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How Tessmer 2004 uses a method that is inherently biased in favour of motorcar daytime running lights, yet still only succeeds in making mixed findings that they reduce accidents

A critical review of Tessmer J M 2004 'An Assessment of the Crash-Reducing Effectiveness of Passenger Vehicle Daytime Running Lamps (DRLs)'

British Motorcyclists Federation

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A. Summary

In spite of the conduct since 1964 of perhaps forty observational, or 'monitoring', studies of the effect of motorcar or motorcycle daytime running lights, road safety research scientists have still failed to devise a satisfactory study-design or method of detecting and measuring the effect of daytime running lights upon accidents.

Either in consequence of this failure, or because daytime running lights may potentially increase as well as reduce accidents, the main studies out of the forty studies have variously made findings that daytime running lights have reduced accidents; have had no effect upon accidents; or have in fact increased accidents.

Against this background Tessmer 2004:

- Uses a study-design—the 'side-by-side' comparison of the accident experience of two fleets of motor vehicles, one of which uses daytime running lights, and the other of which does not—that is inherently biased towards finding a reduction of accidents from daytime running lights
- Uses two methods of measuring the effect of daytime running lights—the 'Simple odds' method, and the Odds-ratio method—neither of which is specific to the effect of daytime running lights; rather each of them responds also to the effect upon accidents of a number of prevalent other causes, or 'other factors'
- Makes mixed findings by the Simple odds method, and the Odds-ratio method — namely a finding by the Simple odds method that daytime running lights reduce accidents; but a finding by the Odds-ratio method that daytime running lights increase accidents.

Despite the mixed findings, Tessmer 2004 seeks to draw a definite conclusion in favour of daytime running lights from his study.

First, Tessmer passes over without discussion or mention the inherent bias of his study-design in favour of daytime running lights.

Second, Tessmer mentions, but then passes over without description, discussion or comparison, the other factors besides daytime running lights that the 'tests' of the Simple odds, and the Odds-ratio, method respectively respond to.

And third, Tessmer passes over without discussion or mention the fact that:

- By virtue of its formulation, the 'test' of the Odds-ratio method affords a more reliable measure of the effect of daytime running lights than the test of the Simple odds method.

Instead he prefers the finding from the test of the Simple odds method to the finding from the test of the Odds-ratio method because:

- By virtue of its formulation, the Simple odds test is more sensitive to the effect of daytime running lights than the Odds-ratio test
- The standard error of the Simple odds test is much smaller than the standard error of the Odds-ratio test
- Only the finding from the Simple odds method is statistically significant.

Tessmer thereby avoids that, in scientific terms, it is not sufficient for him to detect a reduction of accidents from daytime running lights by the 'more sensitive' Simple odds method; he must also go on to confirm the reduction of accidents in question by the 'more reliable' Odds-ratio method.

For the layman, Tessmer 2004's mixed findings from the Simple odds and Odds-ratio methods render the findings of his study in favour of daytime running lights inconclusive.

For the scientist, the inherent bias of Tessmer 2004's method; the incompleteness of his presentation; and the unsatisfactory nature of his argumentation, render the conclusion of his study in favour of daytime running lights likewise invalid and worthless.

B. Expansion of summary

1. 'In spite of the conduct since 1964 of perhaps forty observational, or 'monitoring', studies of the effect of motorcar or motorcycle daytime running lights, road safety research scientists have still failed to devise a satisfactory study-design or method of detecting and measuring the effect of daytime running lights upon accidents'

Essentially the daytime running light prediction is that daytime running lights will reduce daytime multi-vehicle accidents, but not other accidents.

In order to verify the prediction, the forty monitoring studies have employed, whether solely or in combination, the following three study-designs:

- 'Before-and-after' comparison study — or more loosely 'Monitoring study'[1]
  (Will typically compare the incidence of accidents before and after the introduction of a mandatory daytime running light use or fitting law)
- 'Side-by-side' comparison study — or more loosely 'Fleet study'[2]
  (Will typically compare the incidence of accidents that are contemporaneously experienced by two fleets of vehicles, one of which uses daytime running lights, and the other of which does not use daytime running lights)
- 'Accident & Control group' comparison study
  (Will typically compare the proportion of vehicles that are using daytime running lights in a group of drivers who have experienced accidents, and a matched group of drivers who have not done so).

The studies have employed, whether solely, or severally together, the following three tests to detect and measure the predicted effect of daytime running lights to reduce accidents:

- Changes in the undifferentiated ratio of daytime: nighttime accidents (da:na), or multi:single-vehicle accidents (mva:sva), ie:

$$\frac{da}{na} \quad \text{or} \quad \frac{mva}{sva}$$

- Changes in the incidence of daytime multi-vehicle accidents (dmva), as measured by the 'Simple odds' method, namely changes in the value of the fraction:

$$\frac{dmva}{dsva + nmva + nsva}$$

- Changes in the incidence of daytime multi-vehicle accidents, as measured by the 'Odds-ratio' method, namely changes in the value of the fraction:

$$\frac{dmva / dsva}{nmva / nsva} .$$

The three tests

To treat the three tests, while the tests do become progressively more sophisticated, and specific to, the predicted effect of daytime running lights selectively to reduce daytime multi-vehicle accidents, they nevertheless continue to respond also to changes in other factors besides the incidence of vehicles using daytime running lights:

- The 'Undifferentiated ratio' test—to take the form of the test that records changes in the ratio of daytime:nighttime accidents—only constitutes a test for the daytime 'half' (da); not also the multi-vehicle half (mva), of the prediction that daytime running lights will reduce daytime multi-vehicle accidents
- The 'Simple odds' test does constitute a test for the daytime running light prediction in full.
  But, by its formulation, the Simple odds test responds also to changes in any other factor besides daytime running lights that influences either, more 'narrowly':
  - The incidence of daytime single-vehicle accidents (dsva), nighttime multi-vehicle accidents (nmva) or nighttime single-vehicle accidents (nsva),

  or more 'broadly':
  - The incidence of daytime, nighttime, multi-vehicle accidents or single-vehicle accidents — ie the ratio of daytime to nighttime accidents (da:na), or multi-vehicle to single-vehicle accidents (mva:sva)
- The 'Odds-ratio' test, by a different formulation, does eliminate the 'broad' response of the 'Simple odds' test also to changes in any factor besides daytime running lights that influences:
  - The ratio of daytime to nighttime accidents (da:na), or multi-vehicle to single-vehicle accidents (mva:sva).

  But the Odds-ratio test retains the 'narrow' response to changes in any factor besides daytime running lights that influences:
  - The incidence of daytime single-vehicle accidents (dsva), nighttime multi-vehicle accidents (nmva) or nighttime single-vehicle accidents (nsva).

Thus to give an example of one of the residual sensitivities to other factors of the Odds-ratio test, since traffic density is very low in the late hours of the evening, changes in any other factor that influences the volume of late evening leisure driving, such as the currently prevailing:

- Climate
- Age distribution of the driving population
- Pattern of working hours of the general population
- Level of disposable income of the general population
- Pattern of leisure activity of the general population
- Level of enforcement of the laws against drinking and driving,

[1] The effect of a mandatory daytime running light fitting law can also be monitored by a 'Side-by-side' comparison study

[2] One may compare the accident experience of the same fleet before and after it uses daytime running lights by a 'Before-and-after' comparison study, as well as compare the accident experience of two fleets, one of which uses daytime running lights, and the other of which does not use daytime running lights, by a 'Side-by-side' comparison study

will produce a marked change in the incidence of nighttime single-vehicle accidents (nsva), and so a correspondingly marked response from the Odds-ratio test.

And it will be observed that most countries are subject to a marked year-to-year variation in Climate; and important long-term trends, including rising or falling trends, in both Climate and the other five listed factors.

The 'Accident & Control group' study-design

To treat the three study-designs, the 'Accident & Control group' comparison study-design suffers from the practical impossibility of achieving or verifying a close match of:

- The relevant 'personal' characteristics of the drivers (eg attitude)
- The relevant 'driving' characteristics of the drivers (eg age and experience)
- The relevant breakdown by type of the vehicles
- The routes, times and conditions of travel of the drivers

as between the Accident group, and the Control group.

The same impossibility applies where, as an adjunct to a 'Before-and-after' comparison study, the drivers of a country, state or province that has not enacted a law mandating the use or fitting of daytime running lights are used as the Control group for a 'Target' or 'Experimental' group of the drivers of a country, state or province that has enacted such a law.

The 'Side-by-side', or 'Fleet study', study-design

The 'Side-by-side' comparison, or 'Fleet study', study-design also suffers from the difficulty of matching the two comparison groups, but the problem is not intractable, since a study can be designed in particular so that the two groups 'share the same road at the same time'.

Instead the problem of the Fleet study method is the inherent bias of the method in favour daytime running lights.

The inherent bias arises because, correctly, a Fleet study should compare:

- The accident experience of the fleet of vehicles that does not use daytime running lights against a background of 0% of vehicles generally that use daytime running lights
- The accident experience of the fleet of vehicles that does use daytime running lights against a background of 100% of vehicles generally that use daytime running lights.

However by definition the comparison of a Fleet study is conducted contemporaneously.

Otherwise the study would not 'control' for changes in the other factors besides daytime running lights that influence the three tests that are used to detect and measure the effect of daytime running lights.

So a Fleet study will compare, say:

- The accident experience of the fleet of vehicles that does not use daytime running lights against a background of 60% of vehicles generally that use daytime running lights
- The accident experience of the fleet of vehicles that does use daytime running lights against a background of 60% of vehicles generally that use daytime running lights.

Correspondingly the fleet of vehicles that does use daytime running lights will be advantaged by the 'novelty', or 'contrast', effect of being viewed against a background of other vehicles in which 40% of vehicles do not use daytime running lights — so creating a positive bias in favour of daytime running lights.

And the fleet of vehicles that does not use daytime running lights will be disadvantaged by the 'distracting', or 'obscuring', effect of being viewed against a background of other vehicles in which 60% of vehicles do use daytime running lights — so creating a second positive bias also in favour of daytime running lights.

And whilst a positive and a negative bias might cancel each other out, two positive biases must rather maintain, or indeed reinforce, each other.

The 'Before-and-after', or 'Monitoring study', study-design

The 'Before-and-after', or 'Monitoring study', study-design suffers the least out of the three study-designs from the problem of matching the characteristics of the drivers and vehicles of the two comparison groups.

But because Monitoring studies compare the incidence of accidents consecutively, rather than contemporaneously, they depend utterly upon the availability of a test for the effect of daytime running lights upon accidents in the 'Before' and 'After' periods that is not sensitive also the effect upon accidents of changes in other factors besides daytime running lights.

And as has been described under *'The three tests'* above, such a test does not exist.

2. 'Either in consequence of this failure, or because daytime running lights may potentially increase as well as reduce accidents, the main studies out of the forty studies have variously made findings that daytime running lights have reduced accidents; have had no effect upon accidents; or have in fact increased accidents.'

The main studies out of the perhaps forty observational, or 'monitoring', studies to date of the effect of motorcar or motorcycle daytime running lights are the monitoring studies of actual daytime running light laws.

Tessmer 2004 recites *(p3)*:

> 'Seven countries require the use of DRLs during all daytime periods: Canada, Denmark, Finland, Hungary, Iceland, Norway, and Sweden.'

and then states *(ibid)*:

> 'Results of DRL studies from these countries consistently, however not conclusively, show that DRLs reduce the number of two-vehicle crashes during daylight, dusk, and dawn.'

In fact, (1) Theeuwes & Riemersma 1995 reviewed Andersson & Nilsson 1981's study of the effect of requirement of the use of daytime running lights in Sweden, and concluded:

> 'The present analysis of the Swedish study regarding the nationwide DRL implementation shows that there is no unequivocal evidence for an effect of DRL on accident rates. The present results are in line with a recent study conducted by Elvik (1993). This study also shows that in Norway the use of DRL did not result in a reduction of multiple daytime accidents. As argued by Elvik (1993), it is realized that in any non-experimental research design confounding factors may have distorted the analysis.
>
> The present analysis indicates that the estimated reduction of 11% of multiple vehicle accidents during daytime is mainly based on a questionable estimate of this number under the assumption of no effect of DRL. Other indicators do not substantiate the claim of effectiveness of DRL.'

Theeuwes & Riemersma then went on to list the results of the 'other indicators'.

And (2), Perlot & Prower 2003 reviewed the studies of the effect of requirement of the use of daytime running lights in six out of the seven countries that Tessmer lists, excluding Iceland, and reported the following individual findings that daytime running lights did not reduce accidents.

First as to Finland[3]:

> '[Finland *(Andersson et al 1976)*]
>
> In their English language summary of findings, Andersson et al reported, without qualification, that the value of the odds-ratio for multi-party accidents in Finland fell from 1.88 to 1.76 following the enactment of the Finnish law in 1972; in conclusion the fall and their other findings strongly indicated that the use of daytime lights had done a great deal to reduce accidents.
>
> However the Swedish language main text reveals:
>
> 1) That Andersson et al's category of 'other' accidents comprised for a large part animal accidents:
>
> 'Övriga flerpartsolyckor innehåller till en stor del djurolyckor ...',
>
> and it was essentially only the odds-ratio value for other accidents that fell (1.35 to 0.79); the values for multi-vehicle and pedestrian accidents showed either a negligible fall (2.27 to 2.25) or rise (0.90 to 0.91)
>
> ...' [p3]

And second as to Sweden, Norway, Denmark, Canada and Hungary[4]:

> 'Sweden *(Andersson & Nilsson 1981)*
>
> - Fall of multi-party accidents after law — But fall not statistically significant
> - Fall of multi-party accidents only in first year after law; Recovery of multi-party accidents in second year after law to higher figure than in last year before law
>
> Norway *(Vaaje 1986)*
>
> - Fall of casualties from multi-party accidents and pedestrian accidents after 1985 law — But, per *Koornstra et al 1997*, Vaaje considered amount of falls implausibly high when set against estimated increase in use of daytime lights
>
> Norway *(Elvik 1993)*
>
> - No fall of multi-party accidents after 1985 and 1988 laws
>
> Denmark *(Hansen 1993 & 1994)*
>
> - Per *Koornstra et al 1997*, fall of multi-vehicle accidents after law — But statistically insignificant rise of pedestrian accidents
>
> Canada *(Arora et al 1994)*
>
> - Fall of multi-vehicle accidents for one-year-old motorcars built in first year after law — But unexplained lower fall for brand-new motorcars built in second year after law
>
> Hungary *(Holló 1995 & 1998)*
>
> - Confusing background of other road safety measures, and unexplained trends in the data caused by other factors
> - Pedestrian accidents excluded from the analyses
> - Small sample of accident data, and so failure to achieve statistical significance
> - Initial findings made for before and after periods of just a year, so à la motorcycle study *Janoff et al 1970* post, Holló failed to establish normal year-to-year variation in the values of his study measures; When period extended, mixed findings from disaggregated year-to-year data.' [pp5–6]

3. 'Against this background Tessmer 2004:
- **Uses a study-design—the 'side-by-side' comparison of the accident experience of two fleets of motor vehicles, one of which uses daytime running lights, and the other of which does not—that is inherently biased towards finding a reduction of accidents from daytime running lights'**

See *Section 1, 'The 'Side-by-side', or 'Fleet study', study-design'*, of the present paper, above.

4. 'Against this background Tessmer 2004:
- **Uses two methods of measuring the effect of daytime running lights—the 'Simple odds' method, and the Odds-ratio method—neither of which is specific to the effect of daytime running lights; rather each of them responds also to the effect upon accidents of a number of prevalent other causes, or 'other factors''**

See *Section 1, 'The three tests'*, above.

As to the 'Simple odds' method, to give examples, increasing traffic density; speed restriction; or changes in the property damage report threshold, will evoke the general sensitivity of the method to changes in the ratio of multi-vehicle to single-vehicle accidents (mva : sva).

Likewise changing working and leisure patterns, or changes in daylight saving time, will evoke the general sensitivity of

[3] Perlot & Prower 2003 refer in the context simply to 'multi-vehicle' accidents, 'pedestrian' accidents, etc; not 'daytime multi-vehicle' accidents, 'daytime pedestrian' accidents, etc
[4] Ditto

the method to changes in the ratio of daytime to nighttime accidents (da : na).

As to the 'Odds-ratio' method (or equally also the 'Simple odds' method), to repeat the example that was given in *Section 1, 'The three tests'*, since traffic density is very low in the late hours of the evening, changes in any other factor that influences the volume of late evening leisure driving, such as the currently prevailing:

- Climate
- Age distribution of the driving population
- Pattern of working hours of the general population
- Level of disposable income of the general population
- Pattern of leisure activity of the general population
- Level of enforcement of the laws against drinking and driving,

will evoke the general sensitivity of both methods to changes in the incidence of daytime single-vehicle accidents (dsva), nighttime multi-vehicle accidents (nmva) or nighttime single-vehicle accidents (nsva) — in this instance the general sensitivity to changes in the incidence of nighttime single-vehicle accidents (nsva).

5. 'Against this background Tessmer 2004:

- Makes mixed findings by the Simple odds method, and the Odds-ratio method — namely a finding by the Simple odds method that daytime running lights reduce accidents; but a finding by the Odds-ratio method that daytime running lights increase accidents.'

Result of the 'Simple odds' test

Calculation from the data supplied in Tessmer 2004: Tables 2–5 *(pp12–14)* yields the following values of the 'Simple odds' test:

Table 2: DRL-Equipped Vehicles in Target and Single-Vehicle Fatal Crashes, FARS 1995–2001

Simple odds = 0.236

Table 3: Vehicles w/o DRL in Target and Single-Vehicle Fatal Crashes, FARS 1995–2001

Simple odds = 0.249

Table 4: DRL-Equipped Vehicles in Target and Single-Vehicle Non-Fatal Crashes, NASS/GES 1995–2001

Simple odds = 1.43

Table 5: Vehicles w/o DRL in Target and Single-Vehicle Non-Fatal Crashes, NASS/GES 1995–2001

Simple odds = 1.51 .

As will be observed, in accordance with the daytime running light prediction, for both Fatal and Non-fatal crashes the value of the Simple odds test was higher for Vehicles without daytime running lights.

Ie the incidence of crashes was higher for Vehicles without daytime running lights.

Result of the 'Odds-ratio' test

By contrast with the above result for the 'Simple odds' test, however, calculation from the same data—ie from the data supplied in Tessmer 2004: Tables 2–5 *(pp12–14)*—yields the following values of the 'Simple odds' test:

Table 2: DRL-Equipped Vehicles in Target and Single-Vehicle Fatal Crashes, FARS 1995–2001

Odds-ratio = 2.75

Table 3: Vehicles w/o DRL in Target and Single-Vehicle Fatal Crashes, FARS 1995–2001

Odds-ratio = 2.59

Table 4: DRL-Equipped Vehicles in Target and Single-Vehicle Non-Fatal Crashes, NASS/GES 1995–2001

Odds-ratio = 3.94

Table 5: Vehicles w/o DRL in Target and Single-Vehicle Non-Fatal Crashes, NASS/GES 1995–2001

Odds-ratio = 3.65 .

As will be observed, in contradiction of the daytime running light prediction, for both Fatal and Non-fatal crashes the value of the Odds-ratio test was lower for Vehicles without daytime running lights.

Ie the incidence of crashes was lower for Vehicles without daytime running lights.

6. 'Despite the mixed findings, Tessmer 2004 seeks to draw a definite conclusion in favour of daytime running lights from his study.'

In the Conclusions section of his paper, Tessmer 2004 passes over entirely, without recital or mention, the above adverse finding against daytime running lights from the 'Odds-ratio' test.

Instead he only recites the finding in favour of daytime running lights from the 'Simple odds' test *(p17)*:

> '**Conclusions**
>
> The effectiveness of daytime running lamps, based on the simple odds, was analyzed in the preceding sections using data from FARS and NASS/GES from calendar years 1995 to 2001. FARS and NASS/GES data show that during the period of the study 1995 to 2001, DRLs reduced daylight two passenger vehicle opposite-direction crashes by about 5 percent. DRLs have also been shown to reduce fatal opposite direction crashes between a motorcycle and a passenger vehicle by 23 percent. The results for two-vehicle daytime opposite-direction crashes are statistically significant at the $p < 0.10$ level, although one would prefer a statistical level of $p < 0.05$.'

The omission of the adverse finding against daytime running lights from the 'Odds-ratio' test from Tessmer 2004's Conclusions section is especially wrong because it enables laymen to assert misleadingly before the world, with the support of Tessmer's own words, that 'Tessmer made unqualified findings that daytime running lights reduced accidents'.

7. 'First, Tessmer passes over without discussion or mention the inherent bias of his study-design in favour of daytime running lights.'

The inherent bias of Tessmer 2004's Side-by-side comparison study-design in favour of daytime running lights has already been described under *Section 2, 'The 'Side-by-side', or 'Fleet study', study-design'*, above.

Yet Tessmer 2004 neither discusses, nor attempts to justify, the study-design anywhere in the paper.

Rather he takes the validity of the study-design for granted.

8. 'Second, Tessmer mentions, but then passes over without description, discussion and comparison, the other factors besides daytime running lights that the 'tests' of the Simple odds, and the Odds-ratio, method respond to.'

The other factors besides daytime running lights that the 'tests' of the Simple odds, and the Odds-ratio, method, respond to have already been described under *Section 2, 'The three tests'*; or the treatment of the lack of specificity of the tests in *Section 4*, above.

Tessmer 2004 does mention the other factors, in general terms, in the following passages from his paper *(Present author's emphases)*:

> 'The generalized odds ratio attempts to adjust for **a variety of exogenous factors** other than the presence or absence of DRLs not specifically controlled for within the model.' *(p1)*

> 'Like the simple odds, the odds ratio attempts to control for **a variety of factors** other than the presence or absence of DRLs. The estimated effectiveness of DRLs based on this technique is extremely sensitive to **small changes encountered in real world crash data**. As a result, reductions in target crashes during the daytime using the odds ratio technique may not be detected over the **inherent background noise of the data system**.' *(p1)*

> 'The generalized simple odds method was used to analyze the data. This technique implicitly attempts to control for **factors**, other than the presence or absence of DRLs, **that could be associated with crash occurrences**.' *(p2)*

> 'The odds ratio is easier to understand for inexperienced analysts than the simple odds and, like the simple odds, attempts to control for **a variety of factors** other than the presence or absence of DRLs. Unfortunately, when using the odds ratio, the estimated effectiveness of DRLs is extremely sensitive to **small changes encountered in real world crash data** and none of the results were statistically significant. This does not mean that DRLs do not reduce target crashes during the daytime. It just means that the odds ratio technique does not detect these changes over the **inherent background noise of the data system**.' *(p23)*.

But as will be observed from the passages, Tessmer 2004 does not go on at all also, in specific terms:

- To describe or discuss the other factors
- To enumerate and compare the other factors that the Simple odds test responds to with the other factors that the Odds-ratio test responds to.

The omission is a serious omission.

In an observational study, such as Tessmer 2004, that is conducted under real-life conditions, the reader depends upon the author to validate his or her data by describing all of the conditions, apart from the experimental condition, that may have influenced it.

But Tessmer makes no attempt at all to validate his data in this way.

9. 'And third, Tessmer passes over without discussion or mention the fact that:
- By virtue of its formulation, the 'test' of the Odds-ratio method affords a more reliable measure of the effect of daytime running lights than the test of the Simple odds method.'

See *Section 2, 'The three tests'*, above!

There are three reasons why Tessmer may be supposed to know that the Odds-ratio test affords a more reliable measure of the effect of daytime running lights than the Simple odds test:

1) Tessmer is a statistician.

   Tessmer must therefore know that, by virtue of its formulation, the Odds-ratio test has the advantage over the Simple odds test that it will not respond to changes in any other factor besides daytime running lights that influences the proportion of daytime to nighttime accidents, or multi-vehicle to single-vehicle accidents.

2) Tessmer digests the findings of the study, Andersson et al 1976, in his paper *(p3)*.

   Tessmer should therefore have read Andersson et al's explanation of the formulation of the Odds-ratio test:

   > 'In this way one eliminates the effect of measures exerting general influence on ... the relative incidence of single and multiple accidents ... and on the relative incidence of accidents during daylight and during the hours of darkness ... .' [pxix].

3) Elvik et al 2003 model the comparative effect upon the Simple odds test, and the Odds-ratio test, of hypothetical changes in the incidence of accidents caused by other factors (Elvik et al 2003: Table 7 [p78]).

   Elvik et al demonstrate not only the existence of the distinctive response of the Simple odds test to changes in any other factor besides daytime running lights that influences the proportion of daytime to nighttime accidents, or multi-vehicle to single-vehicle accidents, but also the large potential size of the response[5].

Tessmer is equally capable of conducting the same modelling exercise.

The result of the finding from the Simple odds test is the basis of Tessmer 2004's conclusions in favour of daytime running lights.

It is therefore critical that Tessmer justifies his decision to prefer the finding from the Simple odds test over the finding from the Odds ratio test.

Nevertheless Tessmer passes over without discussion or mention the particular advantages that the Odds-ratio test holds over the Simple odds test as a test for the effect of daytime running lights.

10. 'Instead he prefers the finding from the test of the Simple odds method to the finding from the test of the Odds-ratio method because:
    - By virtue of its formulation, the Simple odds test is more sensitive to the effect of daytime running lights than the Odds-ratio test
    - The standard error of the Simple odds test is much smaller than the standard error of the Odds-ratio test
    - Only the finding from the Simple odds test is statistically significant.'

Tessmer 2004 justifies his preference for the finding from the Simple odds test over the finding from the Odds-ratio test in the following passages from his paper *(Present author's emphases)*:

> 'Like the simple odds, the odds ratio attempts to control for a variety of factors other than the presence or absence of DRLs. The estimated effectiveness of DRLs based on this technique is extremely sensitive to small changes encountered in real world crash data. As a result, **reductions in target crashes during the daytime using the odds ratio technique may not be detected over the inherent background noise of the data system**.' *(p1)*

> 'The **standard error of the odds ratio is much larger than the standard error of the simple odds**. To be statistically precise, when using the simple odds, the null hypothesis can be marginally rejected, however, the power of the odds ratio is not sufficient to reject the null hypothesis. Therefore the analysis in the main body of this report was based solely on the simple odds.' *(p8)*

> 'The odds ratio is easier to understand for inexperienced analysts than the simple odds and, like the simple odds, attempts to control for a variety of factors other than the presence or absence of DRLs. Unfortunately, when using the odds ratio, the estimated effectiveness of DRLs is extremely sensitive to small changes encountered in real world crash data and **none of the results were statistically significant**. This does not mean that DRLs do not reduce target crashes during the daytime. It just means that the odds ratio technique does not detect these changes over the inherent background noise of the data system.' *(p23).*

The present author does not dispute that, by virtue of its formulation, the Simple odds test is more sensitive to the effect of daytime running lights—or as Tessmer 2004 would seem to make the same point, has a much smaller standard error—than the Odds-ratio test.

However it is metaphysical of Tessmer to propose that the finding from one experimental test can be disregarded in favour of the finding of another experimental test simply because, by default of the collection or availability of sufficient experimental data, the finding of the first experimental test happens not to be statistically significant.

Rather scientifically, if the finding of the first experimental test is relevant to the conclusions of the experiment—and if, as discussed in the *following Section*, statistical significance falls to be treated as a necessary condition of the admissibility of any findings of the experiment—, the experiment remains incomplete pending the collection or availability of sufficient additional data to render the finding statistically significant.

11. 'Tessmer thereby avoids that, in scientific terms, it is not sufficient for him to detect a reduction of accidents from daytime running lights by the 'more sensitive' Simple odds method; he must also go on to confirm the reduction of accidents in question by the 'more reliable' Odds-ratio method.'

The proposition is semantically and scientifically self-evident.

One may **detect** the **apparent** existence of an effect by a **sensitive** test; but one thereafter **confirms** the **real** existence of the effect by a **reliable** test.

Nevertheless to cite out of the epidemiological literature solely in further support the argument of Phillips & Goodman 2004, statistical significance is not the only measure of the reliability of a finding.

Rather Tessmer 2004's data is extensive — in Tables 2–5 *(pp12–14)* he reports total sample sizes of vehicles involved in crashes to be:

[5] It should be borne in mind, when reading Elvik et al 2003, that Elvik et al confusingly refer to what Tessmer 2004 and most other authors call the 'simple odds' test as the 'odds ratio' test, and the 'odds ratio' test as the 'ratio of odds ratios' test.

Further, to make confusion worse confounded, Elvik et al sometimes fail to follow their own terminology — eg in Table 6 [p76] they 'revert' to labelling column 6 of the table 'Odds ratio' instead of 'Ratio of odds ratios'

Table 2: DRL-Equipped Vehicles in Target and Single-Vehicle Fatal Crashes, FARS 1995–2001

11,079 vehicles

Table 3: Vehicles w/o DRL in Target and Single-Vehicle Fatal Crashes, FARS 1995–2001

33,620 vehicles

Table 4: DRL-Equipped Vehicles in Target and Single-Vehicle Non-Fatal Crashes, NASS/GES 1995–2001

1,652,000 vehicles

Table 5: Vehicles w/o DRL in Target and Single-Vehicle Non-Fatal Crashes, NASS/GES 1995–2001

5,115,000 vehicles .

And an adverse finding from data so extensive, even though the finding may not be statistically significant, must either be accepted or upset.

12. ‘For the layman, Tessmer 2004's mixed findings from the Simple odds and Odds-ratio methods render the findings of his study in favour of daytime running lights inconclusive.’

Does not call for expansion.

13. ‘For the scientist, the inherent bias of Tessmer 2004's method; the incompleteness of his presentation; and the unsatisfactory nature of his argumentation, likewise render the conclusion of his study in favour of daytime running lights invalid and worthless.’

Does not call for expansion.

C. Further commentary: Findings

14. Tessmer 2004 does not compare the accidents of two populations of drivers who differ solely, as to characteristics that may evoke responses from the Simple odds test (or Odds-ratio test), in that:
    - One population drives vehicles that use daytime running lights
    - The other population drives vehicles that do not use daytime running lights.

    Rather, on scrutiny of Tessmer's data, the population that drives vehicles that use daytime running lights turns out, for some reason that Tessmer fails to investigate, also to manifest a higher ratio of nighttime single-vehicle accidents : nighttime multi-vehicle accidents than the population that drives vehicles that do not use daytime running lights. Tessmer's findings are therefore, not only mixed findings, but also confused findings

Calculation from the data supplied in Tessmer 2004: Tables 2–5 *(pp12–14)* yields the following figures of the breakdown of non-daytime 'target' crashes[6] — ie accidents that either because they took place at nighttime, or because they only involved one vehicle, will not have been affected by the use or non-use of daytime running lights:

Table 2: DRL-Equipped Vehicles in Target and Single-Vehicle Fatal Crashes, FARS 1995–2001

Daytime single-vehicle crashes (dsva)	37.41%
Nighttime single-vehicle crashes (nsva)	50.92%
Nighttime target crashes (nmva)	11.66%
All crashes (dsva + nsva + nmva)	100.00%

Table 3: Vehicles w/o DRL in Target and Single-Vehicle Fatal Crashes, FARS 1995–2001

Daytime single-vehicle crashes (dsva)	37.36%
Nighttime single-vehicle crashes (nsva)	49.82%
Nighttime target crashes (nmva)	12.82%
All crashes (dsva + nsva + nmva)	100.00%

Table 4: DRL-Equipped Vehicles in Target and Single-Vehicle Non-Fatal Crashes, NASS/GES 1995–2001

Daytime single-vehicle crashes (dsva)	36.52%
Nighttime single-vehicle crashes (nsva)	31.81%
Nighttime target crashes (nmva)	31.66%
All crashes (dsva + nsva + nmva)	100.00%

[6] In Tessmer 2004's study the term 'target' accidents comprehends the sub-set of multi-vehicle accidents that, by virtue of their configuration, might be predicted in daytime to be reduced by daytime running lights

Table 5: Vehicles w/o DRL in Target and Single-Vehicle Non-Fatal Crashes, NASS/GES 1995–2001

Daytime single-vehicle crashes (dsva)	36.13%
Nighttime single-vehicle crashes (nsva)	29.80%
Nighttime target crashes (nmva)	34.07%
All crashes (dsva + nsva + nmva)	100.00% .

Suppose that the population of the drivers of DRL-equipped vehicles were the same in all respects, apart from the use of a vehicle with daytime running lights, as the population of the drivers of Vehicles w/o DRL.

Then one would expect that, for both Fatal crashes and Non-fatal crashes, for DRL-equipped vehicles and Vehicles w/o DRL the percentage figures of Daytime single-vehicle crashes (dsva%), Nighttime single-vehicle crashes (nsva%), and Nighttime target crashes (nmva%) would be the same.

Contrary to this expectation, however, as will be observed, for both Fatal crashes and Non-fatal crashes the ratio of the percentage figure of Nighttime single-vehicle crashes : Nighttime target crashes (nsva% : nmva%) is markedly higher for DRL-equipped vehicles than for Vehicles w/o DRL.

To calculate exact figures, for Fatal crashes, the ratio of the percentage figure of Nighttime single-vehicle crashes: Nighttime target crashes is 12.3% higher for DRL-equipped vehicles, or for Non-fatal crashes, 14.8% higher.

To refer back to *Section 1, 'The three tests'*, both the Simple odds test, and the Odds-ratio test, will respond not just to the predicted effect of daytime running lights to reduce Daytime multi-vehicle accidents, but also the effect of any other factor to reduce or increase Daytime single-vehicle accidents, Nighttime single-vehicle accidents or Nighttime multi-vehicle accidents.

So in Tessmer 2004's study, the population of the drivers of vehicles that use daytime running lights, and the population of the drivers of vehicles that do not use daytime running lights:

- Differ in a respect that might potentially vitiate Tessmer's findings in favour of daytime running lights from the Simple odds test (or equally his findings against daytime running lights from the Odds-ratio test)
- Differ in the respect to an important degree.

Tessmer 2004 does not mention or discuss the breakdown of non-daytime target crashes that is presented above.

Tessmer does not attempt to validate his findings in the light of the different ratio of Nighttime single-vehicle crashes : Nighttime target crashes for vehicles that use daytime running lights, and vehicles that do not use daytime running lights, that the breakdown reveals.

He does not follow up the different ratio of Nighttime single-vehicle crashes : Nighttime target crashes by attempting to identify the corresponding characteristic, or characteristics, in which the population of drivers of vehicles that use daytime running lights, and the population of drivers of vehicles that do not use daytime running lights, differ .

The omissions in question render Tessmer 2004's findings potentially not, as they ostensibly purport to be, a measure of the effect of daytime running lights, but a measure of the confused effect of:

- Daytime running lights
- The different characteristics of the drivers of vehicles that use daytime running lights, and the drivers of vehicles that do not use daytime running lights, in his study.

Tessmer 2004's findings are therefore, even on the face of them, not only, as treated under *Section 5*, unreliable because they are mixed findings, but also unreliable because they are confused findings.

D. Further commentary: Conduct

15. Tessmer 2004 does not attempt to establish the margin of error of the Simple odds test (or the Odds-ratio test) under the conditions that applied during his study period

Statistically it is not possible reliably to detect, let alone measure the size of, an effect until one has established the margin of error of the test that one employs for the purpose.

And given the existence of multiple other factors besides daytime running lights that potentially may have influenced Tessmer 2004's findings from the Simple odds test (or the Odds-ratio test), the findings are subject to multiple potential sources of error.

But just as, as treated under *Section 8*, Tessmer mentions the existence of other factors besides daytime running lights that the Simple odds test, and the Odds-ratio, method will respond to, but then fails to describe, discuss or compare them, so too he fails:

- To attempt to identify such of the other factors as may have prevailed in the United States during his 1995–2001 study period
- To attempt to establish the margin of error that the presence of the other factors in question may have introduced to the Simple odds test (or the Odds-ratio test).

It might be possible nevertheless to gain a suggestive impression of the size of the margin of error by calculating the ‘normal year variation’ for 1995–2001 in the values of the Simple odds test, and the Odds-ratio, test for the accidents of Tessmer's sample.

But Tessmer 2004 chooses to publish only overall total, not year-by-year, figures of his data.

16. Tessmer 2004 does not discuss—or attempt to discount by his study-design—the full set of potential adverse side-effects of daytime running lights to increase, as well as reduce, accidents between motorcars

The mechanism of daytime running lights is not that they should assist drivers and other road users to detect vehicles in the central field of human vision.

Human central vision can detect objects such as a motorcar without difficulty .

Thus Hörberg & Rumar 1975 reported incidentally in passing that experimental subjects were able to detect a yellow Volvo motorcar on the taxi runway of a military airfield, against a background of the sky and the runway in daytime, at distances of more than 3000m from them, even when the Volvo did not display any lights.

The mechanism of daytime running lights is rather that bright light is one of the stimuli—bright light, strong contrast, a looming large object, or fast movement—that reflexly attracts the attention of a human observer in peripheral vision.

For instance Hörberg & Rumar 1975 evolved their specification of daytime running lights by testing how effective lights of different type and intensity were in attracting the attention of experimental subjects at a peripheral angle of vision of 30°.

Because in this way daytime running lights ‘actively’ and reflexly attract the attention of a driver or other road user to a vehicle that is using them, rather than merely ‘passively’ assisting the driver to see the vehicle, they have potentially three important side-effects:

- A first ‘distraction’ effect, whereby a driver may overlook a vehicle (or pedal cycle or pedestrian) because he is looking away from the vehicle in central vision, or at a small peripheral angle of vision, in the direction of a vehicle that is using daytime running lights
- A second ‘distraction’ effect, whereby a driver may overlook a vehicle (or pedal cycle or pedestrian) because he is looking away from the vehicle at up to 30° peripheral angle of vision in the direction of a vehicle that is using daytime running lights
- A ‘masking’ effect, whereby a driver may overlook a vehicle (or pedal cycle or pedestrian) because he views the other vehicle in line with, and so against the background of, a following vehicle that is using daytime running lights.

The character and development of the three side-effects with the increasing use of daytime running lights in a country, like the United States, where all motorcycles already use daytime running lights, is that:

- The first ‘distraction’ effect is a **disadvantage** of daytime running lights that will **disappear for motorcars and motorcycles—but not pedestrians and pedal cycles**—at a figure of 100% daytime running light usage
- The second ‘distraction’ effect is a **disadvantage** of daytime running lights that will **persist** at a figure of 100% daytime running light usage
- The ‘masking’ effect is a **disadvantage** of daytime running lights that will **disappear for motorcars—but not pedestrians and pedal cycles , or motorcycles**—at a figure of 100% daytime running light usage.

The explanation why motorcycles, in spite of using daytime running lights themselves, may nevertheless be masked by the daytime running lights of a following motorcar, is that, if another driver (or other road user) views the motorcycle in line with one of the lights of the following motorcar, the driver may well wrongly confuse the light of the motorcycle with the light of the motorcar.

If so the driver may wholly fail to observe the presence of the motorcycle.

To illustrate the bias that the three side-effects, should Tessmer 2004 fail to discount them, will introduce into Tessmer's ‘Side-by-side’ comparison study-design:

- Let us take as example, first, a Side-by-side comparison study that is conducted against a general background of under 50%—let us say 40%—daytime running light usage
- Let us simplify by considering only accidents between motorcars.

Since the vehicle whose daytime running lights cause a distraction or masking accident will not usually itself be involved in the accident, but instead two vehicles drawn from the general population of vehicles, a figure of some 60% of distraction accidents will be attributed to vehicles not using daytime running lights, and some 40% to vehicles using daytime running lights.

Given that for motorcars the first ‘distraction’ effect, and the ‘masking’ effect, will disappear at a figure of 100% daytime running light usage, but the second ‘distraction’ effect will persist, the result of these attributions will be to create:

- As to the first ‘distraction’ effect, and the ‘masking’ effect, a **bias in favour of daytime running lights** (attributions running at 60%:40% against vehicles not using daytime running lights, instead of a ‘neutral’ 50%:50%)
- As to the second ‘distraction’ effect, a **bias in favour of daytime running lights** (60% of wrong attributions against vehicles not using daytime running lights).

To continue:

- Let us take as example, second, a Side-by-side comparison study that is conducted against a general background of over 50%—let us say 60%—daytime running light usage.

The result will instead be:

- As to the first ‘distraction’ effect, and the ‘masking’ effect, a **bias against daytime running lights** (attributions running at 60%:40% in favour of vehicles not using daytime running lights, instead of a ‘neutral’ 50%:50%)
- As to the second ‘distraction’ effect, a **bias in favour of daytime running lights** (40% of wrong attributions against vehicles not using daytime running lights).

In summary the examples suggest that, unless Tessmer 2004 conducted his study against the background of a high figure of general daytime running light usage in the United States, as to accidents between motorcars the two distraction effects and the masking effect may potentially have operated between them to bias Tessmer's findings in favour of daytime running lights.

Tessmer 2004's failure to discuss or allow for the potential adverse side-effects of daytime running lights to cause distraction or masking accidents therefore vitiates his findings as to accidents between motorcars.

17. Tessmer 2004 does not discuss—or attempt to discount by his study-design—the full set of potential adverse side-effects of daytime running lights to increase, as well as reduce, motorcycle, pedal cycle or pedestrian accidents

To repeat from the *previous Section*, the character and development of the three side-effects of daytime running lights that were described in the section is that, with the increasing use of daytime running lights by motorcars:

- The first ‘distraction’ effect is a **disadvantage** of daytime running lights that will **disappear for motorcars and motorcycles—but not pedestrians and pedal cycles**—at a figure of 100% daytime running light usage
- The second ‘distraction’ effect is a **disadvantage** of daytime running lights that will **persist** at a figure of 100% daytime running light usage
- The ‘masking’ effect is a **disadvantage** of daytime running lights that will **disappear for motorcars—but not pedestrians and pedal cycles, or motorcycles**—at a figure of 100% daytime running light usage.

Accordingly, Tessmer 2004's failure to discuss or allow for the potential adverse side-effects of daytime running lights to cause distraction or masking accidents vitiates Tessmer's findings as to motorcycle, pedal cycle or pedestrian accidents even more completely than, as stated in the *previous Section*, it vitiates his findings as to accidents between motorcars.

E. Further commentary: Presentation

18. Tessmer 2004 fails to define 'target' non-fatal 'crashes' — then confusingly sometimes refers to them as 'opposite direction/angle', and sometimes as 'opposite direction', crashes

The references to 'opposite direction/angle' crashes appear in Tessmer 2004's Abstract *(pi)*:

> 'DRLs reduced **opposite direction/angle** daytime non-fatal crashes by 5 percent.'

and Executive Summary *(p1)*:

> 'DRLs reduced **opposite direction/angle** daytime non-fatal crashes by 5 percent.'

> 'DRLs reduced **opposite direction/angle** daytime non-fatal crashes by −7.9 percent that is DRLs increase **opposite direction/angle** daytime non-fatal crashes by 7.9 percent.'

In fact, on a whole reading of Tessmer 2004, it would seem that, by 'target' non-fatal crashes—in like fashion to target fatal crashes—, Tessmer means solely 'opposite direction' crashes.

F. Further commentary: Scope

19. Tessmer 2004 restricts himself to the study of the effect of daytime running lights upon 'opposite direction' accidents — whereas the intended purpose of daytime running lights is not to prevent:

- Accidents with a vehicle that is viewed—as in most 'opposite direction' accidents—in central vision,

but:

- Accidents at intersections (or pedestrian accidents) in which a driver, motorcyclist, pedal cyclist or pedestrian fails to observe another vehicle at up to 30° peripheral angle.

So, in contradiction of the title *'An Assessment of the Crash-Reducing Effectiveness of Passenger Vehicle Daytime Running Lamps (DRLs)'*, Tessmer's study does not represent a comprehensive, or overall, study of the effect of daytime running lights — rather it represents merely an ancillary study of one particular incidental effect of daytime running lights

The daytime running lights prediction

Daytime running lights are proposed as a remedy for the category of accidents—principally accidents in which a driver infringes the right-of-way of another vehicle at an intersection—that drivers explain by saying that 'they did not see the other vehicle'.

Thus, to repeat from the treatment of potential adverse side-effects from daytime running lights in *Section 16*:

> The mechanism of daytime running lights is not that they should assist drivers and other road users to detect vehicles in the central field of human vision
>
> Human central vision can detect objects such as a motorcar without difficulty.
>
> Thus Hörberg & Rumar 1975 reported incidentally in passing that experimental subjects were able to detect a yellow Volvo motorcar on the taxi runway of a military airfield, against a background of the sky and the runway in daytime, at distances of more than 3000m from them, even when the Volvo did not display any lights.
>
> The mechanism of daytime running lights is rather that bright light is one of the stimuli—bright light, strong contrast, a looming large object, or fast movement—that reflexly attracts the attention of a human observer in peripheral vision.
>
> For instance Hörberg & Rumar 1975 evolved their specification of daytime running lights by testing how effective lights of different type and intensity were in attracting the attention of experimental subjects at a peripheral angle of vision of 30°.

And it is particularly at such locations as intersections, roundabouts, or private driveways that drivers are called upon to observe other vehicles that they may view, not in central vision, but at up to 30° peripheral vision.

Correspondingly the first objective of a study of the effectiveness of daytime running lights, such as Tessmer 2004, is to confirm the prediction that daytime running lights will reduce the number of:

- Accidents between a motorcar on the major road at an intersection and:
  - A vehicle turning off the major road into the minor road
  - A vehicle turning onto the major road from the minor road
  - A vehicle crossing the major road from the minor road;
- Accidents between a motorcar and a pedestrian who is crossing the road.

In Tessmer's terms—or rather in the descriptive terms of the configuration of accidents that are adopted by the FARS and NASS/GES databases—these accidents will in many instances represent 'angle' crashes.

It will only be the second objective of the study to investigate also the beneficial or adverse side-effect of daytime running lights upon:

- Head-on collisions (eg overtaking collisions),

and:

- The residue of accidents—besides accidents at intersections; pedestrian accidents; and head-on collisions—where daytime running lights will also usually be viewed in central vision, or up to 30° peripheral vision.

In Tessmer's terms, these accidents will for the most part represent 'opposite direction' crashes.

Restriction of scope of Tessmer 2004's investigation

Nevertheless Tessmer 2004 at the same time:

- Purports, by his choice of Title, *'An Assessment of the Crash-Reducing Effectiveness of Passenger Vehicle Daytime Running Lamps (DRLs)'*, to conduct a study in pursuit of both first and second objectives, namely a study of the effectiveness of daytime running lights to reduce both 'angle' and 'opposite direction' crashes
- Actually conducts a study that is restricted to the pursuit of the second objective, namely a study of the effectiveness of daytime running lights solely to reduce 'opposite direction' crashes.

Reason for restriction of scope

The reason that Tessmer 2004 gives for restricting the scope of his study to treating only 'opposite direction' accidents in this way is *(p10)*:

> 'Neither the FARS nor the NASS/GES databases have a variable that partitions the data exactly into target and comparison crashes. Both data sets have variables, which permit one to approximate the desired partition. Therefore, it is possible that the partition of target crashes and comparison crashes may not be perfect. For example, the geometry of an angle crash might prevent a driver from seeing the DRLs of the other vehicle. If angle crashes that cannot be affected by DRLs are included in the set of target crashes, the estimated effect of DRLs, using FARS may be underestimated. Since the effectiveness is expected to be small, fatal target crashes have been limited to head-on crashes and sideswipe opposite direction crashes. ... .'

The reason does not stand up to scrutiny.

As has been said, the main objective of daytime running lights is to reduce accidents at intersections. And the predominant configuration of accidents at intersections is precisely 'angle' crashes.

Therefore by haplessly excluding 'angle' crashes at intersections because in some of them:

> 'the geometry ... might prevent a driver from seeing the DRLs of the other vehicle',

Tessmer 2004, in lay terms, 'throws the baby out with the bathwater'.

Presentation of restriction of scope

To treat however the restriction of the scope of Tessmer 2004's study as he chooses to present it, first, in the Documentation Sheet, Tessmer (1), by repeating the Title, incorrectly presents the scope of the study to comprehend a substantive investigation of the effect of daytime running lights to reduce accidents:

> '**Title and Subtitle**
>
> An Assessment of the Crash-Reducing Effectiveness of Passenger Vehicle Daytime Running Lamps (DRLs) *(pi)*;

but then (2) correctly presents the scope to comprehend merely an investigation of the ancillary effect of daytime running lights to reduce 'opposite direction' crashes:

> '**Abstract**
>
> This study estimates the effectiveness of passenger vehicle daytime running lights in reducing two-vehicle opposite direction crashes ... .' *(ibid)*.

In the Executive Summary, Tessmer abandons the dichotomy of contradictory statements, and states consistently:

> '**Executive Summary**
>
> This study estimates the effectiveness of passenger vehicle daytime running lights in reducing two-vehicle opposite direction crashes ... .' *(p1)*
>
> 'Simple Odds Results:
>
> - DRLs reduced opposite direction daytime fatal crashes by 5 percent.
> - ... *(ibid)*

In the Literature Review, Tessmer, actually explains the purpose of daytime running lights in similar terms to the present author— but at the same time reverts to presenting the scope of his study, incorrectly, to comprehend a substantive investigation of the effect of daytime running lights:

> '**Background**
>
> This is the second NHTSA study on the effectiveness of Daytime Running Lamps (DRLs). The preliminary study was published in June 2000 and is the basis of this research.
>
> Many traffic crashes are the result of the failure of a driver to notice another vehicle. Visual contrast is an essential characteristic that enables a driver to detect vehicles. The purpose of daytime running lamps (DRLs) is to increase the drivers' ability to detect DRL-equipped vehicles, particularly in the peripheral visual field, by increasing visual contrast. ...' *(p3)*.

In the Findings, the dichotomy between the presentation of the title, or heading, and the presentation of the text, returns:

> '**DRL Effectiveness in Fatal Two-Vehicle Crashes - Results**
>
> The effectiveness, based on the simple odds, of DRLs in preventing two-vehicle opposite direction fatal crashes during daylight is estimated to be 5.3 percent with (p = 0.052).' *(p13)*.

Finally, in the Conclusions, the dichotomy continues, but takes the form of the conflicting presentation of the first two sentences of the Conclusions:

> '**Conclusions**
>
> 'The effectiveness of daytime running lamps, based on the simple odds, was analyzed in the preceding sections using data from FARS and NASS/GES from calendar years 1995 to 2001. FARS and NASS/GES data show that during the period of the study 1995 to 2001, DRLs reduced daylight two passenger vehicle opposite-direction crashes by about 5 percent. ...' *(p17)*.

Misleading suggestion of Conclusions

The successive presentation in the Conclusions of the sentences:

> 'The effectiveness of daytime running lamps, based on the simple odds, was analyzed in the preceding sections ... .'

and:

> '[The] data show that ... DRLs reduced daylight two passenger vehicle opposite-direction crashes ... .'

is particularly wrong.

The presentation is calculated to suggest to the layman that a reduction of 'daylight two passenger vehicle opposite-direction crashes' by 'DRLs' demonstrates the 'effectiveness of daytime running lamps'.

Whereas in truth, a reduction just of 'opposite direction' crashes does no such thing.

The suggestion is therefore totally misleading.

Stephen Prower
Research officer
British Motorcyclists Federation
8 February 2005

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