driving after the use of cannabis. Under a causal interpretation, this suggests that roughly 20–30% of traffic crashes involving cannabis use occur because of the cannabis use. By comparison, the comparable 'average'

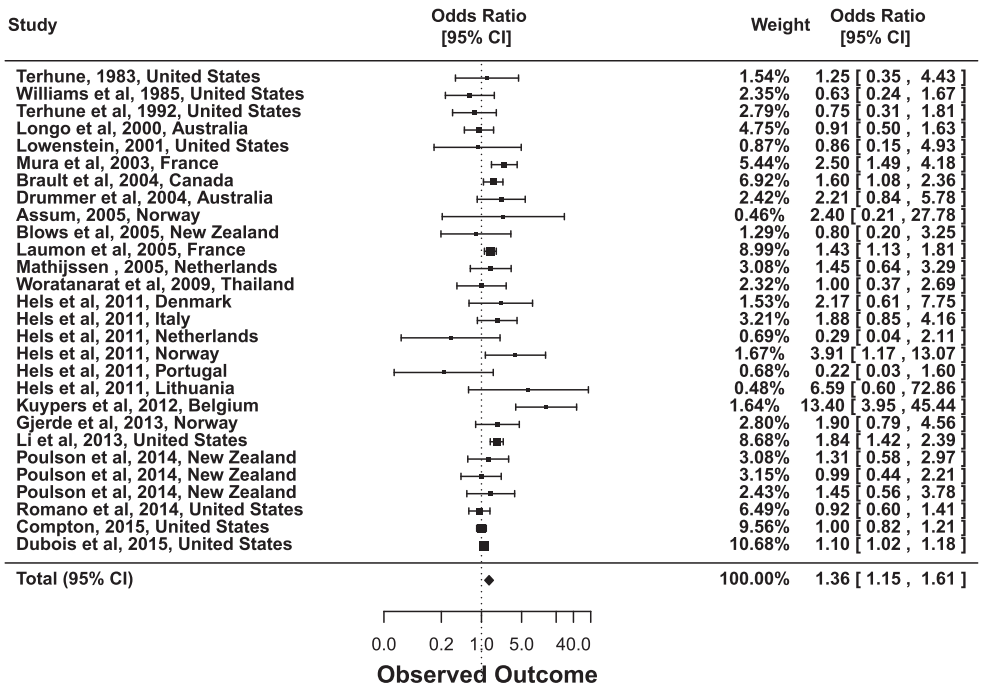


**Figure 5** Meta-analysis of observational studies investigating the association between acute cannabis consumption and motor vehicle crashes (ordered by year)

**Table 3** Subgroup analyses.

| | | *Mixed effects model* | | *Meta-regression model (PEESE)* | | |
|---|---|---|---|---|---|---|
| *Group* | *#* | *OR* | *CI* | *OR* | *CI* | *Publication bias (p-value)* |
| All | 28 | 1.36 | (1.15, 1.61) | 1.22 | (1.1, 1.36) | 0.52 |
| Case control | 17 | 1.60 | (1.19, 2.15) | 1.32 | (1.04, 1.68) | 0.55 |
| Culpability | 11 | 1.12 | (1.05, 1.2) | 1.20 | (1.08, 1.35) | 0.78 |
| High quality | 9 | 1.39 | (1.06, 1.83) | 1.18 | (1.05, 1.34) | 0.10 |
| Medium quality | 12 | 1.30 | (0.95, 1.78) | 1.15 | (0.87, 1.51) | 0.90 |
| Low quality | 7 | 1.45 | (0.97, 2.17) | 1.79 | (1.34, 2.39) | 0.36 |
| Limited or no confounder adjustment | 17 | 1.52 | (1.07, 2.15) | 1.70 | (1.19, 2.41) | 0.62 |
| High confounder adjustment | 11 | 1.17 | (1.04, 1.33) | 1.18 | (1.06, 1.31) | 0.49 |
| Low quality use data | 7 | 1.07 | (0.92, 1.25) | 1.08 | (0.9, 1.3) | 0.83 |
| Medium quality use data | 9 | 1.81 | (1.23, 2.66) | 1.90 | (1.41, 2.55) | 0.61 |
| High quality use data | 12 | 1.37 | (1.05, 1.79) | 1.20 | (1.04, 1.39) | 0.37 |
| Alcohol controlled | 14 | 1.11 | (1.04, 1.18) | 1.18 | (1.07, 1.3) | 0.75 |
| Alcohol not controlled | 14 | 1.79 | (1.28, 2.51) | 1.69 | (1.25, 2.28) | 0.90 |
| Fatalities involved | 15 | 1.32 | (1.08, 1.61) | 1.24 | (1.1, 1.4) | 0.92 |
| Fatalities not involved | 13 | 1.51 | (1.02, 2.24) | 1.11 | (0.82, 1.49) | 0.37 |

OR = odds ratio; CI = confidence interval; PEESE = precision-effect estimate with standard error.

relative risk for accidents with fatalities after drinking alcohol has been estimated at 7.5 [54], which would imply that approximately 85% of crashes involving alcohol occur because of alcohol. Assuming causality, the differences can be due to differences in the impairment produced at various consumption levels, and/or differences in the average consumption levels of those choosing to drive after cannabis and alcohol use, respectively.

The average effect of 1.2–1.4 found for cannabis is comparable to the increased risk for any traffic crash found for a blood alcohol content (BAC) of 0.04–0.05 [55]. As alcohol seems to be linked more reliably to an increase in risky driving, however, the risk of crashes resulting in fatalities from a 0.02–0.05 BAC is estimated to rise by 100–360%, depending on age and crash type [56]. No comparable increase in the risk

 

of crashes involving fatalities is found in the subsample analysis for cannabis studies (Table 3). This is consistent with results from the experimental literature, which reports that alcohol increases driving speed and risk-taking while some cannabis users attempt to compensate for their impairment by driving more cautiously.

While there is heterogeneity across studies, the subsample analyses all show pooled effects in the range of 1.07–1.81 (random effects) and 1.08–1.9 (meta-regression), suggesting that the average risk increase after cannabis use is unlikely to be of the magnitude associated with alcohol. The importance of confounding is particularly evident in the subsample analysis for alcohol confounding, where both methods find an OR below 1.2 when alcohol is controlled for and higher estimates (1.79 and 1.69) when not. Higher estimates are associated with case–control studies, low study quality, limited control of confounders, medium quality use data and not controlling for alcohol intoxication.

### Alternative interpretations of the results

The causal interpretation of the above results would be that cannabis intoxication has a moderate effect on traffic risks. However, it is important to note that remaining selection effects may bias the estimates in either direction.

Because cannabis users tend to be aware of their impairment [4], this may cause selection on effect: if users are more likely to drive when they judge their impairment to be low, then the estimates above will underestimate the (unobserved) crash risk of the currently non-driving users. This matters: if the low estimates are taken as evidence that 'driving after cannabis use' is unproblematic, the new users would tend to be more impaired and have a higher cannabis-induced increase in crash risk.

An opposite bias can result from residual confounding due to selection into cannabis use and selection of users into 'driving after use'. In particular, deciding to drive while intoxicated is a decision correlated with traits that predict higher crash risk independently of cannabis use: high speeds, close following, dangerous lane shifts and drunk driving [7,29–31]. This would give estimates an upward bias, in that 'driving after cannabis use' functions as an indicator of an underlying high-risk type of driver.

While both types of bias are possible, we note that they predict different patterns across empirical studies. When the share of users who decide to drive after use increases, selection on effect implies that OR estimates increase as the new drivers will have higher risks. Selection into 'driving after use' would imply that OR estimates decline, as the new drivers would have lower underlying risk traits.

### Policy implications

The growing interest in the crash risk associated with cannabis use is related to the ongoing debate about cannabis policy. Concerns have been raised that liberalized laws would increase cannabis use, increasing the number of cannabis-intoxicated drivers and raising the traffic crash rate. While our estimates suggest that the impact on crash rates would be low to moderate, even if this argument were correct we would stress that such simple extrapolations are unlikely to be robust to larger policy changes: driving under the influence of legal cannabis would probably be made a direct target for policy, leading to efforts with documented effects from the alcohol field [57–59]. Cannabis use may also influence traffic risks through other causal channels: an ecological study using the staggered introduction of medical marijuana laws across US states found a net reduction in traffic crashes associated with the introduction of these laws [60]. The authors suggest that this could be due to consumers shifting from alcohol (with high crash risk) to cannabis (with lower crash risk), or due to cannabis users driving less than they would have after drinking (e.g. smoking at home rather than driving to a bar). This underscores the larger policy point that a low-to-moderate causal effect of acute cannabis intoxication on crash rates is likely to play a limited role in the overall policy picture surrounding cannabis legislation.

## CONCLUSIONS

A comprehensive review of the literature on acute cannabis intoxication and road traffic crashes finds that acute intoxication is related to a statistically significant risk increase of low to moderate magnitude. Higher estimates from earlier meta-reviews were found to be driven largely by methodological issues—in particular, the use of counts data without adjustment for known confounders. Correcting for these issues, the pooled estimates from these reviews were in line with the results from the updated and more extensive review. Remaining selection effects discussed in the 'Alternative interpretations' section may complicate causal interpretations of the pooled estimates.

### Declaration of interests

O.R. reports grants from Norwegian Research Council, during the conduct of the study. The funding source had no role in the study design; in the collection, analysis and interpretation of the data; in the writing of the report; or in the decision to submit this article for publication. All researchers worked independently of the funding agency. R.E. has nothing to disclose. Extracted data from research articles required to run the meta-analyses are available on request from the corresponding author.

 

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## Supporting information

Additional supporting information may be found in the online version of this article at the publisher's web-site:

**Appendix S1** Study selection and data extraction

 